

## ***Interactive comment on “21st Century Asian air pollution impacts glacier in northwestern Tibet” by M. Roxana Sierra-Hernández et al.***

### **Anonymous Referee #2**

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Checklist of ACP review criteria:

>Does the paper address relevant scientific questions within the scope of ACP?

Yes.

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

New data yes, otherwise, not really. The overall approach, methods, and interpretation of the findings, including the manner in which the results are graphically presented, are very similar to what was reported in the 2018 paper by the same authors on the older (1992) Gulyia ice core. It is understandable that the methods need to be consistent to ensure a coherent continuity between the older core and the new extension. However I can't help to wonder why the authors did not simply report findings from the two

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cores together in a single paper. There is a great deal of unnecessary duplication of information in this new paper that is not really novel, but a simple repetition of earlier work.

>Are substantial conclusions reached?

Somewhat. This is not the first ice core record of trace element deposition to have been developed from central or East Asia (cf Fig. 4 in Sierra-Hernandez et al., 2018), and it shows a continuation of the same general trend in increasing trace metal deposition in the late 20th century seen in other cores, which generally agrees with what is known of regional emission trends from possible anthropogenic sources. However, the analysis of this set of cores from Gulyia ice cap was, in my view, done with exceptional care and attention to data quality compared to previous studies.

>Are the scientific methods and assumptions valid and clearly outlined?

Yes, except in the discussion of the potential role of the North Atlantic Oscillation (NAO) on the atmospheric transport of trace elements (section 3.2). This part of the paper is weak and unconvincing, and lacks clarity. See specific comments below.

>Are the results sufficient to support the interpretations and conclusions?

Yes, but with the same proviso as above regarding the NAO.

>Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Overall, yes, or adequate references to earlier publications with details are provided. However I have some doubts about the method of calculation of the "Excess" of trace elements in the ice core. See specific comments below.

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

Yes.

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>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?

Yes, but the introduction section repeats much of the same information that was previously given in Sierra-Hernandez et al. (2018), and seems unnecessarily long and wordy to me.

>Is the language fluent and precise?

Yes.

>Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes, but I have some questions about the calculation of "excesses" of TEs, see specific comments below.

>Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or >eliminated?

I recommend shortening the introduction, abbreviating the discussion in section 3.1, and dropping section 3.2 altogether. See specific comments below.

>Are the number and quality of references appropriate?

Yes.

>Is the amount and quality of supplementary material appropriate?

Yes.

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Specific comments:

I reviewed an earlier version of this paper which had been submitted for consideration in another journal. I had made a number of recommendations for improving the paper, many of which were implemented by the authors in this new version. I focus below on some of the recommendations that were not followed, and on other points.

In my previous review, I recommended that since the enrichment factors (EFs) were calculated using Fe as a reference element, the authors should show that Fe concentrations in the overlapping sections of the 2015 and 1992 Gulyia ice cores (i.e., for 1971-1992) are comparable. In the revised version of the paper, the authors indicate (in the Supplement) that "the Al and Fe median concentrations are 0.3  $\mu\text{g/g}$  in both records during the 1971–1991 period in which both TE records overlap." However the data are not actually shown. I recommend again that these data should be presented graphically. This would show the reader if the Fe concentrations in the overlapping core sections vary in agreement. The fact that they have the same median concentrations does not necessarily imply that they do.

L187-189: Results of cluster analyses of geochemical data should be treated with a great deal of caution, as they are very sensitive to data pre-treatment (example: transformations) and the choice of the clustering algorithm. See Templ et al. (2008; Applied Geochemistry 23: 2198–2213). The results presented on Fig. S4 would be more convincing if the authors could show that they can be replicated with different clustering criteria or methods.

On Fig. S1, aluminum (Al) shows consistently negative Excesses (i.e., deficits) in the older Gulyia (1992) ice core, including during the "pre-industrial" interval of reference (1650-1750). This is odd. The concentration of trace metals of crustal origin in environmental matrices (e.g., soils, ice) often have positively skewed probability distributions, but not that of major elements such as Al and Fe. Therefore I would expect the probability distribution of the Al/Fe ratio in the ice to be symmetrical, possibly normal. Hence,

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if the Ex(Al) shown on Fig. S1 were calculated as per equation (2) in the paper, I would also expect that in the part of the core that corresponds to the interval of reference (1650-1750) there should be both positive and negative Ex(Al), depending on whether the Al/Fe ratio measured in any part of the core fell above or below the median Al/Fe value for the whole reference interval. In order for the calculated Al Excess to be consistently negative during this interval of reference, the measured Al/Fe ratios must be consistently lower than the reference median Al/Fe value used in the calculation, which necessarily implies that this value can't actually be the median. Something seems to be wrong here, and it may also affect the calculation of Excesses for other trace elements. Maybe there is something missing in the description of the calculation method ?

Sections 3.1 and Figs. 4-5. The attribution of anthropogenically-derived trace metals deposited in the Gulyia ice core to specific regional sources is largely based on visual "curve matching" of trends between the ice-core composite EF (for Cd, Pb, Zn, and Ni) and in regional emission data. This is fair enough, and I think that there is a good case to be made that the increase in the composite EFs points to dominant sources from East Asia and/or the Indian sub-continent (which is hardly surprising). I am less convinced by the argument offered for the predominance of emissions from specific fossil fuel sources in Pakistan. The observed trend in EFs is probably the result of a mixture of emissions from various regions/sectors. Hence, there could in fact be more than one combination of regional/sector emission sources that could produce the observed trend in EF, but the only such combination that is analyzed in detail is that of emissions from Pakistan (Fig. 5). The argument offered in support of Pakistan would be more convincing if it could be shown that no other combination of regional sources can explain the observed trend in EFs. Ultimately, the "case" for the predominance of Pakistan seems to depend on the apparent "peak" in EFs around 2007, which could match a period of peak emissions in Pakistan at that time (if emission data from this region are to be trusted). Given that the factor analysis attributes only 2 % of the variance in TEs to anthropogenic sources (the rest being associated with crustal sources), the

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authors should refrain from over-interpreting minor features in the TE record. This is not to say that Pakistan may not be an important source of TEs to the Gulyia ice cap (it would be surprising if it were not), but I think that the relative dominance of emissions from this region is overstated.

Section 3.2.: I find the discussion of the possible influence of the NAO on atmospheric trace element deposition on Gulyia ice cap to be weak. First, the purported correspondence between high/low NAO phases and the composite index of trace metal EF on the Gulyia ice cap (Fig. 6) is based on a subjective visual comparison, without any supporting qualitative metrics (e.g., correlation coefficients), and it is, to me, unconvincing. Second, this comparison does not offer a definitive way to discriminate or parse, in a quantitative way, the relative influences of the NAO and of anthropogenic source emissions of trace elements, such that one is left to speculate about which factor(s) were dominant at different times. Thirdly, no explicit mechanism is offered in the text to account for this purported relationship. I am assuming the interpretation is the same as that previously suggested in Sierra-Hernandez et al. (2018), i.e. stronger wintertime NAO => enhanced westerlies => more efficient transport of atmospheric trace elements from distant (European) sources in the west to the Gulyia ice cap. As I pointed out in my previous review, this is at odds with the effects of the NAO on atmospheric flow over the Tibetan Plateau in climatology publications (Mao et al., 2011; doi:10.1016/j.atmosenv.2010.10.020; Han et al. 2008; doi:10.1016/j.atmosenv.2007.12.025). Furthermore, there seems to be no clear or consistent association between predominantly low(high) NAO phases and variations in dust deposition on the Gulyia ice cap, as one might expect if the NAO-westerlies linkage was important for atmospheric particulate matter transport (Fig. 6-7 in Thompson et al., 2018). I had previously suggested that one possible way to verify if a stronger winter/spring NAO phase actually enhances east-west atmospheric transport towards the ice cap would be to compare the mean length of air parcel back-trajectories between years of low and high NAO indices. I can only offer the same suggestion again. Unless this or some other supporting evidence can be offered, I recommend that this

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section be excised altogether from the paper.

Minor suggested corrections:

L32-34: "TEs are also released into the atmosphere by human activities including: 1) the combustion of fossil fuels including coal, oil and its distillates (e.g., gasoline, jet fuel, diesel); 2) industrial processes such as mineral EXTRACTION, and metal production."

L163-164: "Their EF averages increase by ~10 % during 1990–2000, and during 2000–2015 by 75 % (Cd), 35 % (Pb), 30 % (Zn) and 10 % relative to the 1971–1990 period."

What does the 10 % figure refer to ? Ni ?

L172: " MOST of the variance (94%) is explained by both Factor 1 (73 %)...."

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