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Climate of the Past Discussions

Interactive comment on "Precipitation and ice core δD-δ^{18}O line slopes and their climatological significance" *by* Ben G. Kopec et al.

Anonymous Referee #1

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The manuscript submitted by Kopec and others deals with the relationship between d18O and dD in Greenland with new data from the Owen ice core drilled at Summit and covering the period 1977 to 2010. The main message covey by this manuscript is a discussion of the slope between dD and d18O with difference in summer and winter that is attributed to an important contribution of surface sublimation in Greenland to the precipitation at Summit in summer. The authors also propose a way to link the slope to a budget of sublimation vs precipitation amount.

I can not support the publication of such manuscript for many reasons given below: - The authors can not ignore all the recent literature on the d18O – d-excess in surface snow and shallow firn cores showing different results that those presented here. As an example, Steen-Larsen et al. (2011) did a very detailed analysis of d18O – d-excess

variations on a shallow ice core at NEEM. This study was followed by the manuscript of Masson-Delmotte, Steen-Larsen et al. (2015) with much more data. In these two manuscripts, the link between d18O and d-excess is clearly different from what is presented in the present paper. The interpretation is thus different as well, with an important contribution of marine source evaporation in the whole d-excess signal. I don't challenge the measurements performed in the present study since the water isotopic measurements are routine work but it is not correct to present new data contradicting previous recent ones in ignoring this work. This is particularly problematic since the authors propose an interpretation by using the global sublimation flux over Greenland and not any regional estimate (or calculated using backtrajectories for example) so that there is no reason why the explanation proposed in the present study should not be valid for another Greenland site. - In the same line, Steen-Larsen, Bonne and others (see some references at the end of the review) have largely studied the imprint of evaporation over the ocean and sublimation over the snow in Greenland on the d18O, dD and hence d-excess signals both with monitoring of the water vapor isotopic composition and with modelling approaches including water isotopes. Again, the authors does not quote any of these studies and only quote for sublimation some older papers that are even not listed in the reference list (Moser and Stichler, 1974; Stichler et al., 2001). - In addition to the recent literature, older papers are also fully ignored such as the study of Hoffmann et al. (2001) presenting a fully different interpretation of the recent d-excess signal at GRIP, i.e. at Summit, and a different signal too. - The dating of the Owen ice core is not described sufficiently while the whole analysis is dependent on this dating. A whole section should be devoted to this aspect showing the chemical concentrations, how they are used to date the ice. In the present manuscript, this section is not robust enough to support the conclusion. - I am very concerned about the way diffusion is treated in this paper. Even if we consider the simple diffusion model of Johnsen correct, it is not used in the right way here. Indeed, in the initial paper by Johnsen et al. (2000), it is stated on section 2.2.3 (p. 171) that an artificial signal of d-excess is created by diffusion and this is observed in the figure 4 of this paper. In

other words, the diffusion does not only play a role in the amplitude of the d-excess signal as mentioned and calculated in this study but also on the phasing between the d18O and d-excess signals. Steen-Larsen et al. (2013) used the Johnsen model and corrected then for a phasing between d-excess and d18O on the NEEM shallow ice core. I am very surprised that the authors do fully ignore this effect which probably fully biases their analysis and interpretation. I am also very surprised that the authors only show the raw series of d18O and d-excess and never the diffusion corrected series.

Summarizing, I have serious doubts on the robustness of the dating and diffusion correction of the series presented here to follow the interpretation proposed. Moreover, the ignorance of a rich and documented literature on the subject limits the scientific interest of the present study for a large community working on water isotopes in the high latitudes of the northern hemisphere.

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