

To the Reviewer,

Thank you very kindly for your brief but succinct review of this work. I will refrain from addressing your general comment that *“The paper is severely flawed due to inappropriate use of method and lack of knowledge of literature. The author claims and assumes properties and processes that are directly wrong.”* as this statement is wholly unsupported by the nature and detail of your comments.

I will address each of your specific comments individually;

Comment #1:

“An ice core record of “insoluble micro-particulate sulfate” does not exist. Some measurements on elemental sulfur from insoluble micro-particles have been done; but by far most measurements are of soluble sulfate (i.e. SO₄⁻) either by ion-chromatography or by continuous flow analysis. Sulfate has several sources: Terrestrial minerals (e.g. gypsum), Marine salts, Marine biology (e.g. from phytoplanktonic DMS), Terrestrial biology (e.g. H₂S) and of course volcanic activity. To claim that total sulfate is from volcanoes is wrong. Check the literature.”

Response:

What we have written in the manuscript;

“The majority of warming events (increasing d18O) are preceded in the GISP2 ice-core record by an increased concentration of insoluble micro-particulate sulfate (Fig. 1), indicative of increases in global volcanic activity (Zielinski et al., 1997; Zielinski and Merishon., 1997)”

You are correct, and this sentence is misleading. The *“insoluble micro-particulate sulfate”* should be *“insoluble micro-particulate correlated to sulfate”*. Where the insoluble micro-particulates we are referring to are micro-tephra. We have clarified this;

“The majority of warming events (increasing d18O) are preceded in the GISP2 ice-core record by an increased concentration of sulfate correlated with microtephra (Fig. 1), indicative of increases in global volcanic activity (Zielinski et al., 1997; Zielinski and Merishon., 1997)”

Note, we are not using the raw sulfate signal, but the volcanic sulfate signal as referenced. We reproduce excerpts from Zielinski et al., (1997) and Zielinski and Merishon (1997)

“The deposition of volcanically derived SO₄²⁻ is noted by the presence of large SO₄²⁻ spikes much above background levels. The coincidence of these spikes with the timing of known explosive volcanic eruptions support this concept” Zielinski et al., (1997)

“During the glacial the added input from continental salts makes a direct detection of volcanic SO₄²⁻ by the presence of large SO₄²⁻ spikes much harder. To alleviate this problem, we used an empirical orthogonal function to identify peaks due to the deposition of volcanic aerosols.” Zielinski et al., (1997)

“Volcanic SO₄²⁻ signals from known historically dated eruptions (e.g., Zielinski et al., 1994) were used as time lines to assist in calibrating the layer counting. The location and identification of microtephra in the core verify the source eruption responsible for some of these chemical signals (e.g., Zielinski et al., 1995, in press) and the age of the individual layer containing the tephra.” Zielinski and Merishon (1997)

Comment #2:

“Sulfate in ice cores correlates really well with dust and calcium, and they all correlate with O-18 ice which should demonstrate that the bulk of the sulfate signal across D/O events is climatic and not due to volcanism.”

Response:

By this the reviewers logic sulfate can not be by a record of volcanism, therefore they must also not agree with the works of;

Mayewski, P. A. *et al.* Major features and forcing of high-latitude northern hemisphere atmospheric circulation using a 110,000-year-long glaciochemical series. *J. Geophys. Res.* **102**, 26345–26–366 (1997).

Zielinski, G. A. Use of paleo-records in determining variability within the volcanism-climate system. *Quaternary Science Reviews* **19**, 417–438 (2000).

Zielinski, G. A. *et al.* Volcanic aerosol records and tephrochronology of the Summit, Greenland, ice cores. *J. Geophys. Res.* **102**, 26625–26–640 (1997).

Zielinski, G. A., Mayewski, P. A., Meeker, L. D., Whitlow, S. & Twickler, M. S. A 110,000-yr record of explosive volcanism from the GISP2 (Greenland) ice core. *Quaternary Research* **45**, 109–118 (1996).

Zielinski, G. A. *et al.* Evidence of the Eldgja (Iceland) eruption in the GISP2 Greenland ice core: relationship to eruption processes and climatic conditions in the tenth century. *The Holocene* **5**, 129–140 (1995).

Zielinski, G. A. *et al.* Record of volcanism since 7000 BC from the GISP2 Greenland ice core and implications for the volcano-climate system. *Science* **264**, 948–952 (1994).

Further, it is unclear what the reviewer means by stating that correlations with ice-core sulfate with $\delta^{18}\text{O}$ is climactic and not due to volcanism. What is the source of their “climactic” sulfate? It is also important to note that there is a significant lag time in the correlation between $\delta^{18}\text{O}$ and the sulfate record, I address this explicitly in the work yet the reviewer makes no reference to this.

Comment #3:

“The author assumes that the climatic variations of the D/O events are global, which they are not. The bi-polar see-saw has been observed also in ice cores. Therefore the rationale of the paper does not hold, since climate variations in the North and the South are out of sync.”

Response:

This is a reworded version of **Comment #4**, please refer to that response. On an additional note, if, as has been supported by Zielinski and others, that the sulfate record is a proxy for volcanism, then there is no way that large this volcanic input would not propagate globally. See;

Robock, A. and Mao, J.: Winter warming from large volcanic eruptions, *Geophys. Res. Lett.*, **19**, 2405–2408, doi:10.1029/92GL02627, 1992.

Hansen, J., Lacis, A., Ruedy, R., and Sato, M.: Potential climate impact of Mount Pinatubo eruption, *Geophys. Res. Lett.*, **19**, 215–218, doi:10.1029/91GL02788, 1992.

Graft, H.F., Kirchner, I., Robock, A, and Schult I.: Pinatubo eruption winter climate effects: model versus observations, *Clim. Dyn.*, **9**, 2, 81-93, 1993.

Trenberth, K. E. and A. Dai , Effects of Mount Pinatubo volcanic eruption on the hydrological cycle as an analog of geoengineering, *Geophys. Res. Lett.*, **34**, L15702, 2007

Comment #4:

“Global levels of methane and CO₂ are also out of sync. as the methane follows the D/O cycles in the North whilst the CO₂ levels follow the Southern ocean temperatures. Both gases cannot follow the same “volcanic signal”.”

Response:

I am uncertain of why the reviewer says that methane and CO₂ are out of sync. Is this their opinion or interpretation of the data? The time series record (Fig. 1) shows a correlated increase for both methane and CO₂ for the highlighted DO events. The reviewer says both gases cannot follow the same “volcanic signal”, yet that is directly refuted by the time series data. The reviewer should offer supporting reasoning for their conclusion instead of making broad/general/non-specific comments that are not supported by the time-series data/

Comment #5:

“Using wavelet analysis on ice core time series has been done before, almost 20 years ago, albeit with different aims and rationales.”

Response:

Once again the reviewer makes a broad negative statement, without supporting evidence or references. Fourier analysis has been done in the past on the entire time-series of data covered by this work; wavelet analysis has only been performed on a subset of the record, up to the LGM (Debret et al, 2007). If the reviewer would like to offer specific supporting references to this comment, I could respond more fully.

Comment #6:

“The GISP-2 time scale was good at its time 20 years ago; but the author should use the present standard time scale, GICC05modelext, and he will observe big changes. The changes are not so much in the absolute ages, but rather in the duration of Stadials and Interstadials, and this has changed our view on accumulation rates across D/O events which directly leads to a different 10Be curve.”

Response:

While the NGRIP/GICC05 may offer a more accurate time-scale, this accuracy is first (1) limited to chronologies extending to only 60,292 b2k, not the entirety of the time period we are investigating. Secondly (2) while the correlation between NGRIP/GISP2/GRIP is highly resolved through the GICC05 tie-in's, this correlation is primarily limited up to the LGM – which is the lower boundary for our time series investigation. Specifically, (a) “The dating of the period from 14.7 to 42 ka b2k is based on the visual stratigraphy and measurements of the ice conductivity.” (b) general agreement was seen between the two timescales for most of the record, although you are correct they do see a change in the stadal duration;

“As seen in Fig. 1, in general there is a very good agreement between the GISP2 and GICC05 time scales. GICC05 agrees with GISP2 to within 250 yr over the entire period back to 30 ka, and the two chronologies determine the onset of interstadials within 300 yr (Fig. 2). One will notice, however, that the duration of the interstadials/ stadials generally appears longer/shorter for the GISP2 time scale than for GICC05 respectively” (Svensson et al., 2006)

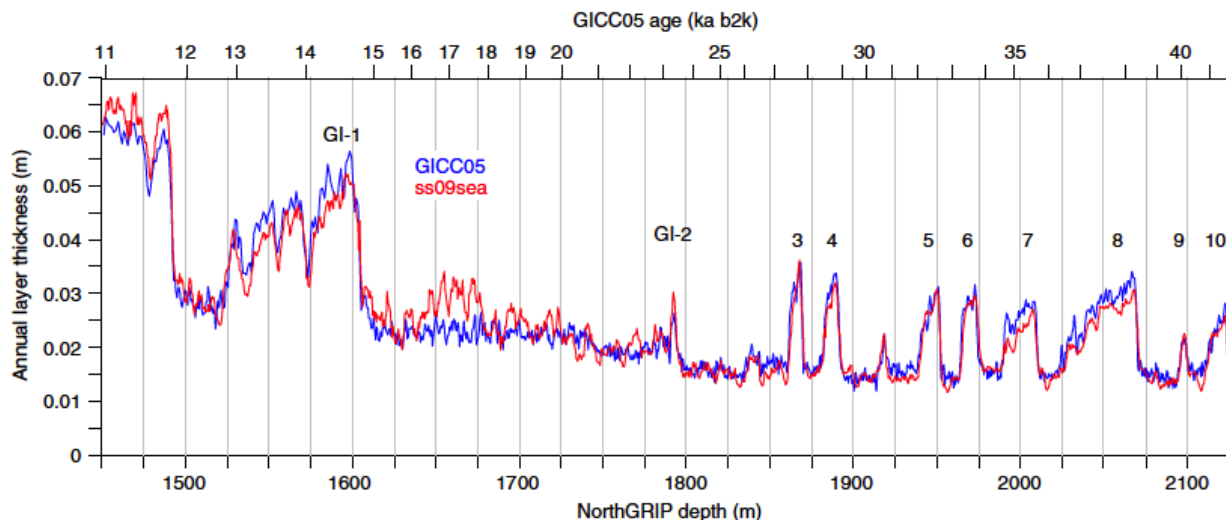
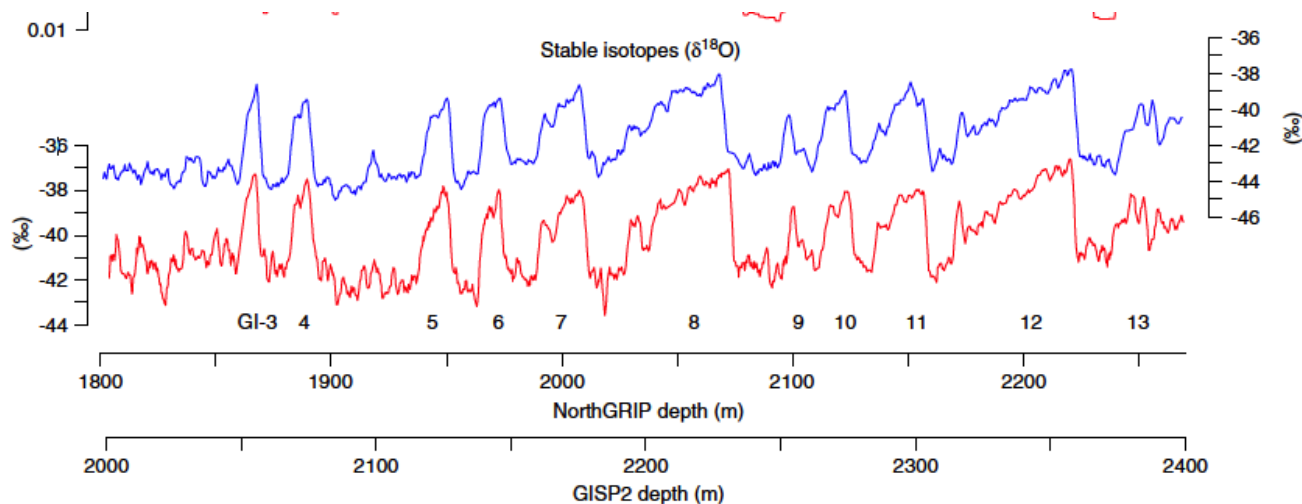


Fig. 3. Comparison of NorthGRIP annual layer thicknesses between the 'ss09sea' model and the GICC05 time scales.

But note, small change in their length will not primarily affect their periodicity. We are also not directly concerned with the accumulation rates, only as they affect the stable isotope record. Most importantly the delta 18O periodicities and durations are not .



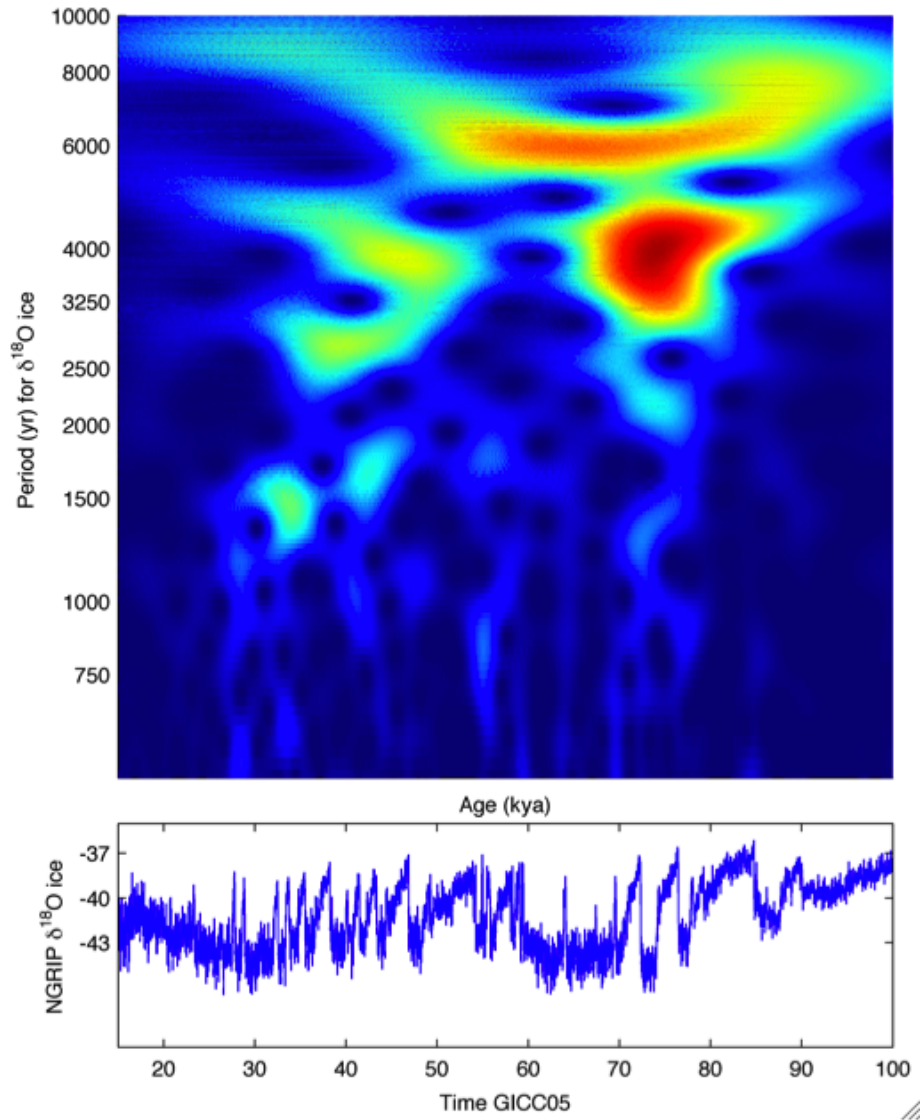
While Svensson et al. (2006) proposes that;

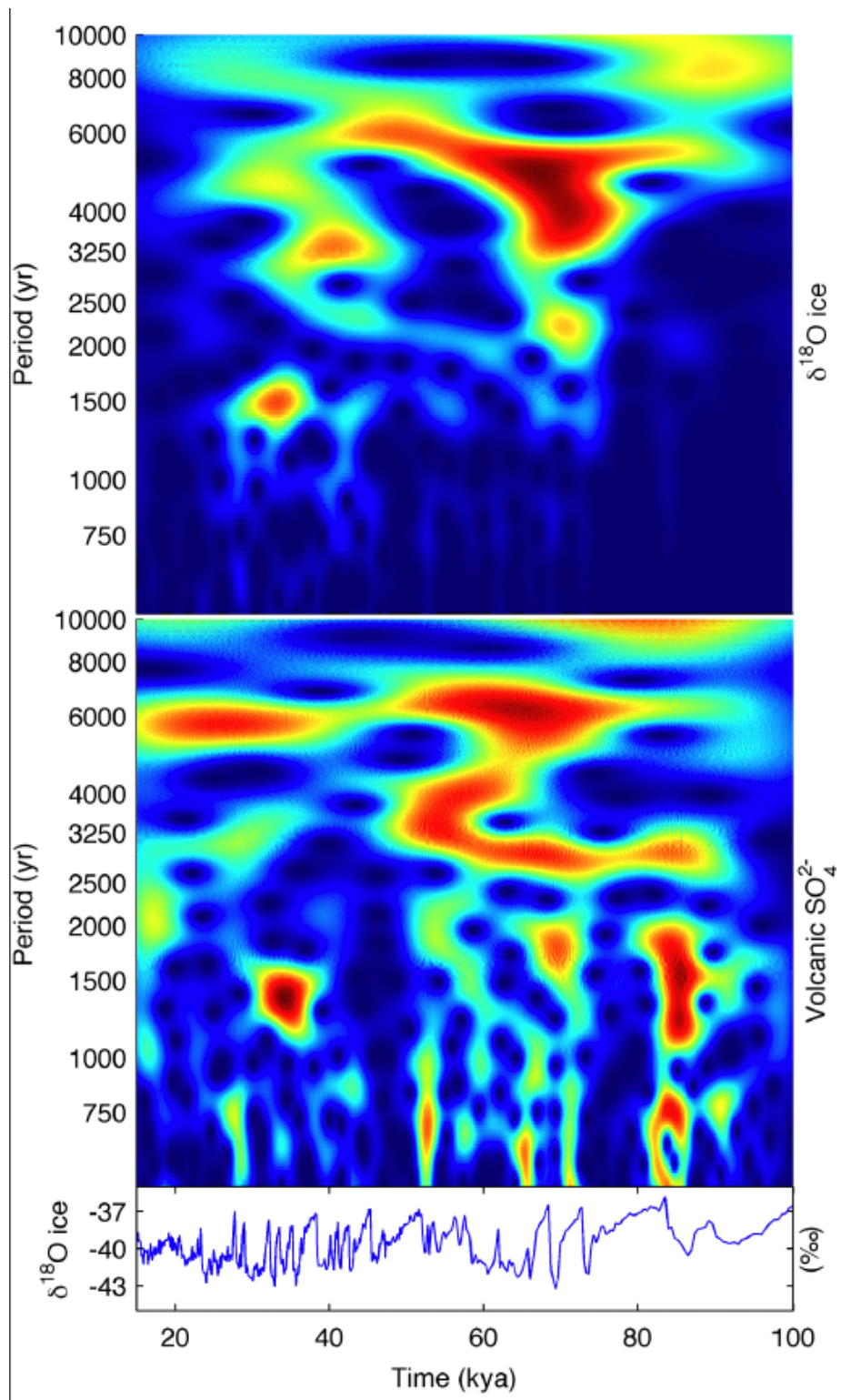
“many others discuss the existence of a 1470 yr climatic cycle in the stable isotope profiles of the last glacial period. The conclusions of these studies may very well be justified, but we would like to emphasize that the finding of such periodicities obviously relies profoundly on the applied time scale. For example, the existence of the proposed 1470 yr cycle depends on the exact timing and phasing of the onset of D–O events, and, as discussed above, this is exactly where we believe that the GISP2 time scale is inaccurate.”

They do not offer and discussion on this proposed in accuracy – making their comments supposition at best. We should also note, that GIPS2 d18O data on the GICC05 time-scale is only available for 11.6-32.55 ka b2k, once again only a small portion of our total time-series. (<http://www.iceandclimate.nbi.ku.dk/data/>).

It is unreasonable for the reviewer to question why we did not use the GICC05 time-scale, as the correlation for GISP2, for the range we are investigating, does not exist.

To address those that may be still hesitant, and still insistent that the Svensson et al. (2006) proposition that the continually observed 1475 periodicity observed in the GISP2 d18O record is an artifact of the GISP2 time-scale, we redo the frequency analysis for the NGRIP d18O record (with its GICC05 time-scale). We observe similar periodicities between this analysis and our original GISP2 analysis. We are fully confident in our original processing.





Original, as appear in the manuscript.

Comment #7:

Why is Mayewski et al. 1997 not cited, as they were the producers of the sulfate curve from GISP-2. Read the paper in JGR 1997.

Response:

The primary references listed for the volcanic sulfate record, available at:
<ftp://ftp.ncdc.noaa.gov/pub/data/paleo/icecore/greenland/summit/gisp2/chem/volcano.txt>

are;

Zielinski, G.A., and G.R. Mershon. 1997. Paleoenvironmental implications of the insoluble microparticle record in the GISP2 (Greenland) ice core during the rapidly changing climate of the Pleistocene-Holocene transition. Geological Society of America Bulletin 109:547-559.

Zielinski, G.A., R.J. Fiacco, P.A. Mayewski, L.D. Meeker, S.I. Whitlow, M.S. Twickler, M.S. Germani, K. Endo, and M. Yasui. 1994. Climatic impact of the A.D. 1783 Asama (Japan) eruption was minimal: Evidence from the GISP2 ice core. Geophysical Research Letters 21:2365-2368.

I thank you for the suggestion of including the original source data as a reference, and yes, I have read the Mayewski et al. (1997); I have updated the references.

Comment #8:

ECM is totally unsuited for wavelet analysis across D/O events because the signal is completely unlinear. Due to high content of calcium carbonates in the cold stadials the ice chemistry becomes slightly alkaline killing the ECM signal. ECM is not a measure of dust. ECM records the acidity as the signal is a function of H⁺ concentration (pH value).

Response:

While the ECM is nonlinear, we are not relying on the interpretation of the ECM periodicities to come to our conclusion. The correlation between ECM, dust and 10Be is seen clearly enough in the time series. We provide the wavelet transformation to be self consistent in our analysis. Wavelet transformation is often used in nonlinear analysis, although often and more strongly in tandem with a Hilbert transform. I refer you to;

Daubechies, I., 1992. Ten Lecture on Wavelets, SIAM.

Jawerth, B. and Sweldens, W, 1994. An Overview of Wavelet Based Multiresolution Analysis, SIAM Rev., v. 36, no. 3.

Nikias, C. L. and Petropulu, A. P. , 1993. Higher-Order Spectra Analysis. A Nonlinear Signal Processing Framework, Prentice-Hall.

Yes you are correct, during cold conditions, high concentrations of calcium carbonate dust alter the acidity of the ice – neutralizing most of the signal. I do not understand how you can say this, and then say ECM does not measure “dust”. True it is not a direct measuring of dust content, but it is highly susceptible to it, which is the point we are making in Figure 4. We see the ECM signal go to zero when the Be10 concentration is high, indicating that the ECM is being flatlined by high concentrations of dust, which we believe is indicative of high Be10 dust concentrations as well...

“Rapid climate transitions associated with the Younger Dryas and Dansgaard/Oeschger interstadial events alter the concentration of alkaline dust and are detected by the ECM.” As taken directly from *Kendrick et al.*(1997), Electrical Measurements on the Greenland Ice Sheet Project 2 Core.

Continuing through *Kendrick et al. (1997)*;

“In the stadial portion of the core, seasonal variations in dust dominated the ECM (Figure 1b), visual stratigraphy and dust concentration measurements. Because these measurements are measuring a different attribute of the same seasonal phenomena (the dust concentration) they are not completely independent. Any meteorological or glaciological phenomena which adversely influences the seasonal dust signal will adversely influence all three of these records”

Your comment is self refuting.