

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-208

Anonymous Referee #1

Referee comment on "Consistent histories of anthropogenic western European air pollution preserved in different Alpine ice cores" by Anja Eichler et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-208-RC1>, 2022

General comments

In this study the authors present a synthesis of air pollution data from four different High Alpine European ice cores for the last ~250 years. They analyse how representative the single core data are for the common pollution history of the area. They create composite records of 10-year averages for the respective pollution species and calculate "times of emergence" (TOE) when the actual anthropogenic pollution becomes significant.

This is an interesting topic and especially the investigation of the representativity of one ice core record for the atmospheric signal of an entire region is very important and was not sufficiently discussed so far. Also the findings of the different times of emergence of the pollution signals in Europe and the present day conditions, including partly failed abatement measurements, are highly relevant and should be published.

The paper is overall well written. The paragraphs follow a clear structure and are easy to follow. However, the general focus of the study remains a little unclear to me. The title and the statements at the end of the discussion (section 3) paragraphs rather suggest that the investigations of the TOE and the evaluation of the present day situation were the main objectives. On the other hand, the authors also dedicate a good deal of the study to the evaluation of the representativity of a single core record for the whole region. In my opinion, both discussions lack depth and critical evaluation. The paper would definitely benefit from a clarification of the main objective and a more focused approach (methodological or interpretation of pollution history).

Especially the discussion section (3.2-3.4) is sometimes rather vague and needs some revision. I think the discrepancies between the single core records (Fig. 2) and the resulting consequences for the composite record (and the following interpretation) are not discussed and assessed sufficiently (see specific comments).

My other major point of criticism is the methodological approach for averaging in section 2.3. It is not justified why the authors calculate 10-year averages for the single ice core records. Is that indeed necessary to “average out inter-annual-fluctuations”? (L203). If only looking at a 250 year time range this seems to smooth out a lot of variation. Could another averaging window also be sufficient and maybe provide more detailed insights? This needs some statistical evaluation. The reasoning behind the 10-year averaging seems random, remains unclear so far and will have consequences for the following discussion of TOE and pollution records. This section needs to be extended, clarified and methodologically justified.

I have some specific comments, especially about some figures and the respective discussions.

Specific comments

L21: please put “long term” in perspective. For most ice core studies, 250 years would not be considered long term

L21: please specify “several” ice cores

L37f: This statement lacks evaluation. What are the consequences from only four core records corresponding well with the modelled trends? Please elaborate

L51: change “ice cores” to “ice core records”

L66ff: Maybe a problem of wording: “... indicates common source regions for the different Arctic sites...” I think this is a rather generalized and bold statement, especially because in the next sentence you state that this is not true for e.g. Svalbard. Please clarify.

L79f: Reference for huge Himalayan dust deposition?

L87: you could add: Preunkert, S., McConnell, J. R., Hoffmann, H., Legrand, M., Wilson, A. I., Eckhardt, S., et al. (2019). Lead and antimony in basal ice from Col du Dome (French Alps) dated with radiocarbon: A record of pollution during antiquity. *Geophysical Research Letters*, 46, 4953– 4961. <https://doi.org/10.1029/2019GL082641>

L90f: The sentence about the downward trend is somewhat repetitive throughout the manuscript. Please shorten.

L93: Please specify that "annual" concentrations at Colle Gnifetti are also heavily biased towards the summer season because of major wind erosion of winter snow.

L102: which were the "different laboratories"?

L114: Can you add a reference for the atmospheric lifetime of the species? Are they the same for all different pollutants?

L137: please add: "... parts or all of the winter snow cover..."

L138: add per year after w.eq.

L139: Please weaken this statement to "... ice at bedrock can be more than..." and cite additionally: Hoffmann, H., Preunkert, S., Legrand, M., Leinfelder, D., Bohleber, P., Friedrich, R., & Wagenbach, D. (2018). A New Sample Preparation System for Micro-14C Dating of Glacier Ice with a First Application to a High Alpine Ice Core from Colle Gnifetti (Switzerland). *Radiocarbon*, 60(2), 517-533. doi:10.1017/RDC.2017.99

L173: were the IC measurements discrete or quasi-continuous? What was the spatial resolution? Please specify.

L174: Is there a reference for the IUP measurements?

Section 2.2.3: Are there no measurements available for the other cores? Please explain.

L197f: Is this sea salt and mineral dust correction valid for such rather continental sites?

L201: It is unclear what happened after the separate calculation of summer and winter mean concentrations. Were they also evaluated separately? Please extend and clarify.

L203: See general comments. How were the 10-year averages calculated? Just arithmetic mean? Weighted? Please extend and explain.

L205: see general comments "should better reflect" is no sufficient justification for the 10-year averaging. Please assess better and explain reasoning.

L210: The reason for not including the GG records does not convince me. 50 years out of 250 is already 20%. If this is considered too short, the GG core should have been left out completely.

L211: Again, are there no BC /EC records available for the other cores? Clarify.

L219 and Fig. 1: It is really hard to see significant differences between the FLEXPART results for the different locations. Maybe this figure could benefit from a larger zoom on the locations and / or a different colour scheme if the goal is to show the very small large-scale variations?

L221: "Most species" – which ones are not present as sub-micrometer aerosols? Specify.

L229: how exactly was this calculated? Please elaborate.

L234: spatial distribution referring to what region? Please specify

L238: which climate simulations? The whole paragraph 2.5 is rather vague. Please specify.

L243: how do you define long-term-trend? What would happen for a smaller / larger averaging window?

L254f: Please reformulate this sentence. It seems odd if you write "could be explained" when you dismiss this possibility later in the text. Also the west-east gradient seems confusing in this context, when in Fig. 1 you mention that for CDD the source region is extended to the west.

L255: Do you have estimates for the ratio of the summer / winter snow accumulation?

This would be an important information.

L259 and Fig. 3: Does that 50 km MATCH-ECLAIRE model resolution actually make sense if the cores are only separated by ~100 km? Maybe focus on the BIODIV version and don't show the low resolution runs? The gradient only seems to be visible in the low resolution plots, this is not sufficiently discussed.

L262: Again, what is the benefit of the MATCH-ECLAIRE, if it underestimates the altitudes in such way? Please consider focusing this paragraph only on the high-res version.

L272: Please reformulate the first sentence. Is this a conclusion from the fact that the west-east gradient was ruled out?

L282: Given the close spatial proximity but very different accumulation regimes: how do the CG and GG records compare? This would be important to know in the course of the data evaluation.

L284ff: In my opinion this last statement of the paragraph questions the representativity of one single record (at least in total concentration apart from trend) for the region. I think this is contrasting one of the main messages of the paper. This needs further discussion and assessment.

L297f: What about the CG15 peak at about 1920 in Fig. 2? This is also visible in NO₃ and NH₄, If it still shows up in the 10 year average, I would consider it significant enough to be discussed. Please comment.

L326ff: Why should these uncertainties only be relevant in the late 20th century? There is almost a factor of 2 difference between observation and model in the modern section. This needs more critical discussion.

L339f: if analytical uncertainty is the reason, this is not sufficiently reflected in the uncertainty envelopes. Again, there are excursions for CG15 around 1910-1920 and FH ~1860. This needs a better assessment than the speculative comments. Please extend.

L400: if the model emission reductions were too optimistic, could this be corrected and re-evaluated to find a better agreement? Please comments

L408: Typo: Methods of analysis

L408: you could add "to the CG cores" after "similar trend"

L408f: please clarify what you mean by "elevated Pb levels in the earlier..." does this refer to the relative change? the total concentration is lower anyway. The same applies for Cd.

L428f: If these are the locations of the smelters, why is there no larger impact in the CDD cores visible? Please comment

L442f: There is no sufficient answer given to the question of representativity throughout the manuscript. This needs more specific discussion

L449f: This representativity is in the current stage of the study only true for the long-term trends, but not for the absolute concentrations. This needs clarification.

L455ff: Does this mean that CDD is less representative for the region than the other cores? This also needs more critical assessment and discussion.

L458-462: This paragraph also needs some evaluation .It only lists the different TOE. What are the consequences for the present day situation?

L476-L479: Please reformulate this final statement. It is common knowledge that beyond the instrumental era ice cores are the main tool to reconstruct aerosol deposition. What especially is the knowledge gain emerging from this study? Is it representativity of the single records? Or rather pinning down the onsets of pollution? This needs clarification (see also general comment).

Table 1: It would help to add a column with the core length and / or estimated maximum age at the bottom.