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Discussions

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

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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 StórhöfÃři, 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 StórhöfÃři? 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.

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 timingin 2012 otherwise? Maybe add the low resolution timeseries at StórhöfÃři to Figure 2?

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.

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?

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.

Specific Comments

pg 3 line 18 - "FLEPXART" typo

pg 7 line 19 "FLEXUDST" typo

It may be clearer to refer to the "soil fraction" as the "bare soil fraction" throughout.

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).

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Please can you elaborate in the text on how these values are derived.

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.

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