Referee #1

First, we thank you for your comments and suggestions. We have revised the manuscript according to your specific comments. Please find a detailed reply to all comments below.

Reviewer's comments are in '*italic'* font. Our response is in '**Bold'** font.

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.
The different β_{site} values are mainly caused by the different $\partial d/\partial \delta D$ slopes of the sites. As we discussed in the Appendix of the manuscript, the values of β_{site} parameter highly depends on the empirical definition of d-excess. If a logarithmic definition was adopted, the difference of $\partial d/\partial \delta D$ could be minimized. Thus, the differences in β_{site} parameter between the different cores also depend on the definition of d-excess. Finally, the β_{site} depends on supersaturation function but also on the range of condensation temperatures that are used. Please see the reply comments below (Figure R1, Table R1).
Therefore, it is difficult to state pure "climatological" meaning of the different β_{site} values because the β_{site} contains different information.

In principle, the slope of $\partial d/\partial\delta D$ multiplied by $\partial\delta D/\partial\Delta T_{site}$ (δ vs temperature slope) approximates to $\partial d/\partial\Delta T_{site}$ (=β_{site}). The present-day observation shows that 1) $\partial d/\partial\delta D$ ranges from −0.13 to −0.21 (e.g., Fig. 3), and 2) $\partial\delta D/\partial\Delta T_{site}$ ranges from 6.3 to 7.0. Thus, the value of β_{site} is 0.8 to 1.5.

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:

We have made a sensitivity study. The super saturation function was changes as the simulated curve corresponds to upper and lower bounds of present-day observed data (see Figure R1). A difficulty of such sensitivity test is that it is difficult to reproduce the present-day surface snow value because we cannot select data for regression analysis based on ice core data (see Section 3.2). Consequently, we performed regression analysis by varying ΔT_{site} and ΔT_{source} by 2 °C (as discussed in Section 3.1).

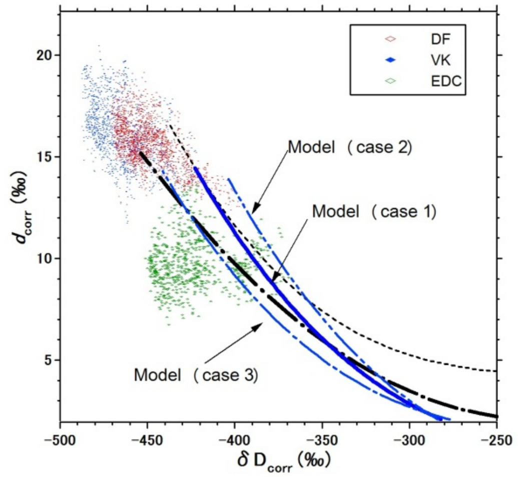

Figure R1 Super saturation function used for sensitivity study

The results are shown in Table R1. The impact of super-saturation function on the β_{site} parameters is small. We have added this new analysis in the revised manuscript.

Table R1 Sensitivity of super saturation function

	Simulated DF isotope ratio			Temperatures (°C)		Sensitivity coefficient			
Super saturation function (S_i)	**δD(‰)**	**$\delta^{18}O$(‰)**	***d*(‰)**	**DF site**	**Source**	β_{site}	β_{source}	γ_{site}	γ_{source}
1.020-0.0030T (case 1)	-422.7	-54.6	14.5	-61.0	18.0	1.3	1.6	7.7	3.2
1.020-0.0025T (case 2)	-403.6	-52.2	13.9	-58.0	18.0	1.2	1.4	7.7	3.3
1.020-0.0035T (case 3)	-441.3	-57.0	14.3	-64.0	18.0	1.1	1.6	7.5	3.0

Specific 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"

These points have been corrected according to the suggestions.

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. Maybe you want to change this to dD_SW

The *d* depends both δD_{sw} and $\delta^{18}O_{sw}$. But, as described in the text, we assumed that the δD_{sw} to be linearly scaled to $\delta^{18}O_{sw}$. We added 'and δD_{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.

The text has been revised.

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 now 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?*

At first, we should note that the off-set discussed here is very small. Compared with glacial-interglacial amplitude of δD (about 60 permil), the off-set discussed (about 0.8 permil for δD) corresponds only 1.3% of full signal amplitude. Therefore, the correction applied here does not influence the conclusions of previous publications.

We thoroughly checked this problem including re-measurement of the old samples stored in the glass vials. As referee#1 suggested, the storage problem is potentially very important information for the community when dealing with sample. Here, we described this issue in detail because this reply in CPD would be a good opportunity to describe such technical problems.

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.*

We re-cut old DF1 ice samples for checking this problem (i.e., we cut the stored ice sample, and melted/measured the sample). The results show that 1) 're-cut old DF1' < 'original DF1', and 2) 'DF2' < 'original DF1'. These off-sets are almost the same, strongly suggesting that only the 'original DF1' data is isotopically enriched slightly (by ~0.2 permil for $\delta^{18}O$) than both 're-cut DF1' and 'DF2' data. This is not caused by different depositional conditions because the same DF1 core (i.e., 're-cut old DF1' and 'original DF1') shows the off-set. Note that the DF2 coring site is just 43m north of DF1 site, and thus there expected to be no isotopic differences between the two cores.

Secondly, this 'off-set' is not caused by measurement noise because the error caused by isotopic measurement (e.g., 0.04 permil for $\delta^{18}O$) is smaller than the offset (0.24 permil for $\delta^{18}O$). After the beginning of DF2 measurement (after 2006), we carefully checked both accuracies and precisions. The 'international reference' values against VSMOW reference were also checked with inter-laboratory comparisons (such as IAEA Water Isotope Interlaboratory

Comparison). The reproducibility was checked by measuring a sample (melted ice beg water) every batch (see Figure R2).

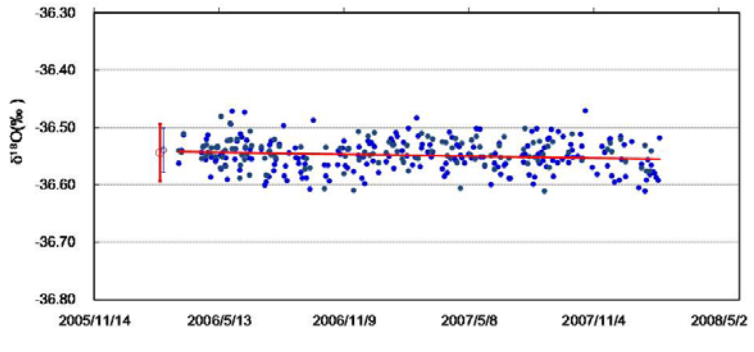

Figure R2. A 2-year-long reproducibility test.
A sample (melted ice berg water) was measured every 'batch' for checking the long-term stability. The average $\delta^{18}O$ value is -36.54 ± 0.04 ‰ (n=330).

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.*

First, we had stored the melted samples, in glass bottle with plastic screw cap, at room temperature (i.e., liquid water). We speculated this is one of reasons of the 'off-set' described above. The time period between 'cut/melted/stored' and 'measured' for DF1 old samples ranged from several months. But for d-excess measurements, it sometimes took a few years.

We quantitatively estimated the evaporation effect of the glass bottle (Figure R3). The result shows that about 1-year long storage enriched the $\delta^{18}O$ by about 0.05 permil. This cannot explain the full off-set (i.e., about 0.2 permil), but surely a cause the off-set.

Note that, for the new DF2 samples, we measured the sample within 1-month after preparation (and samples were stored frozen).

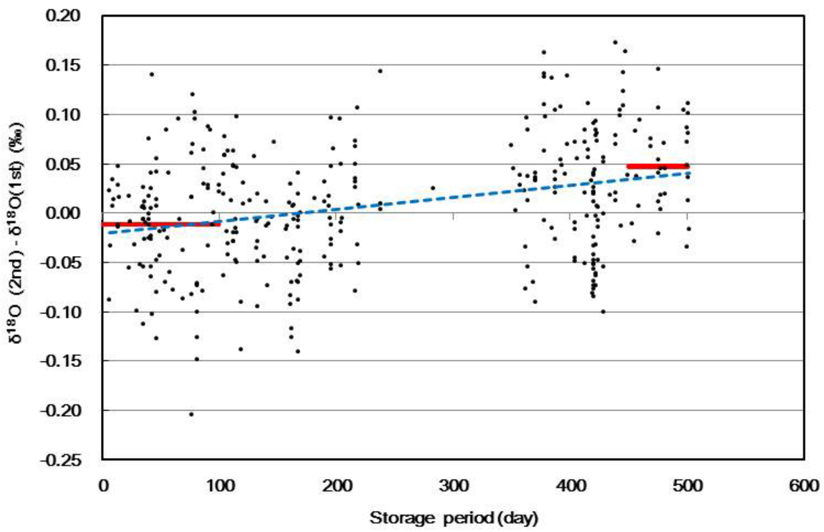

Figure R3. An evaporation effect of long-term storage at room temperature.
Y axis indicates a δ^{18}O difference between first and second measurements. First measurement was conducted just after samples were melted. The second measurements were conducted after a certain period (shown in x axis). Red bars show averaged values between 0 – 50 day and 450 – 500 day, respectively.

c. Does this mean that there is a significant problem with the full DF1 core?

As mentioned above, the off-set is small compared to glacial-interglacial and AIM variations; and that (ii) it may be systematic and therefore does not affect magnitudes of events and previously published conclusions. Therefore, this does not influence the conclusions of previous publications.

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?

We already measured a few of the old samples stored in the glass vials. These data show that siginificant evaporation effect due to long-term storage at room temperature.
Standard used might be another cause, and so we already measured a standard stored in air-tight glass vessel (which is different from the sample-storage vials). The difference between old (2001) and new (2006) measurements is not

significant (i.e., within 0.05 permil for $\delta^{18}O$). Therefore, we have no evidence supporting the standards problem.

e. I now 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?

As described above, we thoroughly checked this problem including 're-cut of the DF1 samples' and 'samples stored in glass vials'. We have evidence showing that the long-term storage causes isotopical enrichment of the sample. It explains about half of the observed off-set between DF1 and DF2. The rest of the reasons remain unknown.

We have added sentences and revised the text. We think that extensive description of this issue in the revised manuscript is not necessarily because this reply letter will be archived in CPD.

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.

The temporal resolution is the cutting scale combined with a depth-age scale. As far as the main discussion of this study (i.e., 10^4-10^5 year time scale), the similar amplitudes of *d* variations between Vostok and Dome F (e.g., 200 and 250 ky BP) suggest that the diffusion is not significant issue for this time scale (~40 kyr cycle). We understand that this would be important issue for comparison with more high time-resolution. For EDC, an ice diffusion length was evaluated (about 8 cm at MIS 11 depth, Pol et al., Clim. Past 2011). I have added the reference and revised the paragraph.

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"

Corrected

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.

This is the difference 'before' we put the records on a common time scale. The

order of the sentences was revised.

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.

Please see the above reply. For example, 200 year corresponds to 165cm-length of ice at EDC (at ~1460m depth, ~108 kyrBP), so it will not be affected by diffusion (i.e. Pol et al (CP 2011) estimated the diffusion length is 8cm at MIS11).

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.

Yes, we intended to refer Figure 2. We have checked the figure and Spectral analyses, confirming the the 40kyr cycle shown in Fig.8 is significant. Therefore, we have revised the description about Fig.2.

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.

We have added a new figure illustrating an enclosed view in figure 2 (figure 3 in the revised manuscript).

L13: Maybe change $d^{18}O$_SW to dD_SW Formula 1/2: Only one of them is actually needed since you could just substitute $d^{18}O$ and dD with a d and state that d* is either $d^{18}O$ or dD.*

Maybe we cannot understand your point, but both equations are needed for calculation. It would be true that the eq.(2) can be removed because the δD_{sw} is simply calculated on the assumption that $\delta D_{sw} = 8\ \delta^{18}O_{sw}$, and the correction formula is essentially the same as $\delta^{18}O_{sw}$. We would like to maintain the current notation because we think that the current presentation is easier to follow the calculation process.

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?

The linear relationship between T_{site} and $T_{condensation}$ affects the range of temperature where the fractionation factor is calculated and expected to have an impact on β_{site} estimates. This hypothesis of a constant inversion is a limitation of MCIM as we know that inversion is different during days of condensation when compared to "dry" clear sky days. However, the work of Ekaykin (2003), which is cited in the reviewed manuscript, at Vostok has confirmed the validity of the mean relationship between condensation and surface temperature based on long-term observation at Vostok Station. They suggest that the mean annual temperature is well representative in terms of isotope composition of snow due to dominant role of diamond dust (observed nearly each day throughout a year) in the total amount of precipitation.

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.

We have performed sensitivity test for the super saturation function. Please see the above reply.

L15-16: Have you tested by increasing the source temperature that you are not able to decrease the isotopic value.

Yes, we have tested. The increase in moisture source temperature decreases the isotopic value. But, at the same time, it increases the *d* value. For example:
Normal condition (T_{site} = -61 °C, T_{source}= 18°C) (δD = -422.7, *d* = 14.5);
Warmer source temperature (T_{site} = -61 °C, T_{source}=22°C) (δD = -434.7, *d* = 20.5).
Consequently, it is difficult to find the site and source temperatures that fit both δD and *d*.

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?

Landais et al. (GRL, 2008) did a calculation based on the available fluxes for the stratospheric input. The stratospheric water input is negligible (stratospheric input /total input is 1070/30000000), and thus it is not important on $\delta^{18}O$.

Amaelle Landais, Eugeni Barkan, Boaz Luz, Reply to comment by Martin F. Miller on "Record of $\delta^{18}O$ and ^{17}O-excess in ice from Vostok Antarctica during the

last 150,000 years", *GEOPHYSICAL RESEARCH LETTERS*, VOL. 35, L23709, 1 PP., 2008, doi:10.1029/2008GL034694

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.

The site temperature is average annual air-temperature at the surface snow. We have revised the text.

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?

We obtained the manuscript. Unfortunately, Ellehøj et al. is under review, so we cannot include the result. Of course, the different fractionation factor much affects the simulated isotopic value. It should be noted that, at low temperature, the fractionation factor is 'tuned' to fit the observed snow isotopic value by using the super-saturation function (see e.g., Jouzel and Merlivat, JGR 1984). Thus, it will change many parameters, and will need a lot of new examinations. We have cited the Ellehøj et al. (under review) and mentioned this in the perspectives (improved quantification of fractionation coefficients at very low temperatures).

L22. I would expect β_site to also depend on the super saturation function.

Please see the above response (figure R1 and Table R1).

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.

The uncertainty is based on the standard deviation of 1kyr duration around the maximum. We described that 'Here, temperatures are based on the average and ± 1σ of 1 kyr duration around the maximum or minimum point.'

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.*

As described in text and Table1, among four coefficients, only β_{site} is significantly different from previous estimates. Thus, Figure 5a shows that the β_{site} coefficient directly affects the glacial-interglacial magnitude of ΔT_{source}. In fact, a very similar result is obtained by changing only the β_{site} from 1.3 (this study) to 0.5 (smaller value). We have added several sentences to explain this point.

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.

We have revised the text.

L 23: Remove "a larger"

L 28: reformulate ": : :slightly lower than : : :"

P.406 L2: Reformulate " The Lower DeltaT_site results: : :"

These points have been corrected

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?

Vimeux *et al*. (JGR, 2001) argued that the Vostok *d* record prior to 250 kyr is affected by the ice flow, but this is just for the deeper part. As no further information available, we have removed this sentence.

L23: Replace thanks with "from"

This sentence was re-written.

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.

It is important to elucidate this. Please see the reply to the last comment.

P408 L21. This sentence confuses me: Do you mean Delta T_source instead of DeltaT_site?

Here, ΔT_{site} is right. As shown in Figure 8 (a) and (d), the obliquity component of ΔT_{site} is smaller than that of δD. In other words, both *d* and δD have obliquity component, and the obliquity signal appears somewhat weakened in ΔT_{site} and much weakened in ΔT_{source}.

P.409: L3: What is the estimated uncertainty on the reported lag periods?

The uncertainties of DF, EDC, and VK are 0.07, 0.11, and 0.08 kyr, respectively. We added these values in the revised text.

L16: Change "and" with "an"?

Corrected.

P.410: L. 20: Maybe also reference Landais et al 2011

The reference was added.

P. 411 L 26: swap the position of dD and d18O in the parentesis.

Corrected.

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?

The blue line represents the simulated curve with parameters described in Section 2. We have revised the figure (i.e., Fig R1 curves for new sensitivity tests) and caption.

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.

We tested the merged version, but it appears to be difficult to see the three similar waves, and therefore decided we would keep the three panels.

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

The isotope differences between the three sites were discussed in Sime *et al*. (Nature 2009), which is cited in this study. Their analysis based on a GCM simulation suggests that differences in δ can be explained primarily by variations in the palaeothermometer gradient (i.e., ∂δ/∂T), rather than in temperature. Note that their study does not include correction for moisture source, but used either δD or $\delta^{18}O$ value. Our study revealed that the ΔT_{site} of EDC is still colder than the others as single δ approach.

We have added sentences on this point (in section 4.2).

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?

Moisture sources for inland Antarctica (for example DF) are expected to be located around 40 - 70°S ocean (as described in P407 L1). Separating the source-areas requires additional information (i.e., chemical composition, backward trajectory analysis, and general circulation model etc.), because the isotopic composition of snow reflects a vapor-amount weighted average information. The ΔT_{source} estimated using MCIM is, therefore, an average ocean surface temperature averaged by different moisture sources. We have added sentences on this point (in section 4.3).

We thank you again for your comments and suggestions.