The paper reports on the record of the fluorescence signature of the organic matter present in an ice core drilled in East Antarctica and covering the last great climate change having occurred between the last glacial maximum and the Holocene period. Very little is known on the level of organics in ice, especially in the case of Antarctica. Therefore these data are very interesting for researches dedicated to our understanding of the atmospheric budget of the organic matter and its change in response to past climatic conditions. However, major revisions of the manuscript are needed before I can recommend publication of this work in CP.

The first weakness of the manuscript is the use of poorly defined wording rendering difficult (sometimes obscure) the reading of the manuscript.
For instance, I guess that, when saying “OM quality”, you mean “fluorescent signal of the OM” ? Also what is a recalcitrant OM ?

Some abbreviates appear in the text without definition. For instance, what is the PARAFAC model that is already mentioned in the abstract, also please indicate what is the basic of this kind of model ?

The abbreviates C1, C2 and C3: I guess that they refer to component 1 etc (and not to C1 carbone chain etc).

In section 2.3, please define A254 and re-define EEMs here.

Section 2.4: I don’t understand the following sentence “A three component PARAFAC model was generated for the subset of samples by drEEM and the N-way toolbox scripts » : what is « drEEM » and N-way ?, please define.

Concerning units: Line 98 : what is au ?

I will avoid the use of RU for Raman unit (RU is sometimes used for relative unit). Also I am not sure that the readers of CP, specially those working on ice cores, are familiar with this Raman unit ? A few words on that would help (see also my comment on Figure 2).

Introduction, first paragraph (lines 31-446):
This paragraph can be improved significantly, for both the wording and the cited references. Two of your co-authors have a nice expertise on the chemistry of ice cores, they certainly can also help here.
From my side I would suggest to start with an overall sentence: “In addition to its water stable isotope content that provides a proxy record of past temperature (see Dansgaard et al. (1993), for instance), ice archives atmospheric information on trace gases like CO_2 and CH_4 encapsulated in air bubbles and chemical species trapped in the ice lattice. Numerous inorganic species trapped in ice has been used to reconstruct past chemical composition of the atmosphere, its recent change in response to growing human activities as well its past natural variability (see Legrand and Mayewski for a review).”

I here agree with another reviewer of the manuscript that the Nature paper from Wolff and co-workers (2006) is an excellent example that you have to mention of what was done on deep Antarctic ice cores in terms of changing sea-ice dust emission and marine biological productivity over the 8 climatic cycles.

Then focus on what was done on organics saying “In contrast, as reviewed by Legrand et al. (2013), information on the load and composition of the organic matter archived in ice are still very limited. »

I think you can find in this review paper relevant references that can be useful for your introduction. In particular, I suggest to report the work from Amanda Grannas made of the nature of OM in polar ice and those done on the HULIS like content of ice.

Legrand, M., and P. Mayewski, Glaciochemistry of polar ice cores: A review, *Reviews of Geophysics*, 35, 219-243, 1997.

Legrand, M., S. Preunkert, B. Jourdain, J. Guilhermet, X. Fain, I. Alekhina, and J.R. Petit, Water-soluble organic carbon in snow and ice deposited at Alpine, Greenland, and Antarctic sites: A critical review of available data and their atmospheric relevance, *Clim. Past*, 9, 2195-2211, doi:10.5194/cp-9-2195-2013, 2013.

Grannas, A., Shepson, P. B., and Filley, T. R.: Photochemistry and nature of organic matter in Arctic and Antarctic snow, Global Biogeochem. Cy., 18, GB1006, doi:10.1029/2003GB002133, 2004.

Section 2.1.: line 75: WD is not at all the first Antarctic ice record available for comparison with Greenland records. Please modify the text.

Section 2.2: line 86: what is the difference between cracks and fractures ?

Section 2.5: Please write a few sentences explaining why your choice was to show these inorganic species. Note that, as far as I know (and checking your fig 4), I see no reason to use three species (Mn, Sr, and Ca) for dust (except if you have in mind to discuss the ratio between the 3 in view to eventually highlight the source region, which seems not to be the case).

Figure 2: Are there any possibility of estimate from the Raman values how much is the concentration of OM ? Indeed, given the scarcity of data on organics, even an order of magnitude would be welcome here. From that and using a typical conversion factor OM/C you can estimate the TOC or DOC content of ice. Also I am surprised that the spikes shown in the fluorescence intensity during the LGM are not more commented in the text.

Section 4: I feel that this section has to be (at several) places deeply revisited:

Line 184-195: I assume that “Humic-like fluorescent OM” corresponds to Humic like substances observed in the atmosphere of many regions. If correct, did you consider these species as primary emitted (with soil particles for instance) or secondary produced from oxidation of gaseous organic precursors emitted by the continental biosphere (vegetation)? See also the discussion in their presence in ice in the following recent paper.

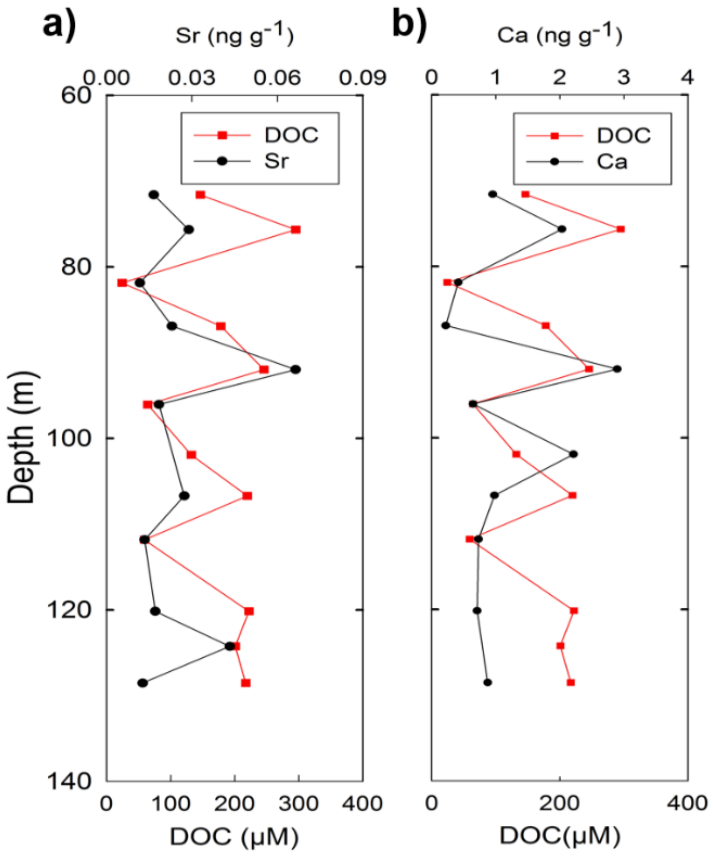
Guilhermet, J., S. Preunkert, D. Voisin, C. Baduel, and M. Legrand, Major 20th century changes of water-soluble HUmic LIke Substances ($HULIS_{WS}$) aerosol over Europe inferred from Alpine ice cores, *J. Geophys. Res. Atmos.*, 118, doi :10.1002/jgrd.50201, 2013.

Section 4.2: Your discussion on change of dust tracers is quite oversimplified and I would recommend you to revisit previous works done on this topic.

Lines 255-265: This discussion is from my point of view rather confusing.

It is incorrect to say that nssS concentrations are used to trace back volcanic eruptions. Only the narrow peaks of nssS are related to volcanic eruptions whereas the background nssS level in Antarctica originates in marine biogenic emissions (please revisit here the paper from Wolff et al., 2006 for instance). Also, I don't think that the wording of the following sentence makes sense "Therefore, volcanic eruptions increase the potential for particles and chemicals to be transported to polar regions and deposited onto ice- sheets." Please modify.

Supplementary material: Following your line 201 on a correlation between DOC and nssCa, I checked the S2 figure (extracted below) that strongly bothers me. Indeed, if the DOC unit you report is correct, DOC levels of this Antarctic ice are as high as 200 μM. If I am right that means 12*200 μg L^{-1} i.e. 2400 ppbC. If correct, please comment with respect to the review of Legrand et al. (CP, 2013). It is very likely that you have a large DOC contamination in this shallow WD core. Also, sorry but I don't see a good correlation in this figure between dust and DOC!!! Please comment.



End of the review