Response to Reviewer 2

The constructive comments/ suggestions by the reviewer is really appreciated. We have now completely revised the manuscript. In the following, we respond to the individual remarks and the revised version of the manuscript will be soon transmitted. Reviewer's comments are identified by RC and authors' by AC.

RC: The authors present dust and trace element data from a shallow firn core in coastal Dronning Maud Land, Antarctica. They mostly relate changes in the records mentioned above to changes in the Southern Annular Mode. It is an interesting study and the results the authors show lead to the assumption that there is an interesting message in the dust record of this core. However, the methods are partly questionable and the paper generally suffers from the lack of a sound meteorological background, which becomes evident in many formulations/quotations in the text and in the simply wrong Figure 5.

AC: We are thankful to the reviewer for the encouraging and positive comments to improve the manuscript. We have considered the various suggestions made by the reviewers and have accordingly rewritten the manuscript. As suggested, we have removed the schematic given in figure 5 and replaced it with another figure by computing wind anomalies between negative (before 1985) and positive (after 1985) phases of SAM. This figure clearly demonstrates the increase in westerlies strength after 1985 compared to the period prior to it.

Specific comments:

RC: I do not address the points reviewer #1 already discussed unless I disagree.

AC: The points addressed by reviewer 1 was taken into consideration and made necessary changes.

RC: I do not point out single language mistakes either, the text needs language editing.

AC: We have done thorough English editing and corrected the grammatical mistakes in the revised manuscript.

RC: 1842; 11-13: here is some confusion of easterlies, westerlies etc. (see below)

AC: We have revised the section to avoid such confusion.

RC: 23: delete "the"

AC: Correction has been made in the revised manuscript.

RC: 1843; 4: what is an atmospheric "event"?

AC: This has been changed to "atmospheric circulation"

RC: 5ff: rewrite: the origin is not related to accumulation rate or atmospheric cleansing..What do you mean by "weak hydrological cycle"?

AC: The entire paragraph has rewritten as per the reviewer's suggestion.

Some of dusts/aerosols enhance scattering and absorption of solar radiation and produce brighter clouds that are less efficient at releasing precipitation. These may

lead to the suppression of wet precipitation, and less efficient removal of pollutants. A weaker hydrological cycle will be resulted under such conditions.

RC: 1844;27: it would be interesting to see the data from the whole core and it would strengthen the statistics, however, I agree that during the past 3-4 decades distinct changes have been observed and it does make sense to look at the most recent period only. Also, the reanalysis data are more reliable during this time than before 1979.

AC: We thank the reviewer in supporting our intention. Considering that we have analysed large number of samples (n = 470) for different size components and important trace metals, we believe that although the time period represented is limited, it offers better insight into the utility of ice records for the study of short-term climatic changes. Processing of the entire core require longer time period and will be undertaken in future.

RC: 1846;16: there are more recent mass balance studies of DML that could be quoted here 25:

AC: Recent references have been incorporated in the revised manuscript.

RC: Marshall (2003) found substantial differences in SAM index derived from Reanalysis data and for observational data.

AC: Even though there are differences in SAM index derived from Reanalysis and observational data, the differences are not large and there is no much difference in the correlation analysis. Since the complementary data (wind) we use are from Reanalysis, we use the SAM index from Reanalysis for consistency. We are confident that our selection of dataset does not affect the main interpretations.

RC: 1848 6: I assume it is meant in this ice core?

AC: Yes.

RC: 7: air temperature where?

AC: Surface air temperature derived from $\delta^{18}O$ data of the same core as reported earlier (Naik et al., 2010).

RC: This is not supported by other studies in coastal DML (Ekströmisen, Fimbulisen, Neumayer,SANAE) 10ff: this is pretty unclear: what do you mean by atmospheric turbulence, why should dry air lead to violent storms? Australia is most likely no dust source for DML.

AC: Air temperature from the present core was discussed by Naik et al. (2010) and this is not contributing much to this manuscript, hence we have removed the same during revision. However, since the core station is very close to Novo station, we compared the derived ice core temperature data with the AWS data from Novo. This comparison is discussed in Naik et al (2010).

RC: 1850:13: it is not clear what is meant by “stronger transport”

AC: We have revised this part for clarity.

RC: Trajectories: Calculation of trajectories for only one month of a restricted time period to investigate the transport in the time period that is covered by the ice core data seems to be quite arbitrary. The choice of 10 days is not explained either. It should make a difference if dry or wet deposition is considered. I am not a dust expert, but I would assume that, in the coastal areas, wet deposition is not negligible. At least this should be discussed. Trajectories also can never be discussed without cross-checking the results with the general synoptic situation(s) during the transport. Trajectories with kinks, as shown in Fig. 4, are very unlikely to represent the real path of the air/dust particle. The colours in Fig. 4 are not explained either and generally Fig. 4 is too small (especially the labels) to really recognize more than the coarse features. The choice of 1985 as threshold year for observed changes of various parameters is quite arbitrary, too.

AC: Since a detailed trajectory study is beyond the scope of this study, we have computed representative back-trajectories for one month each of the two major periods before and after 1985. These periods were chosen since they are in the same phase of El-Nino Southern Oscillation (ENSO) and ENSO has a known linkage with southern hemisphere climatic variability (Michelle and Thompson, 2006). Li et al. (2010) have performed a simulation of dust transport in South America and found that it takes 4–5 days for dust to reach high-latitude South Atlantic after its emission and ∼7 days for the dust to reach Antarctica. Further, Stohl and Sodemann (2010) found that age of Antarctic air in the lowest 100 m of the atmosphere in the cDML for August is 3 – 4 days and for January it is 4 – 7 days. Accordingly, the 10 days trajectory simulation time is sufficient to understand the dust deposition at the present core site. We have considered both wet and dry deposition while computing the trajectories.

We agree that the small scale details on the reconstructed trajectories may not be reflected in the real path of the dust particles, and have therefore, used the back-trajectories mainly to infer an overall understanding of the source region. Necessary changes have been done in Figure 4 as per reviewer suggestions.

RC: 1851; 2nd paragraph. The whole paragraph should be rewritten. “Positive shift” sounds more like a sudden jump rather than a gradual increase/tendency to positive values in the SAM index.

AC: As per reviewer suggestion the paragraph has been rewritten.

RC: 20: It does not make sense to distinguish between the circumpolar vortex and the “southern westerlies”.

AC: We have rewritten this part.

RC: 21: shifts in the wind don’t “alter” the circulation pattern, the winds are the circulation.The relationship between SAM and dust is described only qualitatively, a figure and a quantitative correlation would be helpful and more convincing.

AC: We agree. We have now used statistical techniques in the revised text to support the causal relation between the SAM and dust flux at the core site. The analysis revealed a strong positive correlation ($r= 0.68$ $r<0.00000001$) indicating the role of SAM on dust transportation over East Antarctica. The same has been included in the revised manuscript.

RC: Page 1852/1853-These paragraphs are hard to read and contain several contradictory and incorrect statements.2: Australia as a modern dust source is unlikely. 4: first it is stated that there was a "positive shift" in SAM in the 1960s, now it is after.

AC: The two paragraphs have been completely rewritten as per the reviewers' suggestions.

RC: These formulations are all pretty vague, and there is not much happening in 1985. Which SAM index and what kind of filter/smoothing is used in Fig. 2? The authors use the terms circumpolar vortex and westerlies in a quite confusing (confused?) way.

AC: The SAM index by Nan and Li (2003) is used. The filter used is a Fourier transform low pass filter. Antarctic circumpolar vortex is the flow of air in the upper and middle latitudes around Antarctica and would be influenced by SAM. When SAM moves to high positive value the circumpolar vortex become strong, enhancing the strength of mid-latitude westerlies. We have rewritten the text to improve the quality of interpretations.

RC: What is meant by "gradient of zonal wind strength?

AC: We have now revised the text to avoid such confusions.

RC: Here periods of different length are compared.

AC: Since the present study is about comparing the significant changes occurring around 1985, we compared the data prior and after 1985. However, we have also checked trend between 1964 – 1985 and 1985 – 2006 and are showing almost similar values.

RC: Fig. 2f is contradictory to Fig.1 in fig1a the zonal wind speed at the core site is way higher than in the later period in Fig. 1b. the "strength" of the westerlies is hard to assess since it depends on latitude and longitude. Thus the general statements of the authors concerning these winds are not clear.

AC: We agree. In the revised manuscript, we have calculated wind speed anomalies between 1960-85 and 1985-2006 and the same has been given in figure 5. This figure clearly indicating an increase in westerlies speed after 1985 compared to the period before that. The Figure 1 represent the wind pattern for the entire Southern Hemisphere and Figure 2e represent the wind speed at the core site taken from the nearest grid point from the NCEP/NCAR reanalysis data.

RC: Page 1853/54 are not understandable, and Fig. 5 is incorrect. On average, the coastal areas of Antarctica are under the influence of cyclonic activity in the circumpolar trough, whereas a large-scale subsidence of air masses is observed under anticyclonic influence above central Antarctica. The "enhanced polar easterlies" are due to stronger cyclones and not related to surface anticyclones (l 14.). Enhanced westerlies would lead to stronger zonal transport of dust, but also to reduced meridional (southward) transport. Here again the question of wet deposition comes up.

AC: The figure 5 has been changed and as per the suggestion. We have plotted a new figure by computing wind anomalies between negative (before 1985) and positive (after 1985) phases of SAM. This figure clearly demonstrates an increase in westerlies strength after 1985 compared to the period prior to it.

RC: 1854;20ff: This does not make sense. I find it hard to comment on any single sentence here.The whole section 3.2 has to be rewritten. There is an obvious lack of understanding of the dynamic processes involved, which results in wrong statements, strange formulations and mixing up of cause and effect.

AC: The entire paragraph has rewritten as per reviewer's suggestion and revised to clarify the logical dynamical processes.

Since the structure of the paper makes it difficult to write a well-structured review, I would like to answer to the questions to the reviewers now:

Does the paper address relevant scientific questions within the scope of CP?

Yes. SH climate of the past 50 years is discussed in relation to ice core data.

Does the paper present novel concepts, ideas, tools, or data?

Yes. New data from a firn core are presented. New ideas, too, though some wrong ideas amongst them.

Are substantial conclusions reached?

Yes. Not necessarily sufficiently supported by the data and used methods.

Are the scientific methods and assumptions valid and clearly outlined?

No. Especially the choice of the 1985 threshold is arbitrary and no reason is given for it.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists?

Principally yes. The authors do not hide anything, only the description of what they did and why is not always easy to understand.

Do the authors give proper credit to related work and clearly indicate their own contribution?

Yes. However, the text often sounds like just a row of citations, which does not make it easier to read/understand what the authors mean.

Does the title clearly reflect the contents of the paper?

Yes.

Does the abstract provide a concise and complete summary?

Yes.

Is the overall presentation well structured and clear?

No

Is the language fluent and precise?

No

Are mathematical formula, symbols, abbreviations, and units correctly defined and used?

There are no formulas in the paper, the use of the term wind strength rather than wind speed is not correct.

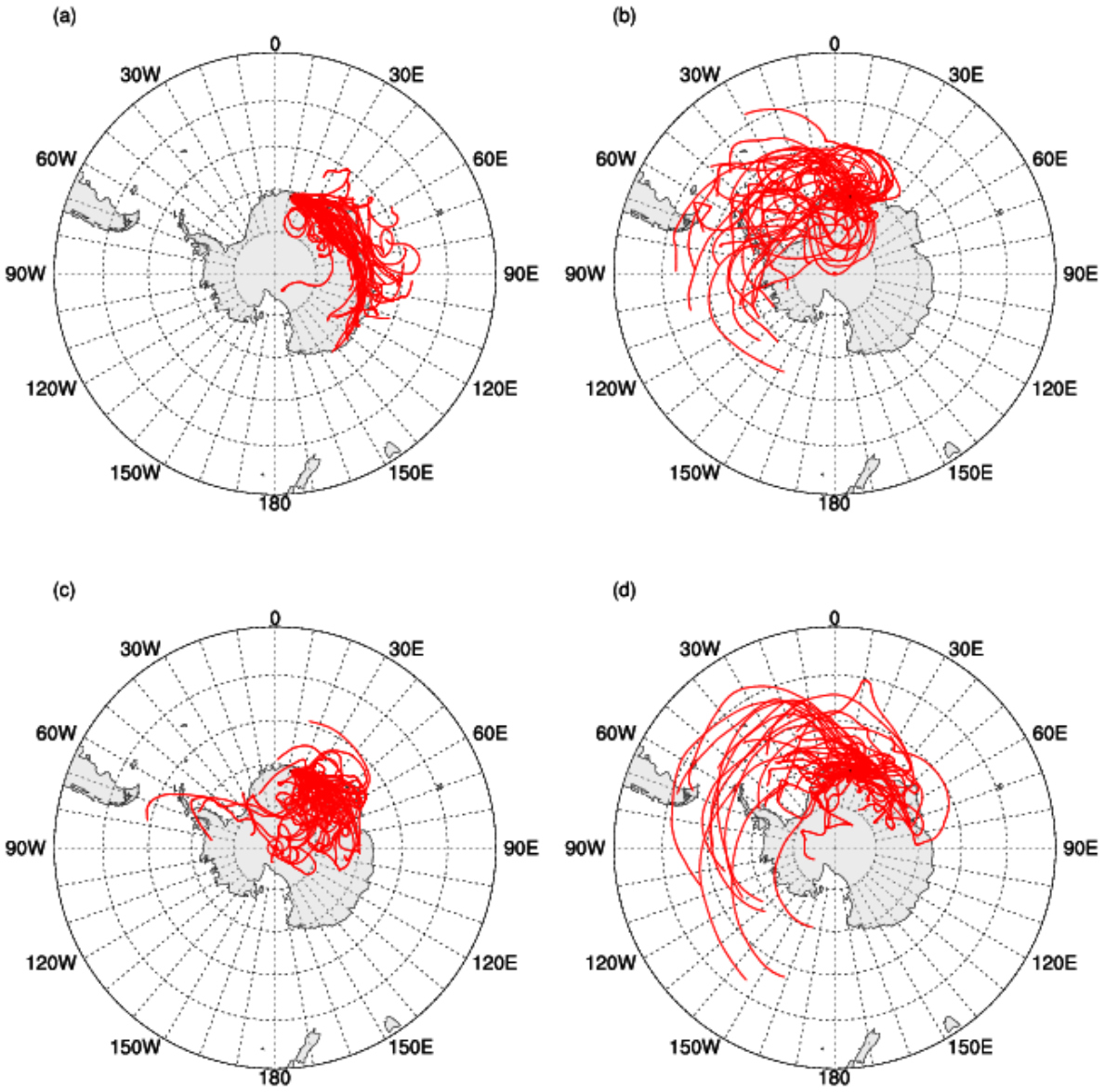

Figure 4. Clustered back-trajectories at study site representing the summer and winter seasons during (a) January 1983, (b) August 1982, (c) January 1998 and (d) August 1998. Each red line represent the trajectory on a particular day.

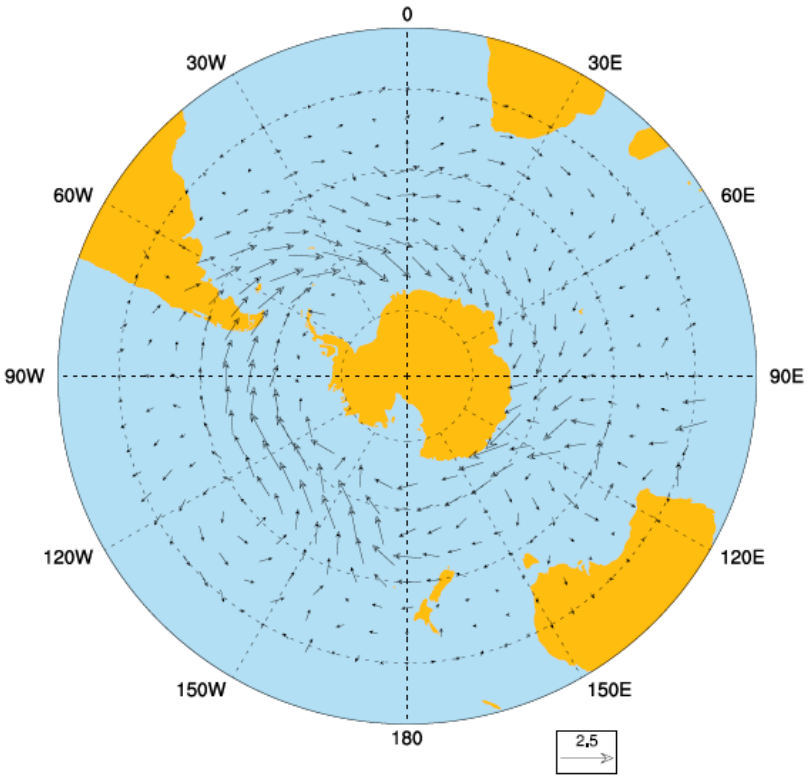

Figure 5. The wind anomalies computed between positive phase of SAM (After 1985) and negative phase of SAM (before 1985). The surface wind data taken from NCEP-NCAR Reanalysis data is used for calculating the difference.