

The authors thank the anonymous reviewer and David Fisher very much for the detailed review and good feedback, which we have commented on below using red fonts. We also likewise thank the editor Thomas Röckmann for good suggestions to improve the manuscript.

The response is structured in the following way: First the review by David Fisher is commented on and then the review by the anonymous reviewer is commented on.

Review by David Fisher:

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.

We have addressed the concern and added 1) statements pointing to the fact that we do not currently have a full understanding of the physics behind the d-excess signal but that we merely suggest a hypothesis here 2) statements on other causes of high d-excess than the presented hypothesis. 3) that future papers on different aspects are being prepared

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.

We think that the use of “intra-seasonal” here is correct since we do cover nearly all of the summer months in Greenland from mid may to beginning of august depending on year to year.

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 Science 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.

We have as suggested added the references both to the current paragraph and the previous paragraph dealing with the condensation history of the air parcel.

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.

The reference has been added.

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.

The references have been added.

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 ?”.

Yes we are aware of this. The correction is performed such that the RH is with respect to ice.

This is now specified in the text.

Line 170 “climatologically mean” huh ?

We have removed the word “climatological”. What we meant was the mean value of the observed variability.

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.

We do not agree with adding a table – but we agree that we should give more information about the model parameters. We have therefore added specific information about the formation of snow crystals

“

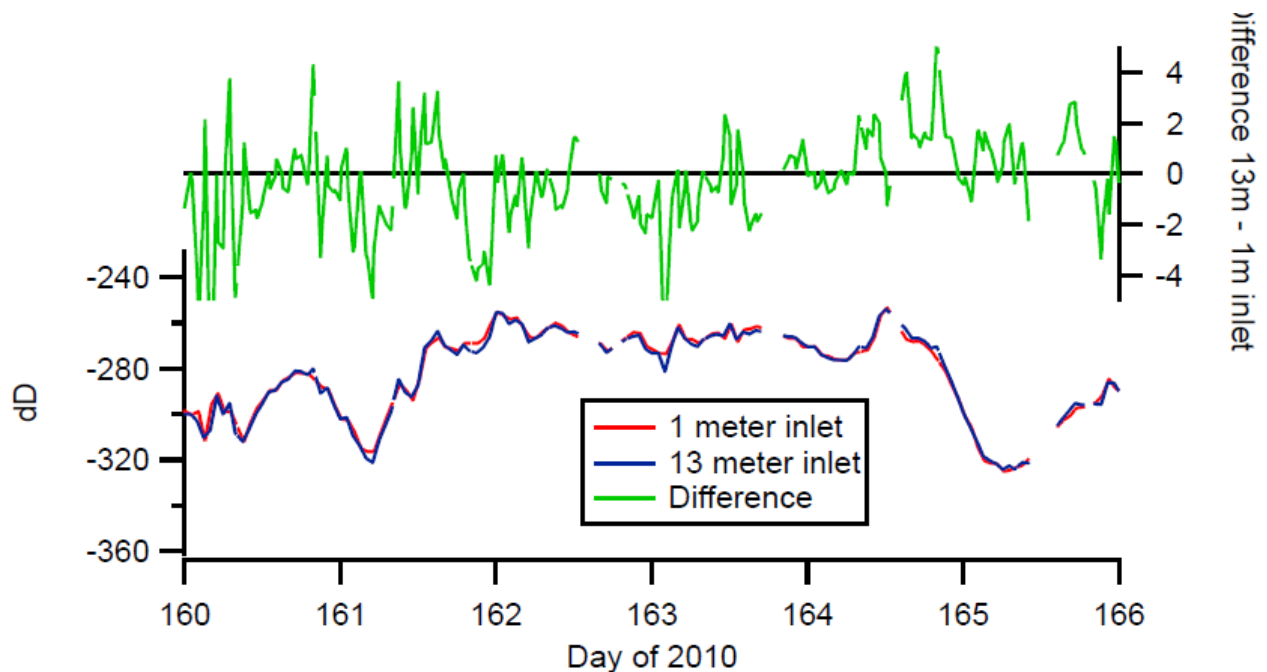
The super saturation (Jouzel+Merlivat) is given by $S = 1.00 - 0.004T$. The fractionation coefficient between 0C and -10C is linear combination of the fractionation coefficient for liquid and for ice. Below -10C only the fractionation coefficient for ice is used. “

Line 222 “measurements” drop “s”.

Corrected

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.

We do not find that it benefits the paper to make a lengthy analysis of this. Instead we have below here attached a graph showing the isotopic composition of the vapor at 1 meter and 13 meter and the difference. We have chosen a period where no diurnal variability is to be found in the humidity or dD. This is a period of occurrence of precipitation and no vertical structure is expected to be found. It can be clearly seen in the figure that no systematic discrepancy between line 1 meter and line 13 meter is to be seen. Only noise in the measurements is seen. We therefore conclude that the tube length do not influence our measurements.



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

Yes – we will do our best to correct this through the paper.

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

This has been corrected.

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.

We have checked all figures and legends to make sure the match. Thank you.

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.

Yes we agree that this might just be a coincidence when correlations exist.

We have added a table with the different slopes to ease the overview for the reader

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 paper's 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.

We agree that this might not be the best. On the same side who says that simulated trajectory beyond 5 days are reliable enough to support using them. We have included statements that we do not know the origins of back trajectories beyond 5 days.

Line 488-9 The sentence "A slight ...humidity is seen." I could not actually see it in the figure. We have removed the sentence.

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 cleanliness. Have a look at that (Fisher 1991) paper.

We have changed the original formulation "the condition during snow formation" to specify the different factors "the cleanliness of the air, the history of the super saturation in the cloud, and the temperature at which snow crystals start to form"

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

We have for one sentence pointed out that it is the comparison between observation and model, which the correlation coefficient is found for. In general is the parameter for which the correlation coefficient is found indicated clearly.

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.

Many uncertainties are related to the estimation of fraction of Arctic moisture. Instead of giving a percentage we instead use it as an indication of Arctic moisture only.

Authors' response to anonymous reviewer:

For the paper in discussion we already corrected it in according to part of the suggestions. These corrections are shown in green. The new comments to the review is written with red.

- 1) L45-47 "Our data show..." Then sentence is too general and unspecific for an abstract. This is rather for an proposal.

This has been removed from the abstract as suggested.

- 2) L 167 "with a large fraction". What does "large" quantitatively mean?

We have included the numbers (2.5 to 4.5 times accumulation JJA compared to DJF) based on Steen-Larsen et al 2011.

- 3) L417-425 Could it be that the discussed differences of d_{18O}/dD slopes

between precip. and vapour are just interannual variability? What is the interannual variability deduced from snow pits or ice cores concerning the $d18O/dD$ slopes?

This is a valid point and we can with the data at hand not rule this out. However we find support for our conclusion in the fact that the slope for the vapor $d18O/dD$ is comparable between 2008 and 2010 and the fact that the slope for the precipitation for 2008 is comparable to the model LMDZiso output.

It is difficult to estimate the interannual variability from snow pits and ice cores since interstitial firn diffusion remove any higher frequency signal.

4. 4) L565-569 The authors discuss only the parameterization of “real” physical processes (RH at the surface, boundary layer physics) in order to explain the failure of the LMDZ model in simulating the deuterium excess excursions. However the isotopic physics of the excess itself is highly parameterized. It would be indeed a major breakthrough if any problem in simulated isotope signals could be linked to “real” problems of the model’s hydrological cycle. However, unfortunately all kinetic processes of the water isotopes are parameterized and tuned in order to get reasonable climatological results. This should be added into the discussion and the conclusions on what can be learned from the model/data comparison here should be reformulated to my opinion.

We agree with the reviewer that the failure of LMDZ can be explained perhaps not only by a failure in the parameterization of creating high d-excess vapor in the first place at the sea ice margin but could also be explained by a failure in the parameterization of the transport of moisture inside the model or the formation of snow crystals. As suggested we have added the following section to the end of the discussions since we find it is outside the scope of the paper to study this in details and we already plan on a paper only on this issue using several models and data from subsequent years.

We add the sentences

“We cannot exclude that the lack in simulating the high d-excess variability are caused by a wrong parameterization of the processes coupled to the transport of moisture or snow crystal formation in the modeled hydrological cycle. It is outside the scope of this paper to study these processes in more details. However we plan to undertake a detailed study using several isotope enabled GCMs and water vapor isotope measurements from subsequent seasons to investigate especially the parameterization of moisture transport from subtropics to Arctic regions.”

5. 5) L571ff I accept that the discussion of the link between AO and the deuterium excess is at this stage still preliminary. However since you have a model available with long simulations it would be logical to have a look if you can find such relationships there.

As pointed out by the reviewer this discussion is completely a hypothesis – we agree on this. The issue with the request of the reviewer is that we argue in the paper that the LMDZiso model is not able to recreate the observed d-excess variability because lack of correct physical parameterization for evaporation in the Arctic. Therefore it is not optimal to use the LMDZiso to see if relation between the AO anomaly and the d-excess in ice cores exists. This was actually tried in the paper by Steen-Larsen et al 2011 where both REMO and LMDZiso d-excess was correlated against AO winter anomaly. No correlation was found. Not published data also showed that no correlation exists between annual AO anomaly and the d-excess of REMO and LMDZiso. We also tried using daily water vapor d-excess model values from LMDZiso and

compared it with daily AO index values. Neither when removing seasonal variations from the d-excess and comparing with dD (with and without seasonal variation) and with AO index (with and without seasonal variations) did any strong correlation nor indications show that modeled d-excess was coupled with AO index.