Interactive comment on "Temporal and spatial variability of Icelandic dust emission and atmospheric transport" by Christine D. Groot Zwaaftink et al.

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The paper presents a modelling study of the emission and transport of dust in Iceland between 1990 and 2016. It highlights the significance of high latitude dust sources on the global dust budget, and the authors present interesting results showing the main transport pathways of dust from Iceland. However, I believe the description of the model set-up needs to be significantly improved before this paper can be published. Details, including a description of the resolution of the model topography used and the particle size distribution applied are missing, and there needs to be some discussion on how their results may be sensitive to their set-up. The manuscript would also be improved by including some discussion on how the supply of new dust sources, related to volcanic eruptions in Iceland, might influence their results.
Authors; Thank you for your constructive review.

1. The Introduction
I can see the importance and relevance of this study but I don't think this is reflected in the introduction. Details are missing and statements are often not backed up with existing data and/or references are missing. Currently, it reads as a series of statements rather than explaining to the reader why the study is important, the approach, and how it fits in with the existing literature. You need to discuss in more detail the work that has previously been carried out to better understand dust emissions in Iceland, including work published by Olafur Arnalds and Pavla Dagsson-Waldhauserova, and you should consider work on dust events in other parts of the world too. Further discussion on modelling dust emissions is also needed. You state that model simulations of dust emissions in Iceland are lacking but there is now a body of work on modelling remobilisation of volcanic ash in Iceland, see Leadbetter et al. (2012), Liu et al. (2014), Beckett et al. (2017), and further afield, for example Folch et al. (2013) and Mingari et al. (2017) who consider remobilization in Argentina. Given that volcanic ash is a significant source of PM in Iceland (indeed there is the question of what is dust and what is ash!!), and the modelling approaches for remobilized ash are very similar to the approach you have applied here you should discuss this.
Authors; Indeed the modelling efforts considering remobilisation of volcanic ash are relevant and are now included in the introduction. We also added more details on current knowledge of Icelandic dust, although we refer to a recent review paper by Arnalds et al. (2016) for a complete overview.

Specific comments:
Line 3: You state that: 'Model simulations indicated that 0.3% of global dust emission may originate from Iceland (Groot Zwaaftink et al., 2016)'. More details are needed here, what model, what simulations were performed and with what aim? If this has already been done then where does the study you are about to present fit in?
Authors; These were global simulations over a three-years periods where spatial distribution of dust emission in Iceland was not discussed.
Changes; We give additional details on this reference.

Line 4: You state that 'it is known that dust storms frequently occur there [Iceland]' and cite Dagsson-Waldhauserova et al. (2014). It would be good to include some numbers here e.g. how many dusty days, on average, occur in Iceland. This will help put your results into context later on too. I realise you comment on this later in the paper but this should be here in the Introduction.
Authors; We added this information in the introduction.

Line 14: You need to provide a reference for the surface type map that you refer to.
Authors: Added.

2. Model Set Up
The explanation of your model set-up is missing many details. I think you should include the equations used in FLEXDUST to model the emission of dust, and explain the variables. Exactly how does your model set-up account for topography, snow cover and soil moisture? You state that precipitation halts mobilization. You need to refer to the work of Leadbetter et al. (2012) here who also considered how best to represent the impact of precipitation on mobilization of volcanic ash in Iceland. Please can you also comment on how well you think this approach is working in respect to representing the timing and frequency of dust events? This is discussed by Leadbetter et al. (2012) and Liu et al. (2014) who both point out that this approach does not account for wetting and drying of volcanic ash deposits, do you think this is true of all dust sources?
Authors; FLEXDUST equations have been given in Groot Zwaaftink et al. (2016) and we do not think this should be repeated here. We rather concentrated our presentation of FLEXDUST on the differences in the model set-up used in the present paper from the one used by Groot Zwaaftink et al. (2016), although we agree that a little more detail will be helpful for the reader. Indeed precipitation influences dust mobilization, as is accounted for in the model and we agree that more discussion on this topic is useful.
Changes: We added equation 1, which gives the dependency of dust emission on (threshold) friction velocity. We now refer to these studies on mobilization of volcanic ash in sections 1 and 2. We also added results of a test simulation where we included a drying period after precipitation that showed dust mobilization was not better represented near the source by inclusion of such a time lag. We also slightly extended the general description of FLEXDUST.

Please provide the Particle Size Distribution (PSD) you used and explain your reasoning for this choice. Why did you choose to consider particles with diameter up to 20 um only? What is the minimum particle size you considered? The work of Liu et al. (2014) gives the PSD of ash particles that had been remobilized and deposited in Reykjavik during March 2013. They found that particles had a mode at 32-63 um. Have there been any measurements of the PSD of particles mobilised from the other dust sources in Iceland?
Authors; We considered particles in the size range 0.2 to 20 µm, consistent with global dust simulations using FLEXDUST and FLEXPART (Groot Zwaaftink et al., 2016). The size distribution is provided in the given references. It is quite standard to consider only particle sizes less than20 µm in dust modelling (e.g. Tegen, 2003). Observations of size distributions in Icelandic dust storms show that particle mean diameter is much smaller than 10 µm (Dagsson-Waldhauserova et al., 2014b). Larger particles may be present close to the sources but their potential for atmospheric transport away from the source region is very limited, due to rapid gravitational settling. As our focus is on dust transport, we do not include such large particles in our simulations.
Changes: we added the minimum particle size to section 2 and discuss observed particle size distributions.

3. Thresholds Friction Velocities.
Please provide your reasoning for the threshold friction velocities that you apply. How were these values determined from the Arnalds et al. (2001) and Arnalds et al. (2016) papers and how are the classes defined? Please also provide information on how these classes are distributed across Iceland. Figure 1 shows the soil fractions applied but please also highlight where the Dust Hot Spots are and how the erosion classes are applied across the other source regions. Please can you also comment on how good a job you think these threshold friction velocities are doing. By applying this range of values are you doing a good job of representing the timing and frequency of events in your model output? How sensitive is your model output to the threshold friction velocity applied? Can you account for some of the mismatch between the observed and modelled PM10 and PM2.5 air concentrations if you vary the threshold friction velocity applied?

Authors; The erosion classes have been presented in several publications (Arnalds et al., 2001; 2014; 2016) and are therefore not repeated here. The dust hot spots are the regions with maximum soil fraction in Figure 1. The threshold friction velocity affects timing and frequency of dust events and the concentration during events, as is now also clear from equation 1. Some of the mismatches are likely related to threshold friction velocity, we expect mostly because we use a fixed threshold (besides precipitation and snow cover influence). The threshold friction velocities of several sources are probably changing over time, even during dust events the surface conditions are changing. Despite the strong simplifications we apply in our model, we are able to capture timing of several events.
Changes; We added equation 1, which shows dependency of dust emission on threshold friction velocity. We added an explanation on the thresholds for each erosion class and a discussion in section 2.1. We discuss the influence of threshold friction velocity on results in sections 3.1.1, 3.1.2 and 4.

4. Topography
What is the resolution of your model topography? Are your results sensitive this? You state in the introduction that dust events can be driven by katabatic winds; does your model topography allow you to capture these meteorological phenomena? Mingari et al. (2017) show how the topography in Argentina influences the local winds and in turn how that drives mobilization. I think you need to consider this. This information would help put in context your later comment in Section 3.1.1 that the model output may not be able to capture observed PM10 concentrations because of the resolution of the topography.
Authors; The topography resolution is the same as in the ECMWF wind fields, thus 0.2 degrees for the high-resolution simulation and 1 degree for the long-term simulations. Indeed we cannot capture all local winds and discuss this in our manuscript.
Changes: We now already introduce this potential problem in section 2. We also add a discussion on sensitivity to model resolution in section 3.1.1.

5. Sources
You compare your model output air concentrations to PM data from monitoring stations across Iceland collected during 2012. You state that: 'In this year no volcanic eruptions occurred that could strongly influence PM measurements' (Line 8, Section 2.3). I disagree. Do you really think that the ash deposits from the eruption of Grimsvotn only the year before and from Eyjafjallajökull in 2010 had all been removed and were no longer a significant source of PM? The study by Leadbetter et al. (2012) considers the remobilization of volcanic ash from the deposits resulting from the eruption of Eyjafjallajökull in 2010. They compared modelled air concentrations using the dispersion model NAME, which includes a resuspension scheme, to PM10 measurements across Iceland during September 2010 to February 2011. Their modelled concentrations agree well with the timing and location of observed peaks in the PM10 data from the monitoring stations, and here only the Eyjafjallajökull ash is defined as the source. I recognize that your study aims to consider the long-range trends of dust emission and transport from sources across Iceland, but I think you need to acknowledge the fact that volcanic eruptions result in significant new sources of unconsolidated deposits which can continue to be remobilized for years after an eruption. In Section 3.2.2 you go on to state that your modelled dust emission rates are an order of magnitude lower than previous estimates given by Arnalds et al. (2014), and you say this could be related to volcanic events. I would suggest that you could explore this further and consider that the deposits from the Grimsvotn and Eyjafjallajökull eruptions could be a significant source of PM in your study.
Authors; We mainly wanted to avoid influence from direct injection of volcanic ash into the atmosphere. Resuspension of deposited tephra should be included in FLEXDUST, so in principle does not constitute a problem. Iceland, generally, is highly dynamic and land cover changes in response to volcanic eruptions and as deposited tephra fields age. In 2012, there was no volcanic eruption in Iceland, but of course ash deposits from previous years may still be remobilized. In fact, the dust sources in our surface type map are partly covered with fresh tephra. Also ash from the Eyjafjallajökull and Grimsvotn eruptions were partly deposited on active dust sources that are included in our model, even though our land cover map does not account for any changes due to the recent eruptions. This means that we partly include resuspension of volcanic material. This should indeed be included as a discussion and we added this in sections 2 and 4.

6. The impact of NAO

I did not follow why you chose to consider the role of NAO as part of your study and what the significance is? What meteorological variables and/or synoptic conditions related to NAO do you think impact mobilization events in Iceland?

Authors; The winter Icelandic low is stronger during NAO positive phases according to model simulations (Bromwich et al., 2005) and this relates to precipitation, temperature and wind in Iceland. Stronger winds can enhance dust mobilization, while precipitation and snow cover can inhibit dust mobilization. We thus wanted to know if dust emission amounts are related to NAO. Furthermore, stronger winds over the North Atlantic can increase dust transport.

Changes: We changed the discussion on NAO in section 3.3.

Minor Comments

In several places, including in the abstract, you state your conclusion that: 'Annual dust emission amounts to 4.3_0.8 Tg during the 27 years of simulation'. I find the term 'amounts to' a little confusing when discussing the yearly average. Please clarify.

Changes; we rephrased where applicable.

Page 1, Line 3. Emission should read emissions.

Authors: rephrased

Page 1, Line 19. 'A model for estimates of dust emission', does not read very well. The structure of this sentence needs to be improved.

Changes; rephrased.

Page 1, Line 26. Please provide references for your examples on the impacts of dust.

Authors; we provide references later in the introduction.

Page 2, Line 14. '.....surface type map of Iceland to identify dust sources'. I think you need to cite Arnalds et al. (2016) here.

Changes; we added an appropriate reference.

Page 2, Line 21. I did not quite follow this sentence: ' ....and originally accounts for snow cover, topography....' . What do you mean by 'originally'?

Authors: It does in the global setup where the model was first introduced, but this differs in the Iceland version.

Changes; rephrased

Page 3, Line 3. 'As we here mainly deal with sediments'. What do you mean by this statement, what is the relevance of 'sediments' is this different to 'dust'. Please clarify. Also the structure of this sentence could be better, what do you mean by 'mainly deal with'?

Changes; Rephrased this section.

Page 3, Line 8. What do you mean by a 'closed snow cover'?

Authors; A snow cover that does not consist of snow patches but covers the area.

Page 3, Line 17. 'as was previously also done for'. Could read better, how about 'and has previously been used to model the transport of Saharan dust'.

Changes: rephrased

Page 3, Line 21. What do you mean by a 'multitude of particles'? Please be specific.

Changes: rephrased

Page 4, Lines 1 and 2. Here you write the units of the particle size (micrometre), in other places you use the symbol. Please correct. Also, the structure of this sentence could be improved.

Changes: rephrased

Page 4, Line 17. ‘Model evaluation is limited due to a lack of data.’ This sentence does not read well. Please improve the structure of this paragraph.
Changes: rephrased

Page 4, Line 19. Should read ‘.....concluding that THE modelled spatial distribution.....’.
Changes: rephrased

Page 5, Line 3. What are the problems with the sensors that you refer to?
Authors; there were different problems, but further details will not improve understanding of the results.

Page 5, Line 12. ‘....and are at larger distance from dust sources, and shorter distance to the ocean’, does not make sense. How about ‘...and are further away from the dust sources, and closer to the ocean.’
Changes; rephrased.

Page 5, Line 25. ‘rather too large in the model’. How about instead ‘.... are overestimated in the model output’.
Changes: rephrased

Page 6, Line 4. Please explain where Storhofdi is in order to put the rest of the discussion in Section 3.1.2 into context.
Changes: rephrased

Page 6, Section 3.1.2. I think you need to cite the work of Prospero et al. (2012) here.
Changes: rephrased

Page 6, Line 25. Here you refer to ‘sandy fields’ for the first time. What do you mean with this term? Is this the same as ‘sandy deserts’, as referred to in the Introduction. Please define these terms.
Changes: rephrased

Page 7, Line 1. Should read ‘during THE winter season’.
Changes: rephrased

Page 7, Line 12. Use of the word ‘particular’ is not right here.
Changes: rephrased

Page 7, Line 14. ‘Looking at total dust emissions from Iceland, 50% is emitted in 25 days, and 90% in 110 days of the year. Previous studies of long-term dust frequency reported 135 dust days per year (Dagsson-Waldhauserova et al., 2014).’ Please expand on this, what conclusions do you draw, do you consider this to be a significant discrepancy, if so why is there a difference?
Changes: we find this a good agreement and now comment on this in the manuscript.

Page 7, Line 26. You refer to emission rates presented by Arnalds et al. (2014). Please provide details as to how these emission rates were determined.
Changes; we added details in the introduction and rephrased this section.

Page 8, Line 12. ‘To understand where dust that is emitted from Iceland can be found in the atmosphere and on the ground’. This sentence is a little clumsy. Could you describe this as ‘to understand the transport of pathways of dust from Icleand..’?
Changes: rephrased

Page 8, Line 29. Typo, remove ‘8’.
Changes: removed

Page 9, Line 3. ‘Baddock et al. (2017) did study trajectories from either south or north Iceland and showed that dust from south Iceland....’. Please improve this sentence. I would suggest: ‘Baddock et al. (2017) studied the trajectories from sources in both the south and north of Iceland and showed that dust from south Iceland...’.
Changes: rephrased

Page 9, Line 10. Please clarify what you mean here. You state that: ‘A large fraction of emitted dust (<20 _m) does not travel far and is deposited in Iceland.’ Do you mean that you have found a large fraction of the emitted dust is on particles with diameter <20 um? But I thought you only considered particles up to this diameter? Perhaps you are just reconfirming that you have only considered this size range?
Authors; indeed we wanted to clarify that we only consider dust <20 um.
Changes; we removed (<20 _m)

Page 9, Line 26. ‘especially varying’ does not make sense. How about: ‘deposition varied significantly’. Also, are you referring to deposition rates or where particles were deposited?
Changes: rephrased

Page 10, Line 4. Please correct the sentence: ‘In this study we made model simulations’. Incorrect use of the word ‘made’.
Changes: rephrased

Page 10, Line 14. Please correct the sentence: ‘Best agreement with PM measurements over one year is found close to dust sources.’ It does not make sense.
Changes: rephrased

Page 10, Line 21. ‘At Storhofdi, near the south coast of Iceland, the timing of peaks in dust concentrations is very well captured in our simulations, as we determined based on a comparison of modelled and measured dust concentrations between 1997 and 2002’. The structure of this sentence needs to be improved. Something along the lines of: ‘.....the timing of the peaks in dust concentration in our simulations compared well with the observed peaks in measured dust concentrations between 1997 and 2002’.
Changes: rephrased

Page 10, Line 24. Please expand, which way does the dust from the north go?
Changes: rephrased

Page 10, Lines 25 and 26. The use of the term ‘much dust’, is repetitive and clumsy.
Changes: rephrased

Figures.
Figure 1. Please provide more details on how the soil fractions were determined, where does this data come from? How does soil fraction relate to ‘dust’ in this context? Is it possible to indicate where the ‘dust hotspots’ are. Please also improve the colour bar to indicate that 1.0 (?) is the maximum.
Authors; we assigned soil fractions to surface types, the dust hot spots are the locations with maximum bare soil fraction.
Changes; we changed the figure and add a comment in section 2.1

Figures 7 and 8. Please improve the labels on the colour bars. Figure 7b only has two!
And neither 7a, 7b or 8a indicate the maximum value. Also, in my version there are no labels for the individual figures (a and b).
Changes; changed