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Interactive comment on "Bipolar volcanic synchronization of abrupt climate change in Greenland and Antarctic ice cores during the last glacial period" *by* Anders Svensson et al.

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The paper presents the first comprehensive attempt to link ice core from both hemispheres on a common timescale at high resolution, based on volcanic matching in addition to the now "traditional" method of using gas records (e.g. methane).

I find the paper very well written, and the analysis is convincing. I wish to congratulate the authors on a very very important contribution to ice core dating, and to our understanding of abrupt climate change.

I have three concerns.

First, citations for data are not consistent, and are incorrect for those data sets that I am familiar with. This should be corrected.

Second, the discussion of the relationship between deuterium excess and oxygen 18 (d18O) is confusing; I think those who have worked closely with these data (including me) will understand the arguments, but others will not.

Third, overall, I think the paper is written for an audience that already knows all the issues very well, but it will be difficult to follow for those that are not already in the ice core research community. Following are my corrections and suggestions on each point.

1. Citations:

A. Reference is made to both Fudge et al. 2013 and WAIS Divide Project Members, 2013, which are the same paper. Reference is also made to Buizert et al., 2015, and WAIS Divide Project Members, 2015, which are also the same paper. The correct citations are the WAIS Divide Project Members, 2013 and WAIS Divide Project Members, 2015. Fudge et al. 2013 and Buizert et al. 2015 should not be used. This was agreed upon by the WAIS community at the time those papers were written.

Note that there is a different Buizert et al., 2015 (https://doi.org/10.5194/cp-11-153-2015) which should be cited when discussing the WAIS Divide timescale, but not the synchronization work nor the isotope data. This is not the same paper as WAIS Divide Project Members 2015.

B. Several of the citations to data are wrong. Please correct these both in the main text and in the Supplement Table. I am sure it would be appreciated by all those who produced the data if the original works were cited.

i) For WAIS Divide sulfate and conductivity, the references are WAIS Divide Project Members 2013, and Sigl et al., 2016. (As noted above, Fudge et al., 2013 is not a correct citation.)

ii) For GISP2, the original reference is Grootes et al., 1993, not Stuiver and Grootes, 2000.

iii) The d18O and deuterium excess data for WAIS Divide is ascribed to Buizert et al. 2018 in various places. This is incorrect. (For example, line 167.)

The correct citations for the WAIS Divide d18O (not dxs) are WAIS Divide Project Members 2013 and Steig et al,. 2013.

The correct citation for WAIS Divide dxs is Markle et al. 2017. This is the sole reference that should be used.

iv) I encourage the authors to double-check references for Dome C, etc. that may also be incorrect.

2. In general, I find the discussion of the relationship between d18O and dxs incomplete and confusing.

A. In the abstract, you write that "During abrupt transitions, we find more coherent Antarctic water isotopic signals (d18 O and deuterium excess) than was obtained from previous gas-based synchronizations." I don't understand this statement. You find that the phase relationship between dxs and d18O is shorter than was found by Markle et al. 2017, and later by Buizert et al., 2018. But you do not show that the records are more coherent. (If you do find greater *coherence*, this is interesting but would require further analysis).

B. Also in the abstract, you say that

"The time difference between Antarctic signals in deuterium excess and d18O, which is less sensitive to synchronization errors, suggests an Antarctic d18O lag of 152 $\pm$ 37 years."

For those not familiar with this subject, it is not clear what "an Antarctic d18O lag" refers to. This is the lag between d18O and dxs, both in Antarctica. I think what you

are trying to say is that because dxs is in phase with Greenland d18O, then the phase lag between d18O and dxs in Antarctica provides an independent estimate of the phase between d18O in Antarctica and Greenland d18O.

This has to be spelled out or no one will understand it!

c. Throughout the paper, too little credit is given to the first paper (Markle et al. 2017) that showed how the lag between dxs and d18O in Antarctica is connected with the lag between d18O in Greenland and d18O in Antarctica. Prior to that, dating quality was insufficient to make this argument. Buizert et al., 2018 did not make this discovery; in that paper, we extended the findings of Markle et al., to other Antarctic ice cores.

Markle is cited at the moment only for suggesting that: "Antarctic warming response to the Greenland warming is likely to be associated with fast atmospheric changes", and "It was suggested that the gradual dln trends before and after the transition follow the gradual source-water sea-surface-temperature trends of the SH via the bipolar seesaw." Those things are true, but were not the main subject of Markle et al.!

To give credit where it is due, I would suggest the following rewrite.

Replace the following:

Besides d18O, we also stack records of Antarctic deuterium excess using the logarithmic definition (dln) introduced by (Uemura et al., 2012). Previous work has found d ln to abruptly increase (decrease) in synchrony with the onset (termination) of GIs at multiple Antarctic sites (Buizert et al., 2018; Markle et al., 2017; Masson-Delmotte et al., 2010), which has been attributed to shifts of the Southern Hemisphere (SH) subpolar jet and westerly winds (e.g. Schmidt et al. (2007)).

With

Besides d18O, we also stack records of Antarctic deuterium excess using the logarithmic definition (dln) introduced by (Uemura et al., 2012). Markle et al. (2017) showed that in the WAIS Divide ice core, dln abruptly increases in synchrony with the onset of GIs; at the termination of GIs, dln abruptly decreases. Markle et al. (2017) used a climate model simulation with moisture tagging to show that this relationship could be explained by north-south shifts in the location of moistures sources associated with changes in the shifts of the Southern Hemisphere (SH) subpolar jet and westerly winds. This is consistent with work of Schmidt et al. (2007) who had previously shown with climate model simulations that the deuterium excess should be inversely correlated with the Southern Annular Mode (SAM) index. Masson-Delmotte et al. (2010) made a similar argument on the basis of the Dome C core, but without sufficient dating precision to demonstrate the close relationship found by Markle et al. (2017). These findings were later extended to multiple Antarctic sites by Buizert et al. (2018).

Please note also that the use of parentheses to mean opposites is very difficult to read, and should be avoided. There is no reason to do this.

“abruptly increase (decrease) in synchrony with the onset (termination)”

3. A. In general, a clearer discussion of the relationship between CH4, d18O, and dxs is needed. I am missing a clear explanation of this for the non expert. I think the following points are important to make clear. Consider, for example, how you would explain Figure.5 to a non-expert.

First, the relationship between CH4 and d18O in Greenland is well established, and the lag is short. Second, cores have been linked mostly by matching methane, but there is uncertainty in the ice timescales because of uncertainy in DeltaAge. Third, the WAIS Divide core has a small enough DeltaAge that it was possible to show a clear lag of 200 years between abrupt warming AND abrupt CH4 increases in Greenland and the changepoint of d18O in Antarctica. Fourth, also with WAIS Divide, it was shown that dxs is is close to being in phase with CH4, and therefore in phase with d18O in Greenland. Fifth, this has been extended to other Antarctic cores by volcanic synchronization within Antarctica. Sixth, the current paper adds volcanic synchronization between Greenland and Antarctica, further refining the relationships among dxs, CH4,

and d18O in both Greenland and Antarctica.

B. A few other small things.

i) In the statement on line 52,

DO events are believed to originate in the North Atlantic, but have a global climatic impact that is documented in a wide range of paleoclimate archives across the Northern Hemisphere (Voelker and workshop participants, 2002).

I would say:

DO events are believed to originate in the North Atlantic, but have a global climatic impact that is documented in a wide range of paleoclimate archives across the Northern (Voelker and workshop participants, 2002) and Southern Hemispheres (Pedro et al. 2015).

ii) Line 71, "Modeling past Deltaage requires assumptions about past accumulation and temperature variations, introducing substantial age uncertainties associated with the synchronization."

This isn’t quite true for WAIS Divide or central Greenland, where we know the accumulation extremely well. Consider changing this to make it clear that the chief uncertainty for WAIS Divide (and for Greenland) is the firn physics, not the lack of knowledge of T and accumulation.

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