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Interactive comment on "Dust and associated trace element fluxes in a firn core from the coastal East Antarctica and its linkages with the Southern Hemisphere climate variability over the last ~ 50 yr" *by* C. M. Laluraj et al.

Anonymous Referee #1

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The authors present a record of insoluble dust particles, Cr, Zn, Ba and Cu fluxes for the period 1960-2005, from a firn core drilled at coastal Dronning Maud Land (DML). Fluxes of these impurities were observed to double after 1985, a phenomenon which the authors attribute to changing Southern Annular Mode from negative values in the 1960's to positive values in the 1990's. The manuscript presents a limited data set (In terms of length and the number of analytes) and the interpretation also seems to be limited and somewhat a-priori. The choice of data is quite selective - important data that could be used to support the authors' arguments have been left out, there

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is an over-reliance on reanalysis data (which must carry large error bars in the pre-satellite period), and the interpretations are poorly supported. The description of the relevance of SAM is cursory (it is present in only one sentence of the introduction) and no evaluation of other climate modes (such as ENSO) are presented. The english used in the document needs to be markedly improved; there are many grammatical errors and the flow of the paper is often obstructed by the inclusion of numerous unrelated references.

The manuscript shows promise but needs to be improved with regard to a number of points - the authors seem to link references rather than use them appropriately and as support for a well-constructed interpretation. The over reliance on references results in jumps between unrelated topics in consecutive sentences (at one stage every sentence is based on a reference). The brevity of the time series fails to convince that the trend observed in the dataset isn't just noise; the link to SAM is tenuous and unsupported by statistical analysis; papers reporting and interpreting glacial dust fluxes are used to interpret modern dust flux data; and finally, the impact of land use changes and the influence of human activity on both dust and trace element fluxes appear to have not been considered.

On a more general note, I think that the authors should be cautious about using such a short data set to evaluate changes in climate. The other data sets that the authors cite, to support their argument that dust fluxes to Antarctica have increased in the 20th century, are at least two hundred years in length, and consider dust flux changes over broader periods. Enormous changes in dust flux are observed in Greenland on annual timescales, so determining the signal from the noise may require a longer data set. The authors state that a 65 m core was drilled but only the top 30 m are reported here; the impact of this data set would be greatly improved if the dust record could be extended into the 19th century.

Specific comments:

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(There are too many spelling and grammatical errors for them to be mentioned individually)

p.1842, line 10. I don't agree that the authors demonstrate a "dominant" role for SAM variability on dust transport

p.1842, line 11. "1985s" is incorrect. Either "1980's" or 1985.

p.1842, line 21. This sentence contains grammatical errors and should be improved. "Antarctica holding fingerprints" is a particularly awkward metaphor.

p.1843, line 3. "Vigorous atmospheric disturbances" is an unusual way of describing the changing conditions at the deglaciation. The role of the hydrological cycle should also be explicitly mentioned. Papers such as Mahowald et al (2005, GBC) and Maher (2010, ESR) are good sources of information regarding the current state of understanding.

p.1843, line 7. Generally speaking, insoluble volcanic particles are identified as ash or tephra and are considered distinct from insoluble dusts, which are of mineral dust origin.

p.1843, line 10. There is more recent research than that of COHMAP. For example, the two papers I mention above. Also the role of glaciation and deflation should be considered in dust production (e.g., Sugden et al., NGeo 2009).

p.1843, line 16. Define short-term and long-term.

p.1843, line 22. All of the isotopic and elemental evidence points to a dominant source of southern South American (SSA) dust during the glacial. You should specify that Australian is potentially a significant source of dust only during interglacials.

p.1843, line 29. This is the only sentence of the introduction that mentions SAM. I would have expected a much more significant description of this climate mode, upon which the interpretation of the data relies. The assertion that shifts in SAM "might"

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have changed dust deposition is a very weak argument.

p.1845, line 29. The detection limits can't be compared to fluxes. Please also provide averages and ranges for the trace element data in ug/L.

p.1846, line 4. I don't have access to Naik et al (2010) but if the results of the NIST 1640 standards are not included there, they should be included here.

p.1847, line 1. I agree that the pre-1979 data is likely to have some bias. It would be good for the authors to indicate some error boundaries or confidence levels.

p.1848. This paragraph is almost unreadable. It contains too many different references and topics, as well as grammatical errors and omissions. It needs to be completely rewritten. Particularly annoying is that the authors refer interchangeably to glacial-interglacial and decadal variability in dust deposition. Millenial-scale dust flux variability cannot be treated in the same way as decadal variability.

p.1849, line 9. A more complete evaluation of the trace element data is required. Were the elemental concentrations enriched relative to mean crustal dust concentrations? Are the data affected by pollution and/or contamination? Given that many evaluations of crustal composition (e.g., Wedepohl, GCA 1995 and McLennan, GGG, 2001) have the following order of abundance: Ba >Cr>Zn>Cu, I am inclined to think that the Zn and Cu results are influenced by either pollution, contamination or volcanism.

p.1849, line 25. I would not describe these correlations as strong.

p.1805, line 3. As mentioned earlier, the authors should also consider that Zn and Cu are emitted quiescently by local Antarctic volcanoes. Zn and Cu do not have to be attributed to anthopogenic sources, particularly in the case of Antarctica.

p.1851, line 9. This sentence is purely speculative, and is a good indication of the poor data interpretation present in the manuscript. Instead the authors should refer to the abundant data (Rare Earth Elements, Sr/Nd/Pb isotopes, etc) showing that Antarctic dust can originate from SSA and Australia during interglacials. This is also confirmed

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by various modeling studies.

p.1851, line 24. The authors offer only a visual comparison between dust and SAM. I would start to be convinced if the authors could quantify such a correlation.

p.1852, line 21. I think it is safe to say that dust reaching East Antarctica is always wind-borne.

p.1853, line 1. How do local conditions relate to the SAM index? Can you be certain that the changes in dust flux aren't just related to local katabatic wind strength and hence do not reflect all of Antarctica? The link to SSA sources would be much more convincing if you could demonstrate the changes (or lack thereof) of dust deflation from SSA sites. In this regard, the authors might look for sediment trap data from coastal SSA, or the extensive publications of Gaiero and coworkers.

pp. 1853-4. The paragraph spanning these pages is again filled with many references and little structure. The authors interchange glacial and interglacial dust transport patterns and conditions, and repeat details that they have previously mentioned, or are not appropriate. Very confusing and frustrating to read.

p.1863. The surface winds shown here seem to disagree with the data you present on page 1853.

p. 1865. I don't get a lot of information from this chart. It would be better to relate the different data by charts of [element] flux vs dust flux, and showing associated gradients and correlation coefficients. If this chart is retained, the other Antarctic dust flux records reported in the text (e.g., by McConnell et al.) should also be shown.

p.1867. This is not a very useful schematic. Instead it would be better to see an image of the different phases of SAM and how they affect Antarctic climate.

Interactive comment on Clim. Past Discuss., 9, 1841, 2013.