Review of acp-2012-835. “Continuous monitoring….. Ice Sheet” by H.C. Steen-Larsen.

This ms reports on a new and very promising use of newly available portable (cheap) devices for measuring stable isotopes of water in real time. It most certainly should be “published” but with some small-ish changes. The mysterious episodes of very large D-excess are speculated about, but the explanation is still pending I think and they should possibly consider a wider range of processes. In fact maybe the authors should just make a list in this ms of possible explanations but promise a dedicated paper about the interpretations for later. There might be too much to cover in one paper. The report they have given on the data and how it was reached and how good it was in a way plenty for one paper. They can easily write more interpretive papers arising of this new density and detail of isotope data.

I find that there some gaps in the references and suggest what they are.

There are some things I think should be added or changed and will list these in reference to line numbers.

Line 36 they claim “intra-seasonal” but the study interval is about 70 days.

Line 57 The reference list about isotopic composition should include some potentially useful papers by Cuffey, Kavanaugh, and Fisher. Missing refs include , (Kavanaugh and Cuffey, 2003, Global Biogeochemical Cycles), (Fisher , 1990, Annals of Glaciology),(Fisher , 1992, Cold Regions Scence and Technology), (Fisher, 1991, Tellus 43B). . In particular some these offer processes that might explain the very big D-excesses, eg (Fisher,1992) and (Fisher,1991). The latter is wholly about D-excess and shows the importance of water vapor supersaturation history and of cloud type (water droplet or ice crystal) as a function of cloud temperature.

Line 58-59 There is mention of post depositional wind scouring and the effect on stable isotope values, but no references. Much has been written about this process by Koerner and Fisher and the refs should be included in the ms.

Lines 107-109. The effects of Kinetic fractionation on D-excess do not reference the original paper and very clear (Merlivat and Jouzel, 1979). The (Fisher,1991) also has a lot to say about kinetic fractionation over a distributed source-set.

Line 161 and others. “Relative humidity“ is used often in this ms but it is not made clear if it is relative to water (flat surface) or to ice. The Campbell off-the-shelve instrument probably gives out RH relative to water so it is instructive to convert these to being wrt ice. At the least it has to be stated what it is “wrt water or ice ?”.

Line 170 “climatologically mean” huh ?

Line 200 It is good to use the LMDZiso model with built-in water isotopes but as it is there is very little info about the input parameters for the isotope part. Details about the

humidity and windiness of the source input areas and details about the temperature of the clouds shift from being water droplet to ice crystal clouds for example. This could be done maybe in a small table, or maybe by referencing to run parameters. I would prefer the small table. The statement in Lines 434-440 is quite specific but since there is little info about the model runs (especially wrt isotope part) one does not know what could be wrong in the model run and what process is responsible for the mismatch.

Line 222 “measurements” drop “s”.

Line 268-270 The issue of tube length seems important enough to me, to actually display some data that demonstrates the stated insensitivity to tube length.

Line 288 IRMS, OK we are all in the same game, but I think for others acronyms should be explained once or twice.

Line 375 Table 2. It might be worth writing in words “mean” and “Standard Deviation”.

Lines 404-408 and Figure 3. There is some confusion made by the legend in figure 3. The coloured dots in the legend actually refer the lines with those colours , not the dots. And the dot colours in the main text refer to something slightly different. Suggest making the figure legend consistent with the main text. Of course the content of it all is very nice.

Lines 420-423 There is discussion of the earlier reported work (Steen-Larsen, 2011) about the isotopes measured in the firn. It would be useful to have a small summary table of the means, etc for the O18,D and d from this work,,, and also include the same from this. The differences in D vs O18 slopes discussed also might be temporal and not process related because the time interval involved were not the same.

Lines 493-500 The author’s are right to spend time on these large episodes of large D-excess. They are very odd and clearly quite real. Back trajectory tracing is a great idea also, but they should mention a couple of points about its limitations. Most back trajectories go back 5 days or so, but the half life of a water molecule once thrown into the air is more like 10-ish days. This implies that the back trajectory water delivery model (which starts at zero 5 days in the past) is missing at least ½ the water vapor that eventually ends up at the site. This is a general critique of the back trace method , not this papers utilization of it. I have read many papers that have done exactly the same as the authors here (some of them had my name on them). It is worth keeping in mind I think, maybe even mentioning.

Line 488-9 The sentence A slight ….humidity is seen.” I could not actually see it in the figure.

Line 502-03. There are a number of very significant processes that have a large effect on d that are not in the list; supersaturation history of the air mass once it gets into the colder regions as well as the temperature at which the clouds switch from water droplet to

ice crystal clouds. The latter could be a function of air cleaness. Have a look at that (Fisher 1991) paper.

Line 547 In giving correlation coefficients etc it is not always completely clear what two series are being compared.

Line 550-553 In talking about the proportions [or fractions] of moisture from various sources one should actually present the percentages in print or in a little figure.