

# ***Interactive comment on “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” by M. Legrand et al.***

## **Anonymous Referee #2**

Received and published: 3 July 2013

In this study existing data on dissolved organic carbon in ice from Antarctica, Greenland, and the Alps are compiled and reviewed along with a few new data. This is an interesting topic especially because the results from the few available studies on organic carbon and seem to be inconsistent. However, I have a few concerns with the manuscript which I would like to be addressed before publication.

The manuscript is a mixture of a review with few new data. Some of the discussions/conclusions, for example the long-term trends from pre-industrial to recent years for Greenland and the Alps, are not well supported by the presented data, but are

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based on other publications. For the interpretation of temporal trends the data basis in this manuscript seems to be too small. Even at one single site I expect to see year-to-year fluctuations of the OC concentration due to variations in source strengths, transport, precipitation etc. With all the problems related to the different techniques applied, any statements on temporal trends are weakly supported by the data. Therefore, I suggest omitting this paragraph.

For a critical review of existing data there is not sufficient discussion of the methods used by other groups. Instead only the UV-oxidation method is presented in detail. TOC data obtained with catalytic combustion are generally higher which is here attributed to sample contamination due to the use of PE bags. This is contradicting the original work in which blanks are given and corrected for. I wonder if part of the discrepancy is due to the different technique and the different component (DOC versus TOC). Is UV oxidation as efficient as catalytic combustion? It is discussed in detail that UV oxidation may lead to a 10% overestimation because of oxidation of particulate OC when not filtering. The efficiency was tested on phthalate, mono- and dicarboxylic acids, and other C2 and C3 species. However, more than 50% of the OC in the snow samples is not identified, probably including compounds (microorganisms?) which are more resistant to UV oxidation.

Part of the difference in Antarctica might also be explained by the distance to the ocean, which is a source of organic carbon. I therefore do not expect a typical OC concentration in Antarctica, as stated on page 2374, top.

It would be helpful to define at the beginning the various carbon fractions (DOC, TOC, particulate OC, WinOC, OC\* etc.) and to use it strictly throughout the manuscript. For example the term “organic aerosol fraction” page 2376, last line, is not precise.

It is not clear if the DOC values presented include HCHO-C and monoacid-C? Are these volatile compounds not lost during sparging of inorganic carbon?

The seasonality of DOC shown in Fig. 1 is not at all clear. There is a peak in 1300 AD

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(forest fire) and a small one in 1298 AD, but none in 1299 AD.

Minor comments

Page 2360, line 6: Gabrieli et al.

2361, lines 13-14: Give more details: Power of UV lamp, external or internal irradiation, material of extraction set-up.

2369, lines 21-24: Give reference for this statement.

2373, line 11, it can be supposed. . . (not concluded)

Caption Fig. 1: sodium maximum, DOC (instead of OC) concentrations

Caption Fig. 2: DOC (instead of OC) concentrations

Caption Fig. 3 and 4: MSA instead of CH<sub>3</sub>SO<sub>3</sub><sup>-</sup> (consistent with legend)

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9, C1355–C1357, 2013

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