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

Anonymous Referee #2

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This compact paper, "Temporal and spatial variability of Icelandic dust emission and atmospheric transport" presents surface observations and results of lagrangian simulations of dust emission and deposition at high resolution for 2012 and lower resolution for 1990-2016 to estimate the dust emission and deposition to the region. The paper is very well written, presents interesting results from modeling and the observations, and references the appropriate literature. I believe that details are lacking in places and the analysis is a little weak, namely the comparison with observations and the certainty with which the dust emissions can be estimated, and would like to see those parts improved prior to publication.

General comments

The PM10 and simulated dust concentration yield similar mean (21 ug/m3 and 28 ug/m3, respectively) and standard deviations (pg 5 line 10); however, this is comparing dust-only concentrations from the model with bulk aerosol PM10. This suggests that the simulated dust concentrations are actually biased high (maybe up to a factor 2?) relative to the observations (if the non-dust component could be removed from the PM10). I think the way that the model results are compared to the PM10 (and PM2.5) may need reconsidering or the present method better justified. Can you estimate the non-dust component? How much of the PM10 at the sites may be localized dust that would not be captured by the model? This affects the attempt to estimate the annual emissions from Iceland and subsequent deposition. I’m not sure whether the current observational constraints and analysis are able to fully support the estimate. The agreement with the dust concentration measurements seems reasonable at StórhöfÃˇri, but this is only a single measurement site SW of the source regions. Therefore, the constraint on emissions transported in other directions is weak; it appears that equal, if not great, dust mass is deposited to the NE. Could the statistical relationship between observations and modeled dust concentration be used to better estimate the emission, or at least the uncertainty? For example, how much would the emissions need scaling to provide the same average dust concentration (or some other metric) at StórhöfÃˇri? this suffers from the lack of constraints for the dust in the NE, but might give a better representation of the dust emissions and their uncertainty beyond the interannual variability.

Authors: Thank you for your review. Indeed the constraints on dust emission in Iceland are weak. This results from the paucity of available data in Iceland and, to our knowledge, we have used all data that are available to compare our simulations with, even if most of the measurements (especially the PM measurements) do not allow direct comparisons. In the paper, we simply tried to use the long-term measurements that are currently available and also suggest that future more specific measurements would be needed to quantify the apparently important Icelandic dust sources. PM10 and PM2.5 values include other aerosols. This is of less concern at Raufarfell where traffic and sea salt influence are considered limited, but of larger concern in domestic and coastal areas as discussed in section 3.1.1. We therefore give more emphasis to the measurements at Storhofdi that only include dust. Close to the dust sources in NE Iceland we could confirm deposition rates based on snow sample observations (Wittmann et al., 2017). Other quantitative data is unfortunately not available and there are also no other supportive data that would allow a speciation of the PM data into different aerosol types. With the current data we can only conclude that timing of dust events can be captured and that dust deposition and concentrations, and therefore expectedly dust emission, are on the right order of magnitude. More precise estimates on these scales are currently not feasible, yet the model does provide an upper constraint.
Changes: we added and discuss references on different sources causing PM10 values exceeding health limits in Reykjavik and aerosol concentrations (other than dust) at Storhofdi in section 3.1.1.

Following from this, I can’t see any comparison of the low and high resolution runs in 2012 (other than 2.9 Tg and 5.1 Tg totals for 2012 on pg7, line 21). Does this mean that running at high resolution may give 75% higher emission estimates than the 4.3+/- 0.8 Tg presented for the long term estimates? I don’t

think the implications of this are discussed clearly enough. The uncertainty estimate for the interannual variability may mislead the reader to the certainty of the magnitude of the dust emissions (and hence deposition). Does the low resolution run well-reproduce the high resolution simulated dust concentration timing in 2012 otherwise? Maybe add the low resolution timeseries at StórhöfÃˇri to Figure 2?
Authors: In 2012 emission estimates were higher based on high resolution data. Deviations are also likely for other years. The measurements at Storhofdi are not available in 2012, instead we now discuss the high and low resolution runs in comparison to the PM10 measurements.
Changes: we provided additional results from the low-resolution simulation and extended the discussion on the influence of resolution in section 3.1.1.

While the time series of concentration provide a good visual reference of the frequency and magnitude of events, they are not ideal for illustrating the agreement between the simulation and the observations. I recommend providing a scatter-plot (perhaps on a log-log scale?) to better illustrate how well the model captures the observations of dust concentration. This is less useful (and therefore perhaps less necessary) for the comparison with PM for the reason outlined above, unless speciation is available.
Authors: We agree that this is useful for the dust concentrations, in combination with the time series already given.
Changes: We added such a figure for the Storhofdi data.

Emissions are not allowed when the precipitation rate is above the 1 mm/hr threshold. Is there a lag time for this emission suppression after the rain stops? Or is it expected that the timescale for the surface drying and becoming an active once again is shorter than the model timestep? How much do you think this assumption affects the emission?
Authors: In FLEXDUST, there is no time lag suppressing dust emission after rain. In a model test case where dust emission is prohibited if the precipitation sum in the past 4 hours exceeds 2 mm the model failed to simulate some strong dust events that were recorded in PM10 measurements. We therefore assume that during strong wind conditions the top sediment layer is quickly dried and dust emission is possible. This assumption is confirmed by several observations. Dagsson-Waldhauserova et al. (2014b) observed dust mobilization of wet particles, even during low-wind conditions. They discussed that the relatively dark basaltic dust might dry quickly. Also during intermittent snowfall dust mobilization has been recorded (Dagsson-Waldhauserova et al., 2015). Furthermore, analysis of long-term weather observations of dust events (e.g. Dagsson-Waldhauserova et al., 2014a) revealed that suspended dust is observed during precipitation events, although it is noted that precipitation at the weather observation location does not necessarily imply wet conditions at the dust source.
Changes: We added a model test for PM10 concentrations at Raufarfell and discuss the model results and references in section 3.1.

In Groot Zwaaftink et al. (2016) it is stated that, relative to a precipitation threshold, "Especially in northern latitudes, soil moisture appeared a better indicator of mobilization threshold as seasonal variations in surface dust concentrations at remote stations were better captured and total emission amounts were closer to other model estimates." Please can you comment on why this is different to the current research findings for Iceland.
Authors: The global simulations were based on a combination of size-dependent friction velocity thresholds (Shao and Lu, 2000) and increase of threshold friction velocity due to soil moisture according to Fécan et al. (1999). Instead, we here use friction velocities from field observations in Iceland. The combination of these observed thresholds with the soil moisture parameterization of Fécan et al. (1999) lead to very low dust emission rates and dust concentrations far below the measured values shown in section 3.1, as also discussed in our manuscript. Dagsson-Waldhauserova et al. (2014b) noted that the relatively dark basaltic dust of an Icelandic dust source dried quickly and dust mobilization occurred during moist conditions. It could thus be that Icelandic dust mobilization is less dependent on soil moisture and the soil moisture parameterization (Fécan et al., 1999) is not applicable. Furthermore, the ECMWF soil moisture data might not be representative for the layer from which dust is mobilized.
Changes; we extended the discussion on the soil moisture parameterization in section 2.1.

Specific Comments
pg 3 line 18 - "FLEPXART" typo
Authors: Corrected
pg 7 line 19 "FLEXUDST" typo
Authors: Corrected

It may be clearer to refer to the "soil fraction" as the "bare soil fraction" throughout.
Authors: Yes, that's better, we changed this.

Table 1 - it isn't quite clear how these values are derived from the threshold friction velocities presented in Arnalds et al. (2001) and the discussion in Arnalds et al. (2016). Please can you elaborate in the text on how these values are derived.
Authors: We added the explanation below in section 2.1.
Changes: We use observations from Arnalds et al. (2001) and a description of erosion levels (Arnalds et al., 2016) to determine the threshold friction velocity (see Table 1). While Leadbetter et al. (2012) and Liu et al. (2014) chose a fixed threshold friction velocity of 0.4 m s-1 for mobilization of volcanic ash, the range of values applied here is more suitable to cover the different conditions of multiple dust sources. Arnalds et al. (2016) give an overview of erosion classes for each surface type. For regions with extremely severe erosion we assume the average of threshold values observed at several sand fields, for severe erosion we assume average conditions of sandy gravel and for considerable erosion we apply an upper threshold observed for sandy gravel (Arnalds et al., 2001). So called dust hot spots, described by Arnalds et al. (2016), were also included in our simulations. These were assigned a lower friction velocity (see Table 1), corresponding to the lowest threshold wind velocity estimates for erosion by Arnalds et al. (2016), and a slightly larger bare soil fraction (+3%).

Figure 2 - The Raufarfell timeseries is hard to see because of the upper limit. Is it possible to use a discontinuity on the y-axis above _600 ug/m3 to better visualize the data at lower concentrations.
Changes: We changed the figure accordingly.