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Climate of the Past Discussions

Interactive comment on "Mineral Dust Influence on the Glacial Nitrate Record from the RICE Ice Core, West Antarctica and Environmental Implications" *by* Abhijith U. Venugopal et al.

Anonymous Referee #2

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This manuscript presents new ionic chemistry data from the RICE ice core in the Ross Sea region of Antarctica and focuses on the relationship between nitrate and non-sea-salt calcium (nssCa). Data from MIS 3 are presented, encompassing AIM events 3-8. The authors claim that nitrate and nssCa co-vary across AIM events, as has been observed previously in 3 East Antarctic cores. This apparent similarity between RICE and the 3 East Antarctic cores is then assumed to be a continent-wide signal and stated to demonstrate that NO3 reacts with nssCa in the atmosphere prior to deposition (rather than in the snowpack). Finally, this potential interaction of nitrate and nssCa in the atmosphere is used as a basis to claim that nitrate could be used as a proxy for Southern Hemisphere Westerly Winds (SHWW) strength/position during the Last

Glacial.

I find the conclusions of this study uncompelling. They are barely supported by the data analysis presented. The statistical analysis itself is poorly explained and error-prone, meaning the critical observation of co-variation between nitrate and nssCa is placed in serious doubt. Going further, to extrapolate one coastal site to the entire Antarctic continent is not well-justified so we don't really learn anything about the controls on ice core nitrate. The claim of a new SHWW proxy should be removed entirely. In short, I cannot recommend this work for publication.

I provide specific comments below but I stopped at section 4.2.2 when it became clear the discussion had moved beyond what is justified by the data and analysis presented.

Major comments Time interval chosen: The authors don't state why this time interval was chosen for analysis. Why is the deglaciation not included? It would be interesting to see how the proposed nitrate-nssCa relationship responds under different accumulation rates. L197-202 hint at this but no Holocene data is presented so this text is redundant.

Data analysis: The primary observation of the paper is that nitrate and nssCa co-vary across MIS3. The first problem is that this is extremely difficult to see on Figure 2. Yes, there are two peaks (Circled on figure) that are coincident in the two records but that is about it. Maybe plotting the records on a log scale and closer together would help, but I am doubtful. There is no correlation reported between the datasets, despite the claim of "statistically significant coupling" made at L153. The second problem here is that variability in nssCa or nitrate linked to AIM events (as claimed L140) is near-impossible to see – the variability in both is much higher frequency.

Next is the observation that there is a systematic shift in nitrate and nssCa records at ∼31 ka. How was this change-point in the records identified? What is the significance of this observation – maybe I missed it by not reading through to the end.

It seems that the authors try make up for the lack of [reported] correlation between nssCa and nitrate by using more sophisticated statistical methods: PCA and wavelet analysis. I'm afraid I have concerns about the validity of both. PCA: More information about the pre-treatment of data is required. How are outliers defined? Are all the data mean-centred and normalised? PCA is not usually suitable for chemistry data that has a skewed distribution – it is often appropriate the log-transform data first to ensure you start with something like a normal distribution. The risk of not following these steps is that outliers heavily influence the result. Without seeing the datasets used or the resulting EOFs, it is difficult to know, but my concern is that PC1-3 are dominated by 'extreme' variability, e.g., the two coincident spikes in nitrate and nssCa in the case of PC3.

Wavelet analysis: I am not an expert on this technique but know a little. The text to describe both the method (L121-124) and the results (L153-156) is imprecise. The figures need a significance level adding, otherwise the results are meaningless. On Figure 3, the result is again dominated by the two high peaks in nitrate and nssCa. This adds little to the study.

Extrapolation of RICE result: The comparison of RICE to the 3 East Antarctic sites is almost redundant. We already know that nitrate and nssCa are related at these sites. But the RICE results alone aren't enough the infer something about the controls on nitrate (even if the analysis is robust). WAIS Divide nitrate and calcium data are available in high resolution for this time interval. I'm not aware that anyone has looked into the nitrate – your study would be strengthened if supporting evidence could be gleaned from another warm, West Antarctic (though not so coastal) core. https://www.usap-dc.org/view/dataset/601008

Minor comments - Did you consider comparison with acidity or DEP data, following Röthlisberger et al., JGR 2000. - There is no accumulation rate information included. Could it explain some of the variability in nssCa and/or nitrate? - L190-196: Doesn't N and O isotopic data from nitrate suggest a lot of coastal nitrate is actually recycled from the interior? Sorry I can't remember the reference but it would be from Becky Alexander's group.

Figures: Fig. 1: Just a suggestion but could you label the elevation contours and include accumulation rate field in colored shading (even if it is present-day rather than glacial)? Accumulation rate is important for the nitrate preservation. Fig. 2: Inset map not needed if Fig.1 included. Fig. 3: As other reviewer commented, one plot appears to be flipped. Check colorbar labels are accurate.

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