Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-55-RC1, 2018

Climate of the Past Discussions

Interactive comment on "Holocene Proxy Climate Series Should Account for the Site's Elevation, the Variable's Sensitivity to Elevation History and Time-lagged Effects: Three Examples" *by* David A. Fisher

Anonymous Referee #1

Received and published: 11 June 2018

Introduction This paper makes an attempt to bring arguments for an in-depth consideration of any site's position (elevation, elevation history, distance from ocean) when reconstructing past climates using proxies from the respective site. The first chapters discuss these issues in general, and in the final one, the 4.2 ka event as seen in the Greenland and Canadian ice cores is briefly examined. Both the title and abstract are promising a great debate, however, the main text fails to rise to the expectations – at some points, it's building on fallacies of the same order as the ones that are being discussed (see the detailed comments below). Further, the relevance of the paper for the

"4.2 ka BP event" special issue is debatable, as it only marginally touches on this and the results are nothing new – the two figures are reused from one of the he author's previous articles (Fisher, 2011, figs. 6 and 7b). While the hypothesis of climatic events "seen" in NW Canada are triggered by changes happening in the ocean ∼1200 years before is intriguing and worthwhile considering, it is nothing of new – it has been presented (and better explained) in Fisher (2011). The paper would have befitted from an expansion of the 2011 article, by discussing in more detail the mechanisms, implications and responses on a much wider area. And, as a side note, the text of this final chapter reads in places like conference notes, rather than an article.

Detailed comments Chpater 1 Introduction The introduction could do with a stronger discussion of why the issues raised by the author could be problematic. For instance, except for a few very special cases (e.g. Greenland Ice Sheet), little places experienced a dramatic change in elevation throughout the Holocene that could have affected the stable isotope composition of precipitation feeding sedimentary archives that could provide climate proxies. I would expect such changes to act on longer time-scales (100,000s of years).

Chapter 2 While elevation is important when studying stable isotopes as proxies of past climate changes, it is equally (or more, actually) important to consider not the same isotopes (oxygen, in this case) but the same climatic variable. If the same isotopes are to be considered, it is important that they are measured in the same type of archive – here, two records are from ice cores, and two from lakes. Attempting to compare the absolute magnitude of change is wrong – except for (a limited) diffusion-induced fractionation, the ice cores preserve the original stable isotope composition of snow (e.g. precipitation), while the d18O in authigenic lake calcite records both the stable isotope composition of lake water (and hence of precipitation and post-precipitation processes) and lake water temperature. As such, the original d18O in precipitation, while preserved to a certain extent in the ice cores, it is not preserved in lake d18O. More so, the water of Marcella Lake (studied in this article) undergoes strong evapo-

ration, and as such, the stable isotope composition of water (and further of calcite) is not an accurate reflection of the d18O in precipitation. While the idea of comparing d18O on an elevation gradient is correct (and could be important for palaeoclimatic studies), the way it is done here neglects the post-depositional processes and hence the interpretations are meaningless. I suggest a different approach: rather than using the absolute values, the author could use the relative changes against the long-term mean, i.e., calculate percentages of changes. Perhaps a 5 ‰ change at 5000 m asl, based on ice core d18O is larger than a 1 ‰ change at 1500 m asl, based on lake calcite d18O, but both represent a 10 % change on the Holocene scale – i.e., they are equal. Perhaps. Or maybe not, but it would be more meaningful.

Chapter 3 This issue has been discussed in detail by Vinther et al (2008) who proposed corrections and reconstructed the history of GIS elevation, I don't see what this chapter brings new. Again, it seems to be the expansion of some presentation notes.

Chapter 3 See my comments in the introductory part of the paper.

Conclusions Overall, while some of the hypotheses in this paper are worthwhile discussing, the paper fails to do so. The introduction should be more detailed, and the case should be made stronger by bringing examples where not considering the issues discussed here let to wrong interpretations. The discussion of the d18O change across elevation should consider the type of the sedimentary archive and the climatic variable reconstructed, as well as the syn- and post-depositional history of O isotopes. Part 3 is just an overview of the issues addressed by Vinther et al (2008) and the final discussion on the 4.2 ka event is reloaded from fisher (2011). As such, I cannot recommend publication of this paper.

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