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The Cryosphere Discussions

Interactive comment on "A 125-year record of climate and chemistry variability at the Pine Island Glacier ice divide, Antarctica" *by* Franciele Schwanck et al.

Anonymous Referee #2

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In this manuscript the authors present elemental analysis from an ice core located on the Antarctic Peninsula. They combine this analysis with backtrack trajectory modeling, identifying the major air masses pathways to the site. They also correlate elemental time series with sea-ice thickness and SSTs around the continent, identifying regions that correlate with marine tracers. Once the very good and exhaustive literature review is subtracted, there is unfortunately not much substance to this manuscript, as it presents very few new insights. It appears to be made up of three sections, one about the elemental analysis and the correlation with SIC and SST, one second about marine vs crustal contribution of aerosol impurities, and one third about the backtrack trajectory modeling, and no link between the three. Because of this, the paper is mostly a

data paper presenting elemental concentrations and a theoretical exercise with Hysplit, and the combination of the two is not stronger that the separate halves. Although the theme of this manuscript fits within the scope of The Cryosphere, I think the method and the analysis are flawed to the point that the results will be significantly altered. For this reason I suggest to reject the paper at this point so that it can be completely rewritten with no time constraint and resubmitted at a later time.

Major comments:

1. ERA and NCEP data are notoriously unreliable over Antarctica, with huge biases compared to measurements. Especially the 1000m winds rely mostly on simulated model values, which are also notoriously wrong in Antarctica. Bracegirdle and Marshall (2012) may have determined that ERA-Interim data are the most accurate of the 6 reanalysis models, but that doesn't make them correct or even close to reality. What's the sensitivity of your results when using the other reanalysis datasets? How do the ERA data compare with climatology timeseries of monitoring stations close to MJ?

2. The whole chapter 3.1 is methodically flawed (see minor comments below). This puts into doubt most of the interpretation based on these data. See the minor comments below.

3. In chapter 3.3 you mention that modeling studies suggest Australia as the main source for the Antarctic Peninsula, but that ice core studies mostly identify a mix of sources. What about your results of non-marine tracers, you don't mention these in the paper. If you decide to concentrate on marine tracers, then the calculation of nss concentrations is not necessary.

4. You mention that in the Antarctic peninsula, wet deposition dominates and the concentration of trace elements depends on cyclonic activity, which is episodic and seasonally variable. However, you do all the correlation analysis using annual means and I don't think that's representative.

5. One of your conclusions is that “marine derived trace element concentrations are strongly influenced by sea ice concentration and sea surface temperature anomalies”. That is a wrong conclusion, all you have is a correlation analysis, no dynamical or physical explanation to imply causation.

Minor comments:

Line 56: Mineral dust is not a source of aerosols.

Line 84: Location of SST and sea ice changes?

Line 98: Please always use SI units. In the case of clean rooms that would be the ISO 14644-1 standard. A class 100 room is equivalent to an ISO 5 level (10ˆ5 particles per cubic meter). The class 100, 1000, etc. standard has been obsolete for over 15 years, it’s time people move on.

Line 115-119: That method makes no sense to me, although I may just be ignorant of this matter. The standard deviation of the measurements should have no relevance for the detection limit? The instrument could be very precise at medium range, but have a detection limit greater than it’s precision. Or did you mean the average of the blanks plus 3 times the std?

Line 120-125: Where did the samples come from? Did you send frozen pieces of the ice core to Brazil? If so how were they treated in Brazil? Or did you send aliquots from the fraction collector? If so how did you send them? Frozen?

Line 127-129: Briefly mention here why S can be used to count layers. Why did you not use other measurements, such as Ca or Al, for the layer counting?

Line 148 – 152: Where is your dust source? If it’s Oceania are 5 days enough to transport the particles across the Pacific?

Line 173: The regression line in Figure S1 is just ridiculous. Obviously there is no linear relationship between Na and nssS. Please use common sense and don’t blindly

apply methods found in other papers.

Line 202-209: How can you distinguish your calculated excess from the error introduced by crustal Na and oceanic Al? I doubt anything below 10% contribution is significant, once you calculate the calculation uncertainty due to these effects.

Line 214-220: Have you looked at the distributions? Are the elements normally distributed? I doubt it and you cannot use Pearson’s correlation then. Try the Spearman or Kendall correlation instead. And redo the classification of crustal and marine elements.

Line 226-227: It may be best to remove table S3 unless you can address all the comments above.

Line 253: Mean of what? And do you really have a 0.01 pg/g measuring accuracy? Please go through all the text and remove all those decimals.

Line 325-327: How exactly were these classification defined? It sound rather subjective to me, was there an objective criteria? What about South American influence?

Line 333-334: You don’t need to cross the Pacific from South America. The South American contribution would come through the South Atlantic cluster.

Figures:

Figure 2: Have the same sequence of elements in both (a) and (b) plots.

Figure 3: remove “concentrations” after variability

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