

Interactive comment on “A 21,000 year record of organic matter quality in the WAIS Divide ice core” by Juliana D’Andrilli et al.

Anonymous Referee #2

Received and published: 4 January 2017

General comments:

The authors analyzed over one thousand samples for organic matter (OM) from the Last Glacial Maximum (LGM) until the uppermost section of the WAIS ice core. Through parallel factor multivariate analysis (PARAFAC) the authors determine three main components, where the third component (C3) comprises labile OM from terrestrial plants and soil and is only present during the Holocene. The other components (C1 and C2) are present from the LGM onwards. The authors compare their OM results to variations in terrestrial crustal dust input (nssCa, Sr, and Mn) to examine if major glacial-interglacial differences in transport and/or aridity affect the OM composition.

This research presents one of the first continuous studies of OM in an ice core over glacial-interglacial timescales. While the increased input of OM from terrestrial sources

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during the Holocene is not surprising, this research quantifies this change. This paper can therefore serve as a foundation for many future studies investigating OM in polar ice cores. The authors understate any possible changes in atmospheric deposition and/or possible in-situ processes that may affect their results, but the current study may not have sufficient information for determining these changes.

Specific comments:

You state that “Ice core studies rely on the paradigm that atmospheric deposition is the sole mechanism for specific gases and materials to become trapped in the ice” (Lines 47- 48) yet it is unclear if you apply this paradigm to your work. If you do not allow even a remote chance for in-situ production of this organic matter, then please explicitly state so in your work. In lines 179-183 you mention the possibility of in situ OM processing but then do not discuss if such transformation could affect the samples in this work. You mention that tryptophan-like fluorescence in C2 may derive from microorganisms, and then mention that the presence of microorganisms may result in in situ OM processing, but step back from linking the two aspects. In the following paragraph you then mention that Holocene terrestrial plants and soils are the likely source of the C3 OM yet do not mention if in situ processes may affect this material or if you ascribe this material to be solely brought in via atmospheric transport. Please clarify your stance on the source and possible post-depositional processes affecting the samples as both aspects are essential to your interpretations of the data.

Please check that all figures are cited in the text. In lines 128-144 you mention Supplemental Figures 1a-b. You do not refer to Figure 2 in the text. As you refer to the Supplemental Figures but not Figure 2, then perhaps their roles should be reversed with the current Figure 2 included in the Supplementary Information and vice versa.

The left bars and corresponding explanation in the caption of Figure 3 are confusing. In the article text you explicitly state that C3 only occurs during the Holocene. As most readers will likely first look at the figures and captions before reading the article, it

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bears mentioning in the caption that C3 is specific to the Holocene. Demonstrating the variation in C2 by various time periods (LGM, LD and Holocene) is useful but then makes the reader immediately wonder what is the variation in C1 between climate periods. If there is no substantial variation between time periods for C1, please mention this fact in the caption.

This sentence is confusing (Lines 227-229): “During the LGM, tundra ecosystems covered more expansive areas of the Earth (Ciais et al., 2012) and while C was cycling, productivity in the environment differed from warmer climates (Ciais et al., 2012 and references within)”. Do you mean due to the colder temperatures and increased ice cover and tundra during the LGM, that net C productivity was less than in the other warmer times periods of this study?

The final conclusion overstates the results of the study. To state that labile, microbially derived OM “were the greatest contributors to Earth’s atmospheric composition throughout history” is not correct. Labile OM may have been the greatest contributor of total OM in the atmosphere over the time periods covered in this paper, but this situation may not be the case before the LGM. In addition, in this sentence it is not clear what aspect of the “Earth’s atmospheric composition” that you mean.

Technical corrections:

Line 16 = Define PARAFAC as this is the first time that you use this acronym.

Line 48: Place “idea” after “that” in “Extending that to include”.

Line 222: Remove the comma after “LGM”.

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