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Climate of the Past Discussions

Interactive comment on "Ranges of moisture-source temperatures estimated from Antarctic ice core stable isotope records over the glacial-interglacial cycles" *by* R. Uemura et al.

Anonymous Referee #1

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Conclusion: The manuscript deals with an important scientific question regarding past variations in source temperatures. Because of the many assumptions which implicit goes into the model used for interpreting the data it is difficult to make any final conclusion and more work is indeed needed in the future on this topic. The manuscript is well written and should only need minor revision. However I would like to see some of my questions addressed by the authors to strengthen this paper and for my own interest in the response to this review.

Summary: By using a simple distillation model (in this case the MCIM) the authors use the same methodology on ice cores from Dome Fuji, Dome C, and Vostok in order

to deduce past variations in source and site temperatures. In my objective the main significant finding is the very strong correlation between obliquity and source-site temperature difference. From a physical perspective this is to be expected a priori and it therefore comforting that the ice core data based on our assumptions of the moisture transport is showing this. In the manuscript is also presented the result of the linearization of change in dD and d-excess based on change in source temperature and site temperature using the same methodology on the three cores. It would perhaps be beneficial for the manuscript if the authors could put a paragraph in on the climatological interpretation of the determined differences in \beta_site parameter between the different cores. Given the many assumptions, which the model is based on it might not make too much sense to ask for a lot of sensitive tests. However I think that for example the super saturation function, which several recent papers have investigated and found to not conclude might be of interest to make a sensitivity study of.

Please see below for detailed comments: P. 292: L 13: change to " the value of beta_site by more than a factor..." P. 393: L 18: change to "... and by equilibrium distillation at very cold temperatures as well as the amount of rainout from the source to the sink" L 20: I do not know if this is simply a notation but several places in the manuscript you refer to dD and d-excess but then mention the d18O of the ocean isotopic composition. Mayb you want to change this to dD_SW P.294: L4: I think it is wrong to say that the methodology is not well established. I think it might be more correct to state that there are no common methodology used. Maybe you might want to mention that there are still significant uncertainties related to the models used. P. 395 L6: The fact that the two profiles DF1 and DF2 shows "remarkable" similarities should come as a big surprise... hopefully... L 7-24: 1) When you remove the off-set as you argue for what is the statistical difference between DF1 and DF2. Is this what could be expected from your measurements noise or are maybe caused by deposition noise. 2) I'm not sure I follow your argument that it is problems with the storage of the sample. It might be but then I suggest you to be more descriptive of the problem. This is potentially very important information for the community to use when dealing with samples.

3) As I read the text you show that there is an offset between DF1 and DF2 and that you re-measure DF1 and show that the offset is caused by wrong previous measurements of DF1. You suggest that this is caused by storage problems of DF1 samples in glass vials. a. However most people would expect that storage in glass vials should be ok and not cause any fractionation. b. Since the storage problem can only arise between the samples were cut/melted/stored and measured I would not expect this to span a significant period of time. This would mean that there would be significant problems with the storage over long time. c. Does this mean that there is a significant problem with the full DF1 core? d. I would suggest that you re-measured a few of the old samples stored in the glass vials to really show that this is where the problem is because could a more likely reason simple by problems with the standards used? e. I know that re-measuring samples is not fun and I don't think that too many samples are enough to either support your hypothesis or reject it. f. Under all circumstances I think it is important to shed a bit of light on this problem so can I ask you to fill in more details in the text on this? P. 396 L 4-13: I'm curious does these temporal resolutions refer to the cutting scale combined with a depth-age scale or does it take in to considerations of diffusion. In the case of them not taking in to considerations the diffusion I think it would be great if you reported the actual numbers of what the smallest periodicity of a given signal would be possible to observe. L13-14: Maybe change the formulation to " We place the ice core isotopic records on a common age scale in order to be able to make a comparison" L19: The difference you report is that after or before you put the records on a common time scale – I'm a bit confused here. L 23: The same as above: If this is not taking into considerations of diffusion does it actually make sense to say that you have a 200-yr resolution. In any case you could solve this by calculating the diffusion length and show that it is smaller than the cutting scale. L 27: I'm a little bit confused about the stated significant smaller imprint of obliquity in DF than Vostok and EDC. Are you referring to Figure 2? Because then I would perhaps state that it might not be significant. . . but yes it does look less denounced. Secondly I'm a little bit confused by Figure 8 then because in panel b it seems that the 40 kyr cycle is pretty

strong in DF and Vostok but less so in EDC? Is that correct. . ..maybe just update the text to be more precise. P. 397 L2-6: Maybe it would be nice to make an insert in the figure with a blow-up of the lag in DF compared to the other cores during Termination. L13: Maybe change d18O_SW to dD_SW Formula 1/2: Only one of them is actually needed since you could just substitute d18O and dD with a d* and state that d* is either d18O or dD. P398: L25-26. Because of the variability of the inversion strength both spatially but also temporal maybe it would be an idea to include a sensitivity test of the parameter on your model results? P.399 L9-10: Given how much recent studies have shown to not agree on the super saturation and the likelihood that this might change from glacial to interglacial period I would strongly suggest to include a sensitivity test of this parameter. L15-16: Have you tested by increasing the source temperature that you are not able to decrease the isotopic value. L15-16: I was just wondering – could it be such that there were an influx of stratospheric moisture to the site which could result in the low isotopic values? L17: I do not remember if the MCIM includes the temperature inversion it would therefore be good if you in the text could specify that this site temperature is either the snow surface temperature or the cloud temperature. P.400: L11-13: I will suggest a reference to Ellehøj et al. (in review) which have estimated the fractionation coefficient at low temperature and found some significant difference. A copy of the manuscript can be obtained from ellehoj@nbi.ku.dk . Also because of the larger fractionation coefficient that is determined in this paper could it be such that by using this value it is possible to get the right isotopic value as well as temperature? L22. I would expect beta_site to also depend on the super saturation function. P 404 L7: It is not clear from the text on which the uncertainties are based. Maybe just add a single sentence to clear up this. L 14-23: If I understand it correctly you compare the source region estimate based on the parameters for DF and Vostok. However it is not clear to me that you are actually permitted to compare these estimates because there are no argument that the source region is actually the same. Maybe just specify in the text why the figure 5a is interesting. P. 405 L4. Actually your new estimate DeltaT_site is as you state higher than previous value but it is not larger than the uncertainty permit. L 23:

Remove “a larger” L 28: reformulate “. . .slightly lower than . . .” P.406 L2: Reformulate “ The Lower DeltaT_site results. . .” L17-20: I don’t think that is evident that this is the case. Do you have a reference for this or are you able to explain this more in details? L23: Replace thanks with “from” L 26: Because of your statement that there exist a latitudinal temperature gradient in the southern ocean you might want to take this into considerations when you compare the source region development for the different ice cores. P408 L21. This sentence confuses me: Do you mean Delta T_source instead of DeltaT_site? P.409: L3: What is the estimated uncertainty on the reported lag periods? L16: Change “and” with “an”? P.410: L. 20: Maybe also reference Landais et al 2011 P. 411 L 26: swap the position of dD and d18O in the parentesis.

Figure 3: I don’t seem to figure out what the blue solid line represents except being a simulated curve but what parameters have been used?

Figure 5: Maybe panel a and b can be merged since it is only an extra red line that needs to be added in panel a.

Figure 6: Why is it colder at EDC during LGM? This is of course because of lower dD but why does dD become significantly lower at EDC than other east Antarctic stations

General comment: At nowhere in the text is it mentioned that a site (being DF, EDC or Vostok) might have more than just a single source each. What if say for example DF had two source regions that changed in relative magnitude from glacial to interglacial period? I know that it will be very difficult to say anything about this but maybe it will be good to add a couple of sentences on the inherent uncertainty in the model?

Interactive comment on Clim. Past Discuss., 8, 391, 2012.