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Interactive comment on "Estimation and calibration of the water isotope differential diffusion length in ice core records" *by* G. van der Wel et al.

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This paper makes valuable progress on the use of diffusion of water isotopologues as a paleothermometer. The paper is very good, and certainly deserving of publication in The Cryosphere. I have some concerns, however. There are a number of statements made in the paper that are not well-supported. The paper should be re-reviewed after revision.

1) I concur with the first comment that the equations should be more fully developed in the paper.

2) On page '930', it states that "these common factors cancel out to a large extent...".

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I think this is incorrect. The point of Johnsen et al.'s (2000) work is that the various factors such as tortuosity will cancel out completely in the differential diffusion. If this is incorrect, the authors should clarify: what does not cancel out?

3) Also on page '930', it is said that the correlation of the two isotopes decreases with diffusion. This is only true for some timescales. Johnsen et al. 2000 showed that for seasonal timescales, the correlation increases, because the initially out-of-phase deuterium excess signal becomes in phase. This should be clarified.

4) A general comment regarding impurity content: The work of Frietag et al is cited, and assumed to be correct. Yet other studies have shown no such relationship (see Buizert et al. 2015, in CP). This suggests that the apparent affect of Ca on densification is an artifact. The authors need to acknowledge that the question of impurity affecting densification is not settled, and to discuss the implications for their diffusion results. My impression is that this is a correction to the H-L model that is really not very important.

5) Page 938: The biggest problem with the paper is the attempt to examine whether the initial signal is independent of frequency. The authors use precipitation data from the GNIP database, look at the spectra, and note that it is not white. From there, they argue that one cannot assume that the initial spectrum of the isotope ratios is white. This is probably correct, but only in a very strict sense, and I think it is misleading. In fact, Johnsen argued that the spectrum of the isotope ratios in polar firn was essentially white, in spite of the spectra of temperature and other relevant meterological variables not being white. He proposed that this was due deposition noise, which tends to whiten the signal. Furthermore, the question of "redness" depends very much on the timescale being considered. The relevant climate variables are red on an multiannual to decadal and longer timescale, but rather white on shorter timscales. The diffusion affects the signal – at moderate and high snow-accumulation sites – mostly at the high frequencies, where the initial signa is white. Gkinis et al. (2015) did an extensive set of tests in which they examine the degree to which the redness of the climate signal actually matters, and concluded that it does not significantly affect the results. Having

suggested these tests to them, I am familiar with what they did, and I believe that they are correct, for all but the lowest accumulation-rate locations (e.g. perhaps Vostok).

Another point is that it does not actually matter if the initial spectrum is white, if one is interested in relative temperature change through time, as long as temperature changes are calculated relative to the intial spectrum at the surface.

In short, I believe it is incorrect to say (as the authors do) that "the assumption of an initially white spectrum of water isotopes in precipitation should not be made."

6) A major conclusion of this paper seems to be that "the estimation of the differential diffusion length is much more reliable than the estimation of the individual diffusion lengths is". This must be true, in principle, as Johnsen showed. But I am not convinced that the authors have demonstrated that it is true in practice. Much attention is paid to the absolute accuracy of the temperature reconstruction, but in general we are not interested in absolute accuracy, but relative temperature change (e.g., working out the size of the glacial-interglacial temperature change). In that case, it appears to me that the differential diffusion calculation does not do better than the single-isotope diffusion. At least, I do not think that the authors have demonstrated this. There are more assumptions that need to be made with the single-isotope diffusion, but there are problems with differential diffusion as well. In particular, the fractionation factors at cold temperatures are not yet well enough known, particularly for deuterium.

In summary, I think the authors are too quick to dismiss the "single diffusion" calculation. I would like to see a calculation of the *relative* temperature change, based on 1) d18O, 2) dD, and 3) both. Is the answer actually meaningfully different between these? If so, why?

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