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Hydrology and Earth System Sciences Discussions

HESSD

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Interactive Comment

Interactive comment on "A bare ground evaporation revision in the ECMWF land-surface scheme: evaluation of its impact using ground soil moisture and satellite microwave data" *by* C. Albergel et al.

C. Albergel et al.

clement.albergel@ecmwf.int

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The authors thank Carlos Jimenez (Reviewer #2) for his review of the manuscript and for his fruitful comments. For an easier comprehension, general comments of the Reviewer are also reported (2.XX).

2.1 [Given that the main objective seems to be to improve the evaporation, and that the authors seem happy in evaluating their estimates with in situ observations, I am wondering if anything in terms of using in situ evaporation observations has been considered during this work, or if there are plans for this and they could be discussed in the paper. Even without any comparison, some simple figures, as basic statistics reflecting the changes in evaporation (differences between old and new runs) will help putting in perspective the this new formulation in terms of surface fluxes.]

Response to 2.1

Results from the recent land surface model developments at ECMWF were evaluated using a land surface benchmarking database gathered for this purpose. Those included field sites from the FLUXNET (http://www.fluxdata.org/) and CEOP (http://www.ceop.net/) observing networks where latent heat, sensible heat and carbon dioxide fluxes measurements are available. They are used to evaluate different land surface scheme versions: TESSEL as used in the ECMWF ERA-Interim re-analysis and HTESSEL as currently operational at ECMWF are compared for latent and sensible heat fluxes. For instance, The land surface fluxes results from offline-runs indicated an average improvement of 8%, when adopting the HTESSEL (BEVAP_NEW) scheme in replacement of the former TESSEL scheme evaluated as root-mean-square-error reduction on both the latent and sensible heat fluxes measured over 36 FLUXNET and CEOP flux-towers. The offline run corresponding to BEVAP_OLD has not yet been evaluated against surface fluxes, although this is an ongoing activity. The following paragraph is added to the discussion section in the revised version of the manuscript: P.6733, L.7 : Âń Results from the recent land surface model developments at ECMWF were evaluated using a land surface benchmarking database gathered for this purpose. Those included field sites from the FLUXNET (http://www.fluxdata.org/) and CEOP (http://www.ceop.net/) observing networks where latent heat, sensible heat and carbon dioxide fluxes measurements are available. For instance, The land surface fluxes results from offline-runs indicated an average improvement of 8%, when adopting the HTESSEL (BEVAP_NEW) scheme in replacement of the former TESSEL scheme evaluated as RMSD reduction on both the latent and sensible heat fluxes measured over

36 FLUXNET and CEOP flux-towers for 2004 (Balsamo et al., 2012). In the same way, future activities will concern the evaluation of the impact of BEVAP_NEW with respect to BEVAP_OLD in term of surface fluxes. Âż New reference: G. Balsamo, C. Albergel, M. Balzarolo, A. Beljaars, S. Boussetta, J.C. Calvet, E. Dutra, T. Kral, D. Papale, P. de Rosnay, and I. Sandu, 2012: Usefulness of Benchmarking for Global Land Surface Model Development" BAMS Conference notebook, summary from the 26th AMS Conference on Hydrology, 22-26 January, 2012, New Orleans, Louisiana, US. IN PRESS.

2.2 [It may also be worth discussing the choice of one specific soil moisture network for this work. Given the very recent years where the study is conducted, one may expect that more in situ SM observations are available and may allow extending the study to other regions. In our experience, land surface models tend to work better over regions where more ancillary data to parameterize the model exist (e.g. soil texture, porosity, etc), precisely the regions that are later on used to evaluate the model (and US may be one of those regions).]

Response to 2.2

This study focuses on the year 2010 and on the 2010-2011 period. At the time this study was conducted, there were not that many networks with data available for the whole 2010-2011. To our knowledge only the SCAN and the SNOTEL networks (within the US also but in mountainous areas) provide data almost in near real time. Stations from the REMEDHUS (20 stations in Spain), the OZNET (36 stations in southeastern Australia) and SMOSMANIA (12 stations in southwestern France) networks were tested too. Our main concern with these 3 networks is that, according to the specifications of ECMWF Land Surface Model (LSM), their fractions of bare soil are very low. Hence they were almost no differences between the two experiments (BEVAP_OLD and BEVAP_NEW). The SCAN network was chosen because its stations span almost all over the US in areas with different fraction of bare ground (Please see new Figure 1 in Response to 2.15). This group of stations (along with many others, about 400 from the International Soil Moisture Network) has successfully been used to validate various soil moisture products from ECMWF (Please see Albergel et al., 2012 a & b of the reference list).

2.3 [The paper is well written, and the subject is of interest for HESSD readers. Their contents are well presented in general, but I have some doubts about the need of the discussion section. As it is, it seems to me that there is anything new there that is not already commented somewhere else in the text (apart from perhaps the discussion on assimilation of precipitation, which could be moved to the conclusions). For instance, the main numbers about the study are given again here, and repeated again in the conclusions. Figures may need some work and/or additions to make them more readable/interesting.]

Response to 2.3

The discussion section has now been reviewed according to the comments of Reviewer #2 and Reviewer #3. Particularly, discussions related to SMOS and the possible use of surface fluxes measurements were added to the revised version of the manuscript (Please see Responses #2.14, #2.1). We agree with Reviewer #2 about the quality of the figures, their quality was improved (Please see new figures, Responses #15 to #19).

2.4 [P6727-L8. I understand the message the authors are trying to pass here, but to say that in situ data contains “representativeness” errors may not be the best way of presenting this. As it is expressed here, all the “blame” seems to go to the in situ observation, but what about the 6400 km2 spatial representativity of the off-line model runs? If I get this right, the station may be located and/or correspond to one specific biome within the ECMWF pixel, as the ECMWF uses a tile scheme, while the compared SM from the model is the integrated value for the whole pixel. For the same reason than a tile scheme is justified in a model, it is easy to also imagine some combinations of surface characteristics, climate, and hydrological conditions where a in situ station placed in a reasonable location cannot capture the integrated response from the 6400 km2 around the station (without being anything wrong with the station measurements or location strategy).]

Response to 2.4

We agree with Reviewer #2 about the representativeness issue. Several studies have confirmed that soil moisture measured at a specific location is correlated with the mean soil moisture content derived from coarser resolution products (such as from model and/or remote sensing). Indeed, several studies have shown that soil moisture variations in space and time can be related to small scale and large-scale components (Entin et al., 2000). The large-scale component is related to the atmospheric forcing (precipitation and evaporation processes) and the small-scale component is mainly due to soil properties, land cover attributes and local topography. The temporal stability concept proposed by Vachaud et al. (1985) indicates that soil moisture patterns tend to persist in time and therefore that soil moisture observed at a single point is often highly correlated with the mean soil moisture content over an area. To some extent, it is possible to estimate soil moisture over an area from local measurements. That is why we believe that the use of “representativeness errors“ is appropriate here. However as we don’t want all the ’blame’ to go to the in situ observation, their importance is highlighted in section 2.3 of the revised version of the manuscript.

P.6725, L. 25 ’ A usual step for evaluating soil moisture products from model is to determine whether their behavior matches the observations. Hence in situ measurements of soil moisture are a highly valuable source of information. ’

P.6726, L.8 ’ In situ data may contain errors [. . .] ’ replaces ’ As in situ data may contain errors [. . .] ’.

Entin, J. K., Robock, A. N., Vinnikov, K. Y., Hollinger, S. E., Liu, S., and Namkhai, A.: Temporal and spatial scales of observed soil moisture variations in the extratropics, J. Geophys. Res., 105(D9), 11865–11877, 2000.

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Vachaud, G., Passerat de Silans, A., Balabanis, P., and Vauclin, M.: Temporal stability of spatially measured soil water probability density function, Soil Sci. Soc. Am. J., 49, 822–828, 1985.

2.5 [P6728-L25. I see the point of filtering the in situ dataset so only stations that reasonably correlate with the model values are retained for the analysis. But perhaps the name of 'quality control' is not the most appropriate. I can imagine again places where there is nothing wrong with the station sensors, but geographical particularities or even wrong modeling resulting in low correlations.]

Response to 2.5

We agree with Reviewer #2 and the name of 'quality control' is removed in the revised version of the manuscript.

P6728-L25 ' Very little quality control is applied to measurements from NCRS-SCAN stations. Dharssi et al. (2011) used a simple quality control process to identify stations where sensors might be dysfunctional. ' Is replaced by: ' As indicated in NCRS-SCAN website, data are provisional and subject to revision, very little control is applied to measurements from NCRS-SCAN. Dharssi et al. (2011) used a simple process to identify stations where sensors might be dysfunctional. '

P.6728-L.23-24: '[. . .] a quality control is applied [. . .]' is now '[. . .] a process is applied [. . .]' Also, the following sentence is added: P3729, L.1: 'This rather strict process has probably removed some good stations too (e.g. in areas where the model might not realistically represent soil moisture)'

However, a visual check was done (not shown) for the group of stations removed from our pool of stations (26 stations). Most often than not they present spurious patterns (e.g. sudden jump in the signal) or have very little data (few weeks) over 2010-2011.

2.6 [P6729-L3. What is it meant by 'does not alter the conclusions of the paper'?]

Response to 2.6

By ‘does not alter the conclusions of the paper’ we expressed the idea that removing 26 stations (over 148) does not radically change the results/conclusions obtained in this paper. We still have a sufficient number of stations for the comparisons. This sentence might be confusing and is removed in the revised version of the manuscript.

2.7 [P6729-L14. Selecting stations for a given bare ground fraction can also selects different climate/hydrological conditions, which can be affecting the comparisons. For instance, more bare ground and less vegetation could be a relative indication of a drier climate, less precipitation, stronger seasonal cycles, etc. This can result in higher absolute correlations and smaller RMSD. This may be worthy mentioning in the context of comparing statistics before and after the threshold is selected.]

Response to 2.7

We agree with Reviewer #2, according to his comment and comment from Reviewer #1, we demonstrated that areas with a higher fraction of bare ground present larger negatives biases (in situ minus model) with respect to in situ data. Using the NCRS SCAN network, considering all the stations, bias in on average -0.079 m3m-3, if we consider now stations with a bare ground fraction equal or higher than 0.4, it is -0.094 m3m-3 and -0.100 m3m-3 for stations with a fraction of bare ground equal or higher to 0.6. Larger negative biases are found in areas with high fraction of bare ground. The new bare ground evaporation improves the representation of soil moisture particularly in these areas.

2.8 [P6729-L28. True, but a very small increase. The decrease in RMSD seems more significant.]

Response to 2.8

Reviewer #2 is right at this point.

2.9 [P6731-L1. What angles are used for the simulations? I’m assuming a choice has been made.]

Response to 2.9

A 40° incidence angle was used for the simulations. At this angle, more SMOS data are available; it is the chosen angle of the short coming SMAP soil moisture mission, also. This information is added in the revised version of the manuscript.

P.6731, L. 3-5 : ‘The differences between the TB simulated using surface fields from BEVAP NEW and the one from BEVAP OLD are computed for each month of 2010, for both H (horizontal) and V (vertical) polarizations (referred to as TBH and TBV) and an incidence angle of 40°, for 06:00UTC and 18:00UTC.’

2.10 [P6731-L10. Monthly mean “difference” mean “biases”? 4.7 should be 4.72? 14.85K for an annual value, is it bias (too high?) or SD (more likely)? 4.12 to 4.14K? 3.7K is now bias and no SD?]

Response to 2.10

Indeed, it is a series typos, it should be : ’ For TBH (18:00 UTC) the global monthly mean bias between the two data set range from 4.72 K to 7.01 K, with an annual value of 6.2 K. For TBV (18:00 UTC), global monthly mean biases range from 2.94 K to 4.14 K, with an annual mean difference of 3.7 K. ’ It is corrected in the revised version of the manuscript.

2.11 [P6731-L14. It would help to evaluate the TB differences to also see a map of the soil moisture differences.]

Response to 2.11

We agree with Reviewer #2, such a map is added in the revised version of the manuscript. From this new figure, one can see that with the new bare ground evaporation areas with a higher fraction of bare ground (Please see new figure in Response 2.15, also) are drier.

P.6728, L.17: ’ Figure 2 illustrates the mean soil moisture for both BEVAP_OLD and

BEVAP_NEW for August 2010. A simple look to figures 1&2 permits to identify that areas with a high fraction of bare soil are drier with BEVAP_NEW than with BEVAP_OLD. '

2.12 [P6731-L26. Choice of angle based only on data availability? Is there an optimal angle (or angle range) for SMOS observation where the soil moisture may be better mapped?]

Response to 2.12 This is still an open research question. The base of the SMOS concept is using not only a single angle, but a combination of several angles to discriminate more easily the effects of vegetation. Future data assimilation experiments at ECMWF will answer this question.

2.13 [P6733-L9. What is it meant here by 'sensitivity' and what does the figures given mean? Changes in TB with respect to changes in soil moisture? This may require to be elaborated a bit more.]

Response to 2.13

For a better understanding, the following sentence : ' Sensitivity in simulated TB to the new bare ground evaporation is found to be close to 15 K and 10K in H and V polarizations, respectively. ' is rephrased in the revised version of the manuscript. ' It is now ' Changes in TB with respect to changes in soil moisture as a result of the new bare ground evaporation is found to be close to 15 K and 10K in H and V polarizations, respectively. ' in the revised version of the manuscript.

2.14 [P6733-L13. It will be helpful to point out that the bias between SMOS observations and ECMWF simulated Tbs is not just related to the SM, but that other factors are also important (e.g. SMOS internal calibration, assumed model soil roughness for the RT simulations, other input parameters affecting the radiative transfer, or the radiative transfer itself). I guess that the authors have some idea about the expected sensitivity of TB to changes in soil moisture, so you are in the position to say whether the biases

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observed at the moment could be "fixed" by realistic changes in soil moisture, or other elements in the RT model or the input parameters also need to be investigated (I guess so, as a CMEM platform calibration is suggested).]

Response to 2.14

The reviewer is right at this point. Indeed, soil moisture is only a component (although an important one) of the mean bias obtained between SMOS observations and model equivalents. This is why in the main text we added the sentence " The latter are closer to SMOS observations but with large global mean differences and standard deviation (about 10K and 20 K, respectively)", to acknowledge that it is not only an improvement in soil moisture which is going to remove the bias.

In order to make this point more clear we have added the following sentence in P6733-L13:

"These residual biases are also related to other factors such as the SMOS instrument or input parameters of the radiative transfer model, which are not straightforward to assess. Radio frequency interferences affecting the SMOS measurements could also be responsible of the bias".

And in P6733-L12, we have substituted "..to SMOS observations but with large global mean differences.." by "..to SMOS observations but with still large global mean differences.."

2.15 [Fig 1. The huge title in that figure seems awkward. The chose of colors in the scale does not help to interpret the figure, a larger number of color will help interpreting the map.]

Response to 2.15

According to the review, Figure 1 was revisited and its quality improved in the revised version of the manuscript.

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2.16 [Fig 2. Both RMSD and number of stations are discrete quantities, plotting one as a solid line, the other in dot form is confusing, both solid (with markers) or both in dot form (different shapes)?]

Response to 2.16

According to the review, Figure 2 (3 in the revised version) was revisited for a better understanding in the revised version of the manuscript a solid line now joined the dots representing the number os stations available. Also the x-label was modified 'Fraction of bare ground Threshold' according to comment from Reviewer #1.

2.17 [Fig 3. ut_XXXX meaning? Explain in the figure caption, or remove? Year 2010 added twice? Adding the location of the station (lat-lon, station name, type of biome) may be valuable information. The lack of in situ SM for J-F-M is because the ground is snow-covered?]

Response to 2.17

ut_XXXX is the number of the station, it is now removed and the name of the station 'Tule Valley (Utah)' is added instead.

2.18 [Fig 4. Same comment than in Fig 3 about ut_XXXX. More uniformity in Fig 3 and 4 may be an interesting addition to the paper. For instance, just one example of the off-line run but 3 for the IFS? It would be very interesting to see the same 3 examples of Fig 4 also used for Fig 3, so we could also see the effects of different spatial integration and/or other changes in the ECMWF scheme by comparing the ECMWF SM for the off-line and IFS runs for 2010.]

Response to 2.18

As for Figure 3 (4 in the revise version of the manuscript) previously the name of the station now replace ut_XXXX. Figure 3 (with offline runs) represents only 2010 while Figure 4 (5 in the revise version of the manuscript) represents 2 years (2010-2011) it will be hard then to see the effects of different spatial integration and/or other changes in the ECMWF scheme by comparing the ECMWF SM for the off-line and IFS runs for 2010 between Figures 3 & 4.

2.19 [Fig 5. As most of the differences are positive, the map will be better read if a larger part of the color scale is used for the positive differences. As I said above, it would be nice to see also a map of the soil moisture differences.]

Response to 2.19

As for Figure 1, the quality of Figure 5 (6 in the revise version of the manuscript) was revisited for a better readability. A map of the soil moisture differences is provided is the revised version of the manuscript (Please see Response 2.11).

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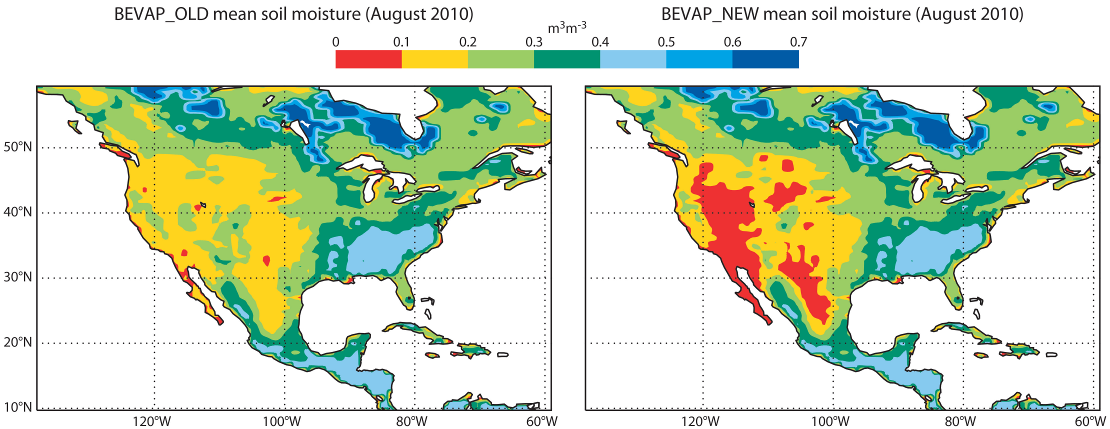

Fig. 1. Figure 2: Mean soil moisture for BEVAP_OLD (letf) and BEVAP_NEW (right) (August 2010).

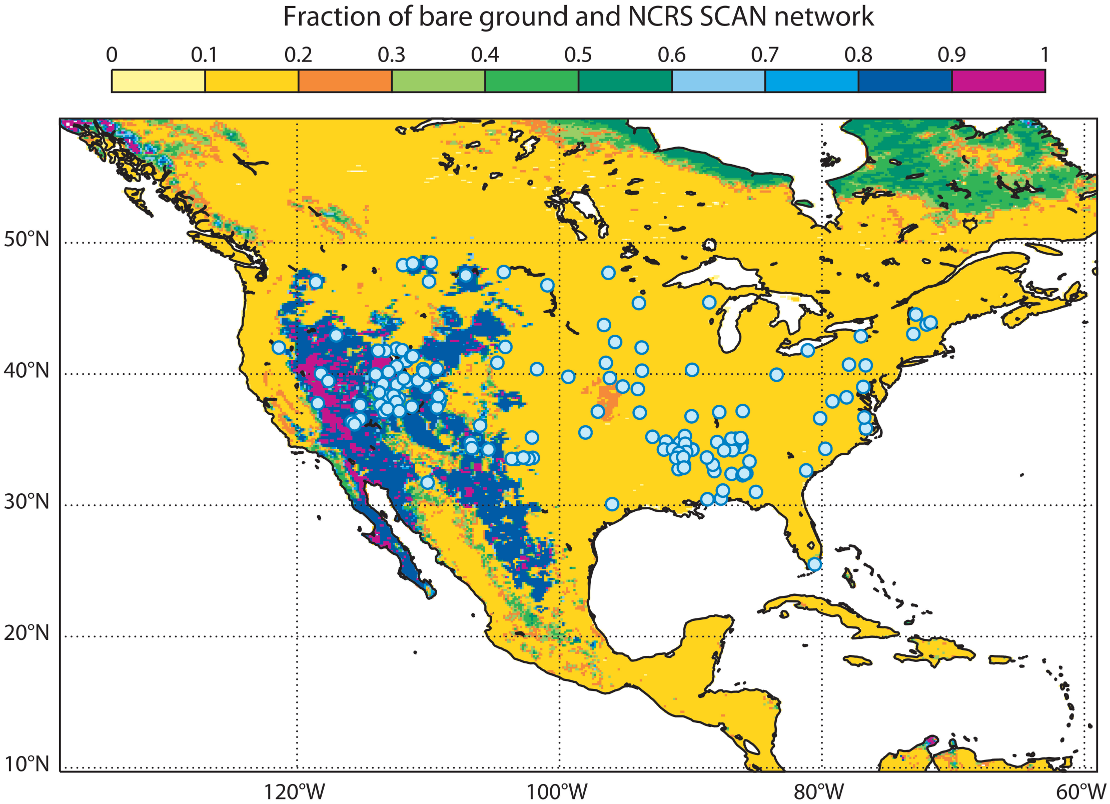

Fig. 2. New Figure 1

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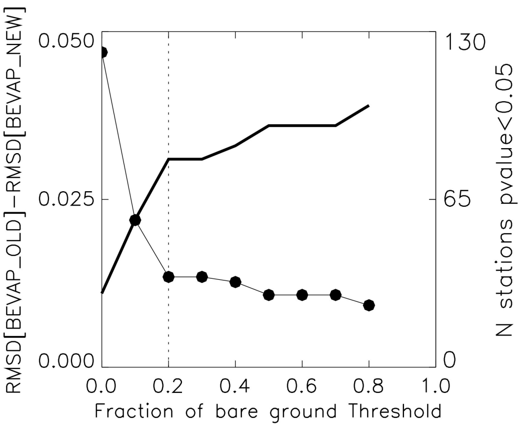

Fig. 3. New figure 2 (3 in the revised manuscript)

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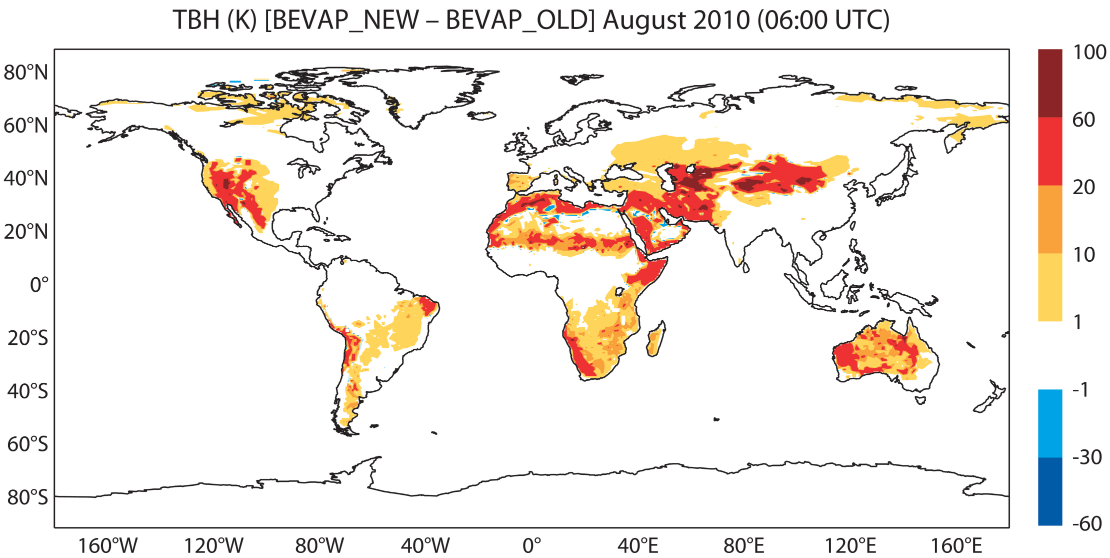

Fig. 4. New figure 5 (6 in the revised manuscript)