Review of Meinander et al. (2022): Newly identified climatically and environmentally significant high latitude dust sources.

This manuscript describes in detail the sources of high latitude dust (HLD) from both hemispheres and the properties of this dust source in relation to its impacts throughout the Earth system. The manuscript also includes a small section on Icelandic dust modelling. The manuscript sections dealing with sources and impacts are well written and comprehensive. This alone provides an update on Bullard et al. seminal paper on HLD and will be a useful reference for a wide variety of topics. However, the modelling sections are short and appear out of context with the rest of the paper. The modelling results come across as underdeveloped, offering little-to-no new insights that I can see beyond the general literature or model description papers. The paper is really quite long, which is perhaps understandable given the Authors effort to document all HLD. Given that I can see no additional benefit from the inclusion of Icelandic modelling I suggest it is removed in entirety. This then leaves a much more concise paper on the sources and impacts. Once the modelling section is dealt with, I would recommend publication in ACP.

Major comment:

The two short sections on modelling Iceland dust currently do not sufficiently add enough to warrant their inclusion in what is already a long manuscript. Especially as the title indicates that this will be a paper on newly identified HLD sources and Iceland is a single location in the Northern Hemisphere which has been recognized for a while as a potentially important dust source.

The bulk of the manuscript does not read like it should include model descriptions. The majority of the DREAM section (first paragraph) is given over to model description, not results. Such text unfortunately does not aid the reader in understanding the sources or impacts of HLD and instead provides a distraction. Such text I imagine will covered, correctly, already in the Authors submitted paper: “Fully dynamic numerical prediction model for dispersion of Icelandic mineral dust”. Similarly, the majority (first paragraph) of the SILAM model ‘results’ are instead model description.

Despite these two model sections being framed as results, I can see no new results presented here of significance, or that are not within the general literature already. E.g., Icelandic transport of aerosol or that a coarser resolution model will have difficulty capturing sub-gridscale level processes/emissions. Specifically:

(1) Figure 13 is for a single day. I feel that such a snapshot will offer the reader little insight about the general source strength or dispersion of HLD aerosol needed to make for a deeper understanding of HLD impacts. It is impossible to see the WORLDVIEW AOD in panel B and no scale is given. Furthermore, there a myriad of issues with correlating a single snapshot of total AOD with modelled dust aerosol loading – especially in a region known for high cloud clover and sea spray. There are co-authors who are experts in these issues and a thorough examination of bias is needed to provided confidence that this model is doing a ‘good job’ and a much longer comparison for relevance to the rest of the manuscript. This would require a significant undertaking.
(2) There is discussion of how well the SILAM model correlates to observations, yet these details are not provided anywhere, or any references given. I am therefore left not knowing how well the model does. The emission maps are better than for the DREAM model as they do provide some indication of seasonality – but again this is not a paper on Icelandic

dust sources but a paper on all HLD sources, especially new ones. A full emission map of all HLD sources would have been helpful.

Despite the HLD impacts section giving many great details on how HLD impacts clouds, chemistry, the cryosphere, and the marine biosphere, no modelling of these impacts are given using these models. This is what I would have expected the modelling results sections to be used for, but in their absence, I can see no need to have a section detailing Icelandic modelling.

Given the significant amount of work that would be needed I suggest that the modelling sections are simply removed. Particular points that the Authors feel are required to remain can be easily included in other sections. This creates a much more concise paper.

I also would like to state that modelling of HLD is a new and growing scientific avenue. And an exciting one. If the Authors are inclined, then what is provided here does provide the foundation of a new paper dedicated to this topic.

General comments:

High Latitude Dust locations: previously the definition of HLD has been chosen, somewhat arbitrarily, on a defined latitude (rounded) rather than a definition based on the properties of the dust and/or its emission environment. This process has been repeated here but looking at Figure 2 there appears to me to be a very clear demarcation of two dust belts. The lower latitude belt which ends around 58N in Eurasia and 55N in Canada and the higher latitude belt which begins around 60N in Siberia and 58N in Canada. With the exception of British Columbia, a clear grey 'no dust emission' region exists separating the HLD and LLD regions. Given this: Why are HLD not defined by these belts, which are based on the characteristics of the environment and thus influence the physicochemical properties of the emission?

Going further, I then find it difficult to see how #7, #8, and #48 in Figure 1 are truly HLD sources and not dust sources on the periphery of low latitude source regions, which thus would have more in common with low latitude dust than HLD. More argument is needed on why these sources do indeed share common characteristics with those of HLD even though not in the HLD dust belt.

Alternatively, the definition of Arctic dust seems more in line with the true HLD source region (rather than a sub-region as present). The 50-60N region is then at best a transition zone (containing #7, #8, and #48) which is the sub-region and dust here can maybe called HLD-'like'.

Abstract: Small clarifier of what SI means as this is likely to be unknown to most readers.

L71: Please define 'High' Arctic.

L75: Please clarify how volcanic origin aerosol links to dust aerosol. The resuspension of previously deposited ash?

L79: 'weather and air quality, marine life, and human health' I think would read better as 'weather, marine life, air quality and human health'. Also add relevant refs for weather/marine life/air quality.

L93: Similarly, some refs are needed.

L181-200: Define if the total area is w.r.t. the region or the globe.

Table 1" Change the 'S' column header to say surface area – no need for an acronym that needs looking up here. Add 'area' to the column headers: total (km2), land (km2), land (%). To make the table a complete standalone item, I suggest adding a column defining what SI above a given threshold mean in general terms.

Section 5: Reads well to me, although I am not an expert for many regions listed here. Maybe add the points from Figure 1 that reside in the given area.

Section 6: See main comment.

Section 7: A very nice summary but I would think a summary figure which relates the sources to the impacts would help to tie the section together and give the reader a reference point. Such a synthesis could also contain the level of certainty in some processes or regions for example as well as research priorities.

Section 7: Although introduced earlier, I could find no direct aerosol radiative forcing interaction of HLD discussion. In particular, given the high iron content (10%; L1019) of Icelandic dust mentioned earlier I think this could be interesting to add in terms of HLD impacts.

L734: Which case? Please clarify.

L885: Reference needed.

L995: Is mineral dust a source of nitrogen in itself? Linking to the atmospheric chemistry section will likely help make this statement stronger.

L995: None of these papers detail marine nutrient limitation patterns, suggest adding (Moore et al., 2013) or similar.

L1005: Iron solubility is introduced here but it needs explaining why this is important in terms of marine productivity (similarly, what soluble iron is and why it is important).

L1028: It is quite a large assumption to infer that because a volcanic eruption will alter marine biogeochemistry that HLD will too. The amount of aerosol released in an eruption is orders of magnitude higher than in a dust event. Can the authors provide evidence that the speciation and composition of these aerosol are also alike?

L1030: The Arctic is not Fe limited. It is N limited. The reference given here shows that under very specific conditions in an artificial aerosol addition experiment - low light and high nitrate already added (i.e., potentially representative of below the euphotic zone) - in a very specific area then iron could be **co-**limiting. This cannot be used to infer that the whole basin is iron limited at times. Please remove this section as it gives the impression that the iron within HLD significantly impacts Arctic biogeochemistry when there is not the evidence of this occurring in the literature to back up such a declaration.

L1040: I think a reference to glacial tilled dust here would be insightful. This has been shown to have a higher iron solubility e.g., (Schroth et al., 2009).

Discussion/Conclusions: Is it possible to add a summary table of how HLD compares to LLD in terms of important characteristics? There are many details and some figures already in the paper which detail parts of this difference. Can this information be synthesised in a concise readily referenceable manner?

Conclusions: What is the percentage of HLD emissions to total global dust emissions? And are there estimates of past/future changes in HLD emissions which can be added?

Technical comments:

L58**:** retrieval -> retreat; waves -> wave

L125: acronym SDS needs defining (appears in combination of longer acronym before)

L125: on resolution -> on a resolution

L185: Minimum values dust -> Minimum values within dust

L864: Australia of 22 January 2020 -> Australia on 22 January 2020.

Moore, C. M. M., Mills, M. M. M., Arrigo, K. R. R., Berman-Frank, I., Bopp, L., Boyd, P. W. W., Galbraith, E. D. D., Geider, R. J. J., Guieu, C., Jaccard, S. L. L., Jickells, T. D. D., La Roche, J., Lenton, T. M. M., Mahowald, N. M. M., Marañón, E., Marinov, I., Moore, J. K. K., Nakatsuka, T., Oschlies, A., Saito, M. A. A., Thingstad, T. F. F., Tsuda, A. and Ulloa, O.: Processes and patterns of oceanic nutrient limitation, Nat. Geosci., 6(9), 701–710, doi:10.1038/ngeo1765, 2013.

Schroth, A. W., Crusius, J., Sholkovitz, E. R. and Bostick, B. C.: Iron solubility driven by speciation in dust sources to the ocean, Nat. Geosci., 2(5), 337–340, doi:10.1038/ngeo501, 2009.