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Interactive comment on "Twelve thousand years of dust: the Holocene global dust cycle constrained by natural archives" *by* S. Albani et al.

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The manuscript presents a collection of 122 dust flux Holocene time-series from various paleoclimatic archives in one database. Very importantly, the authors also include particle size data as well as estimates of the uncertainties for both the age and the dust flux data. Based on these uncertainties they select 43 records to represent global changes during the Holocene and use these to constrain dust simulations using the CESM. The work performed by Albani et al. is of great significance for the paleoclimatic dust community and provides a framework for expansion of the database. There are some issues that need to be addressed before it is ready for publication, though.

Above all, I would like to apologize for being late with my review. Very sorry about that.

Major Comments:

The major issue I have with this paper is the handling of the uncertainties. The uncertainty estimates are a very important part of this manuscript and the mathematics need to be described in much more details (can be in the summary). In each case (loess, marine, ice-core, peat) how are the final DMAR uncertainties actually calculated for data points (show the equations)? Are the errors normally distributed (discuss this for each archive)? If the errors are not normally distributed, how do the authors handle the uncertainty estimation and the error propagation (see the minor comments for more details about this)?

Minor Comments:

p. 4285, line 1: remove "formed by the accumulation of"

p. 4285, lines 8-19: I wholeheartedly agree with this paragraph. How, concretely, did you address this problem, did you average the higher resolution record? Remember to check that the data are normally distributed before averaging, else use the median.

p. 4285, lines 23-26: I would add just one sentence here about how these problems are addressed. As it stands, the reader may think there's nothing to do about it.

p. 4286, lines 2-4: Depends on your definition of "remote". I would argue that at a remote site you have no local input.

p. 4286, lines 4-6: This sounds like there is a clear cut-off between short and long-range transport. Since size distribution is so fundamental in this paper, the background on dust sizes should be considerably expanded.

p. 4286, lines 23-24: The original reference for the nssCa is Röthlisberger 2002 GRL

p. 4287, line 4: Narcisi reference is missing

p. 4288, line 4: Remove "linked to the carbon cycle". Either spend one or two sentences (with useful references) on the link or don't mention it at all. This whole sentence needs references, by the way.

p. 4288, line 9: . . ., as well as...

p. 4288, line 13: Reference about the Southern Ocean

p. 4290, line 3-7: Give some background and references on ^{232}Th

p. 4290, line 14: Add one original reference about end-member modeling.

p. 4295, lines 24-25: The Gaussian distribution of the errors is a fundamental condition for the follow-up steps in this paper and cannot just be assumed. See my comment for p. 4302, line 9.

p. 4298, line 22: Narcisi reference is missing.

p. 4298, line 24: Gabrielli reference is missing (better check them all).

p. 4298, lines 26: Talking about EC when meaning dust in ice is a bit confusing as everything in an ice-core, including the ice, falls from the sky. Why not just talk about particle concentrations?

p. 4298, lines 26-27: Un-calibrated laser data (in Volt or P/ml) is useless for determining the dust flux and not a "critical" uncertainty. If size distribution is present, though, then a conversion of P/ml to mass concentration is possible.

p. 4298, line 28 - p. 4299, line 1: Which data in the Ruth et al papers signifies that Ca is a better proxy for dust in Greenland than Antarctica? The higher Greenland Ca:dust ratios in the first Ruth et al paper are doubtful considering the difficulties with the Laser calibration especially in that early stage. Steffensen et al., 1997 for example published similar Greenland Ca:dust ratios as found in Ruth et al., 2008 in Antarctica.

p. 4299, line 3: The "significant uncertainty" is confusing here. Is this a general statement or did you mean substantial as a flag?

p. 4301, line 14: This formula would be correct if you added the two ages on both

sides. A linear interpolation, however, is of the form A_{sample} = a*A1+b*A2 with a and b between 0 and 1, depending on how close you are to one or the other endpoint. The error for the sample is then E_{sample} = sqrt(a^2 $E1^2$ + b^2 $E2^2$) (With carbon dating we can assume no covariance between the two errors). You can get the a and b values from the distance of each depth horizon from the two dated horizons.

p. 4301, line 22: I don't understand this. I see that 6.8% is 10% of one standard deviation, but I don't see the reasoning behind that value.

p. 4302, line 9: This is where we run into problems with the uncertainties. First, the equation is ε_{MAR} / μ_{MAR} = sqrt(...). Second, how were ε_{SBMAR} , μ_{SBMAR} , ε_{EC} , and μ_{EC} calculated in various datasets (see my Major Comment)? I don't know about SBMAR, but the uncertainty in EC will be a sum of many errors, some of which are normally distributed (e.g. analytical errors, bioturbation?) and some of which are not (e.g. tephras and local source contribution in ice-cores, volcanic and lithogenic input in marine sediments, etc.), which means that ε_{EC} is not a standard deviation in the Gaussian sense. You could argue that they are small enough that the Central Limit Theorem is not violated. However, all of the non-Gaussian errors I can think of are positive. Finally, what does μ represent, what exactly did you average? It should be the variable whose standard deviation is ε.

p. 4302, line 20: Why 5%? Explain or provide reference.

p. 4303, line 16: What's the spread of the analytical uncertainty in those records where it is available?

p. 4303, lines 17-20: How did you get the 26%? Based on the Steffensen data I get a Ca:dust ratio of around 1:100 also for warm Holocene times. As already mentioned, the Ruth et al., 2002 data for dust is unreliable due to the difficulty in calibrating the early lasers. Why not use the stdev of the calcium:dust ratio in the Steffensen data as a proxy-uncertainty and combine it with the analytical uncertainty instead of the arbitrary 20%?

p. 4307: line 2: MAR is spatially more or less log-normally distributed. Averaging over an area will bias the data towards high values! Use the median in these cases.

p. 4307, line 3: Are you talking about spatial gaps or temporal gaps here? In both cases you have to take into account that dust MAR is spatially and temporally close to log-normally distributed. You should therefore use linear interpolation on the logarithms of the MAR data and then transform back.

p. 4312, line 27: Maybe include Kukla in the list of citations here?

p. 4320, lines 10-23: Are the records flagged with low confidence going to be included in the database? So far, only the 43 records with high and medium confidence are included.

p. 4320, lines 13-14: The ASCII files are probably the best way to make it readable for the largest amount of people. In addition to those, I think a NetCDF file would be very handy so one can download all the data in only one file.

p. 4323, lines 14-16: I would expect the sum of all size bins to result in the Dust MAR value for that sample (second column), but this doesn't seem to be the case. What value is given in each size bin? Also, there seems to be a problem with the EDC data; three of the first 4 columns are missing and some samples seem to have their values shifted to small bins. There are some negative values in the Zagoskin data (should probably be zero).

p. 4323, lines 22-24: Could you explain the figures (in the folder /Database/Size_Description) a bit more here? I'm guessing red is the spline and black and green are the original data? How were the samples used for the figures chosen? For EDC and Vostok it looks like the size distribution was truncated at 5 um (and maybe also at the lower tail). Maybe Barbara can confirm if this is the case? If so, the model may be improved by extrapolating the spline (both EDC and Vostok seem to be overestimated by the spline).

p. 4324, lines 1-5: I don't think the MNB is much used outside the modeling community. I suggest to add a few sentences that explain this metric and how to read it.

Figure 4: Units on lower panel y-axis

Figure 5: blue = low confidence? I'm not sure the green dots add much information here. Either there's only one dot that is probably from the same record that is plotted, or they cover the whole range and thus do not really confirm anything. I suggest to either discuss the comparison with DIRTMAP3 data in page 4308 or to remove these from the plot.

Other comments: I have a general question about the ^{230}Th method: Is the difference in sea-level between LGM and Holocene taken into account? This is obviously not of any concern for this paper, but may be an issue for the extension of the method to the LGM.

Interactive comment on Clim. Past Discuss., 10, 4277, 2014.