Response to the reviewers' comments. egusphere-2023-462 García-Pereira et al.

Félix García-Pereira[1], Jesús Fidel González-Rouco[1], Thomas Schmid[2], Camilo Melo-Aguilar[3], Cristina Vegas-Cañas[1], Norman Julius Steinert[4], Pedro José Roldán-Gómez[1], Francisco José Cuesta-Valero[5], Almudena García-García[5], Hugo Beltrami[6], and Philipp de Vrese[7]

[1]Complutense University of Madrid, Faculty of Physical Sciences, and Geosciences Institute (UCM-CSIC), Madrid, Spain
[2]Department of Environment, CIEMAT, Madrid, Spain
[3]Balearic Ocean Centre, Spanish Institute of Oceanography (IEO-CSIC), Palma de Mallorca, Spain
[4]NORCE, Norwegian Research Centre, Climate and Environment, Bergen, Norway
[5]Helmholtz-Centre for Environmental Research (UFZ), Leipzig, Germany
[6]Climate and Atmospheric Sciences Institute, St. Francis Xavier University, Antigonish, Canada
[7]Max Planck Institute for Meteorology, Hamburg, Germany

Correspondence: Félix García-Pereira (felgar03@ucm.es), Cristina Vegas-Cañas (cvegas@ucm.es)

The authors would like to thank the reviewers for their constructive suggestions and the time they devoted to reading and proofreading the manuscript. We have tried to integrate all suggestions and think that the manuscript has improved with them. We do appreciate their contribution.

The next sections contain a detailed point-by-point response to the reviewers' comments. Comments are labeled by reviewers and in order of appearance, i.e. R2C3 is the third comment of reviewer 2. The original number by the reviewer is also preserved if it was given.

1 Anonymous Referee 1

GENERAL COMMENTS:

R1C0: *REVIEWER'S COMMENT:*

General Remarks: The authors have comprehensively responded to the reviewers' comments. This manuscript should be published after the minor revisions described below.

AUTHORS' RESPONSE:

The authors welcome the positive perspective of the reviewer on the response to the revision. We are grateful for the reviewer's minor comments. Please find below the comprehensive point-to-point response to your review.

R1C1: *REVIEWER'S COMMENT:*

Ln 15: "The spectral method is capable of"

AUTHORS' RESPONSE:

The text has been changed according to the reviewer's indication.

R1C2: *REVIEWER'S COMMENT:*

Ln 15: Correlation has a specific statistical meaning and is used here to refer to a vague association between two physical quantities in the preceding sentence. Its use is therefore confusing and therefore should be avoided, while more clearly articulating what relationship the authors are discussing at this point.

AUTHORS' ANSWER:

To avoid using the term "correlation", the sentence has been modified following the reviewer's comment to: "Results with the spectral method suggest that changes in near-surface thermal diffusivity are related to changes in soil moisture content ~~changes. The spectral method shows to be capable of detecting this correlation at short time scales, which makes it a potential tool in soil drought and water resource availability reconstruction~~, which makes it a potential tool to gain information about soil drought and water resource availability from soil temperature data." (see line 14 of the annotated manuscript)

R1C3: *REVIEWER'S COMMENT:*

Ln 23: This is one thing that the authors did not fully improve. The air temperature increases are not the changes in the ocean and land surface warming! The energy imbalance is what causes the changes in air, ocean, and land temperatures and the authors should be more careful about their language here.

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication: ~~At the global scale, the increase of air temperature affects the ocean and land surface disproportionally, with warming in the oceans being smaller mostly due to its larger heat capacity. Hence, while oceans have warmed about 0.88 ^oC since the last half of the 19th century, land has experienced a temperature increase of 1.59 ^oC (Chen et al., 2021)~~This warming has been larger over land than over the ocean, with temperature values in 2011-2020 being 1.59 ^oC and 0.88 ^oC higher than 1850-1900, respectively (Chen et al., 2021). (see line 25 of the annotated manuscript)

R1C4: *REVIEWER'S COMMENT:*

Ln 37: Infrastructure should not be plural.

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C5: *REVIEWER'S COMMENT:*

Ln 41: GST not SAT histories

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C6: *REVIEWER'S COMMENT:*

Ln 59: "for at least a few years." Also begin the following sentence with "Such monitoring." "That" is a vague reference.

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C7: REVIEWER'S COMMENT:

Ln 64: "implications for"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C8: REVIEWER'S COMMENT:

Ln 71: There are multiple places in the manuscript where vague language continues to be used. This is a good example. "Short" and "relatively long" is unhelpful here. Simply indicate explicitly how long each of the records are.

AUTHORS' ANSWER:

An indication to the length of the records in each case has been added following the reviewer's indication: This is achieved by determining the apparent soil thermal diffusivity from four short (4-6 years) and two ~~relatively long~~longer (ca. 20 years) subsurface temperature records obtained at six sites in the area of the Sierra de Guadarrama (see line 74 of the annotated manuscript).

R1C9: REVIEWER'S COMMENT:

Ln 72: What is "its" referring to here?

AUTHORS' ANSWER:

"Its" here is referring to subsurface temperature variability. Therefore, "its" has been changed to "temperature" so as to improve the clarity of this sentence, according to the reviewer's indication.

R1C10: REVIEWER'S COMMENT:

Ln 73: Unique means singular and does not have qualifiers. Something is either unique or it is not. Avoid quite unique, very unique, etc.

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C11: REVIEWER'S COMMENT:

Ln 74: High mountain area relative to what? Again, relatively is vague and has not context here.

AUTHORS' ANSWER:

The word "relatively" has been eliminated, according to the reviewer's indication.

R1C12: REVIEWER'S COMMENT:

Ln 77: "The propagation of the annual wave with depth is subsequently studied."

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C13: REVIEWER'S COMMENT:

Ln 92: "hereinafter"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C14: REVIEWER'S COMMENT:

Ln 102: "information on"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C15: REVIEWER'S COMMENT:

Ln 103: "short grass that changes minimally during"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C16: REVIEWER'S COMMENT:

Ln 133: "easier to detect"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C17: REVIEWER'S COMMENT:

Ln 133: "physically implausible"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C18: REVIEWER'S COMMENT:

Ln 141: "is consistent with a conductive process?

AUTHORS' ANSWER:

The sentence has been modified following the reviewer's indication to: "Note the decrease in high-frequency variability with subsurface depth, which is consistent with ~~conductive law~~a heat conduction process (Carslaw and Jaeger, 1959)."

R1C19: REVIEWER'S COMMENT:

Ln 144: "of one such"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C20: REVIEWER'S COMMENT:

Ln 183: "This CA framework"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C21: REVIEWER'S COMMENT:

Ln 201: "Because the periodogram calculation"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C22: REVIEWER'S COMMENT:

Ln 208: "To prevent poor signal-"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C23: REVIEWER'S COMMENT:

Ln 209: "from biasing the analysis"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C24: REVIEWER'S COMMENT:

Ln 214: There appears to be some inconsistent reasoning at this point. It is indicated a few sentences above that the CA method is used to estimate the near-surface soil thermal diffusivity values, which are then used in the SpM framework. It is also argued that the SpM method can be used to analyze shorter records, relative to the CA method. It is therefore not clear how this latter fact can be true if a CA estimate is used in the SpM approach. I trust there is an explanation for this, but the way it is written in the manuscript it gives the impression of a contradiction.

AUTHORS' ANSWER:

The SpM approach can be applied regardless of the length, the time frequency or the depth of the subsurface temperature records. However, as it is explained in the methodology section, the spectral attenuation curves are quite noisy at the high frequencies, so the authors included the e^2-fold decay strategy to cut-off the curve at a certain frequency and filter out these noisy signals. In the manuscript, this cut-off frequency was calculated from thermal diffusivity values coming from the CA, yet the same could be done assuming a realistic value of the soil thermal diffusivity (e.g. 10^{-6} m^2/s). The following plot (Fig. R1) shows how this would work for three values of thermal diffusivity (0.6, 1, and 1.4 10^{-6} m^2/s) for three-hourly and daily near-surface temperature data coming from HRR (the same used in Fig. 11 in the manuscript). The thermal diffusivity estimates slightly increase when assuming higher a priori diffusivity values for the cut-off frequency, but changes are very small in comparison to the range of unceirtainty of CA diffusivity estimates within the first meter of the soil (see Table 5). This demonstrates assuming a certain a priori thermal diffusivity for computing the e^2-fold decay cut-off frequency is perfectly feasible, rendering the SpM-based thermal diffusivity estimates completely independent form CA-based ones. One possibility would be to include in the manuscript the attached graph as a response to the reviewer. However, the authors believe it is more appropriate to keep the current figure in the main document (Fig. 11 a,b,c,d), since this figure better illustrates the consistency between the CA and the SpM. A piece of text was added instead to clarify this aspect in the Methodology section: "To determine this frequency at every site and level, near-surface soil thermal diffusivity values coming from the CA are used. However, it is possible to use a physically plausible prior thermal diffusivity value (ca. 10^{-6} m^2s^{-1}) to calculate this frequency, leading to very similar results and thus making the SpM independent from the CA." (see line 220 of the annotated manuscript)

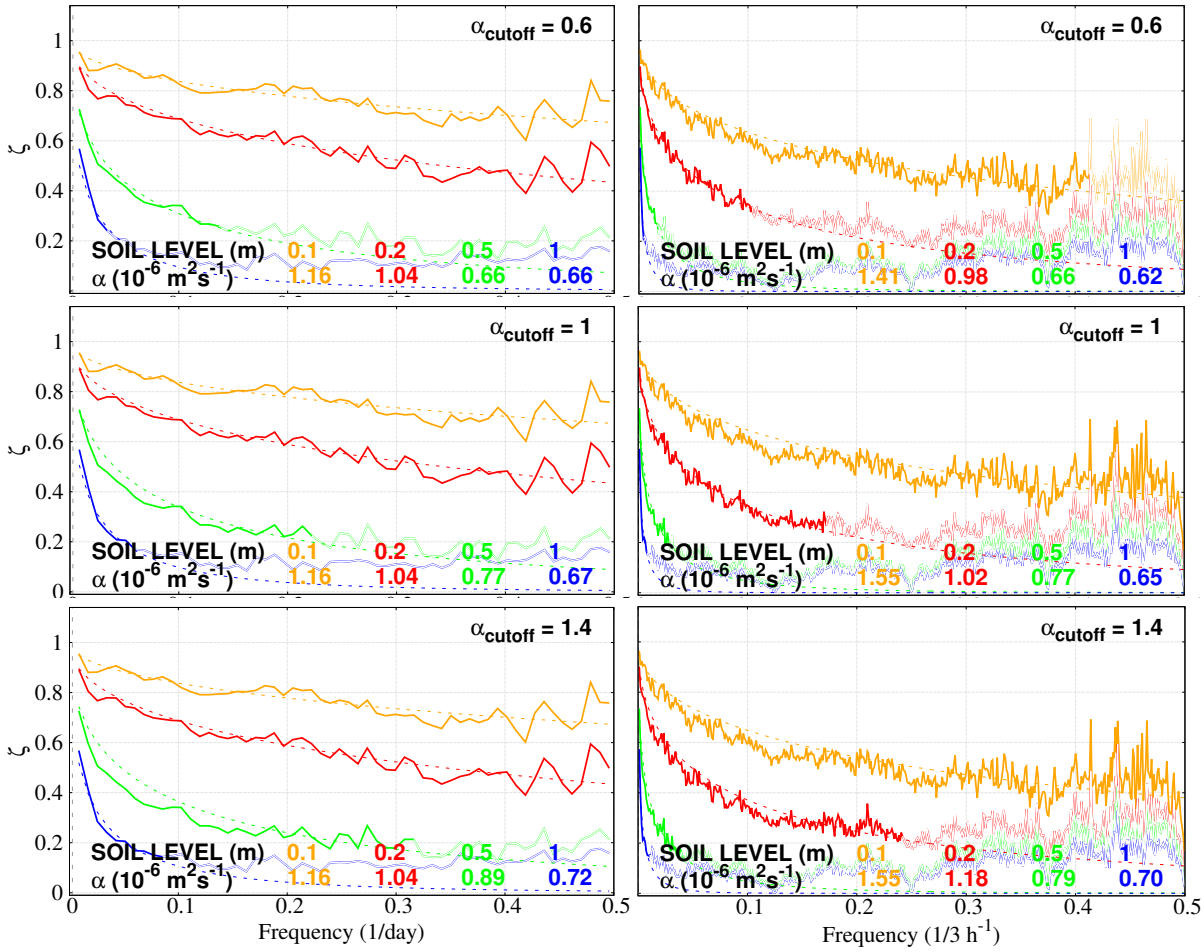

Figure R1. SpM analysis of subsurface temperature attenuation at HRR for daily (left), and 3-hourly (right). The hollow parts of the observed attenuation curves were excluded in the estimation of apparent thermal diffusivity, according to the e^2-fold decay criterion explained in Section 3. In this case, a priori thermal diffusivity values of 0.6 (above), 1 (center), and 1.4 (below) $10^{-6}\ m^2/s$ were used to derive the cut-off frequency.

R1C25: REVIEWER'S COMMENT:

Ln 217: "allows some insight"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C26: REVIEWER'S COMMENT:

Ln 222: The BRH 20m and associated notation is confusing because it gives the impression of different sites instead

of different measurements at the same site. I suggest using subscripts to make this clearer, i.e. BRH$_2$0m, BRH$_2$m, etc

AUTHORS' ANSWER:

BRH 20 m, BRH 2 m, and TRCH 1 m certainly refer to different and independent installations for each of the sites. Therefore, the authors have specifically used this notation to prevent the reader from interpreting BRH$_{20m}$ and BRH$_{2m}$ as different subsurface temperature logging depths from the same sensor.

R1C27: *REVIEWER'S COMMENT:*

Ln 257: The authors appeal here to the influence of a pine forest at the RPI site. They have nevertheless given the impression in the multi-site description that the vegetation conditions are similar. This should be rectified and gives the impression of a contradiction of what was said earlier, namely the vegetation is roughly the same at the sites and does not play a role.

AUTHORS' ANSWER:

Even though the vegetation within the fenced perimeter of the sites is very similar, as specified in line 106 of the revised manuscript, there are differences in the distant surrounding canopy. Even so, the statement in line 257 was speculative and the authors do not have solid argument to support it. Moreover, this switch in mean temperatures with depth can only be explained by processes taking place within the soil. Therefore, they have decided to eliminate the statement about the influence of the pine forest, following the reviewer's indication, and modified the sentence to: "However, this pattern is not observed at RPI and CTS, as RPI shows lower temperatures below 5 m despite SAT and GST being warmer~~. This might be the result of the interaction between enhanced radiative warming and low ventilation in JJA due to the fact that RPI is surrounded by the canopy of a pine tree forest.~~, which might be due to processes within the soil that have not been further investigated in this paper." (see line 267 of the annotated manuscript)

R1C28: *REVIEWER'S COMMENT:*

Ln 270: Stronger and weaker is vague here and the authors go on to indicate they are talking about amplitudes. They should just say the latter, i.e. the amplitude of one frequency is diminished relative to the amplitude in the other.

AUTHORS' ANSWER:

From what it is indicated by the first reviewer here, and in the second reviewer's comments R2C5 and R2C6, it seems the message and construction of this paragraph were not clear. It has been modified following R1 and R2 indications to: "On the other hand, note that SAT variability differs from one site to the other, being the amplitude of the daily cycle ~~stronger~~greater and the annual cycle ~~weaker~~smaller at CTS than at HRR, as illustrated by ~~a more prominent~~

~~*amplitude (i.e. greater differences)*~~*greater differences of SAT variations in July 2019 with respect to those in January 2019." (see line 282 of the annotated manuscript).*

R1C29: REVIEWER'S COMMENT:

Ln 297: The authors argue that the obs are outside the range of the values estimated from the analysis, but they only include the mean estimates. I don't think this statement is exactly true if you consider the uncertainty ranges of the estimated diffusivity values.

AUTHORS' ANSWER:

If the wide interval of confidence for thermal diffusivity values stemming from applying the CA at NVC is taken into account, the mean thermal diffusivities of quartz and feldspar are certainly inside the estimated range of values. This is not the case for SGV, whose apparent diffusivity estimates are below these values. The statement has been corrected following the reviewer's indication to: "The observations at ~~NVC and~~ SGV render values ~~out of~~that fall below this range, with apparent diffusivity values between 0.5 and ~~1.7~~0.8 10^{-6} m^2s^{-1}." (see line 311 of the annotated manuscript).

R1C30: REVIEWER'S COMMENT:

Ln 298: "refer within this first meter of depth to soil material" Something has gone terribly wrong with this section of the sentence and I don't know how to fix it.

AUTHORS' ANSWER:

The original sentence "The shallower observations refer within this first meter of depth to soil material and sediment present in TRCH 1 m rather than the gneiss (granite) characteristic of the other sites at depth" is certainly misleading. It meant to indicate that for the specific sites of NVC and SGV, only temperature at the first meter of the subsurface are available, so apparent diffusivity values only represent thermal diffusivity at that range. As shallow subsurface is just soil, diffusivity values should differ much from the other sites, where bedrock is extensively accounted for (down to 20 m). To make it clearer, the sentence has been rephrased following the reviewer's indication to: "~~The shallower observations refer within this first meter of depth to soil material and sediment present in TRCH 1 m rather than the gneiss (granite) characteristic of the other sites at depth~~While subsurface temperatures at CTS, HYS, RPI, and HRR extensively cover the soil and bedrock depths, NVC and SGV only include data within the first meter of the soil, which can yield very different thermal diffusivity estimates with respect to the other sites" (see line 312 of the annotated manuscript).

R1C31: REVIEWER'S COMMENT:

Ln 313: "Fig. ??"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C32: REVIEWER'S COMMENT:

Ln 320: The authors use a change point characterization of their trends, but they are not specific if this analysis was done objectively or by eye. They should clarify how they determined their changepoints and note that there is undoubtedly a lot of uncertainty in the changepoint estimates. This is important given the emphasis they place on the comparison between the changepoint value and the depth of the stratigraphic change.

AUTHORS' ANSWER:

The change point characterization was achieved using the two-phase regression analysis, and it is therefore objective. This method finds the combination of parameter values a0, b0, a1, b1, and c that minimizes the root mean squared error, where a0, a1 are the intercepts, and b0, b1 are the slopes of the first and second segments, respectively; c is the change point. A clarifying comment on this has been included in Section 3 to address the reviewer's indication: "This CA framework will also be used to estimate changes in apparent thermal diffusivity with depth, either by considering changes between pairs of subsurface levels or ~~using a two-phase regression analysis (Solow, 1987, 1995; Solow, 2018) to identify~~identifying depths where significant changes in apparent thermal diffusivity occur. This was achieved using a two-phase regression analysis (Solow, 1987, 1995; Melo-Aguilar et al., 2018), which determines whether there is a change point for which linear fits to the segments before and after the change point significantly improve the results compared to a linear fit to the complete curve." (see line 190 of the annotated manuscript)

R1C33: REVIEWER'S COMMENT:

Ln 322: "porous non-consolidated material"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C34: REVIEWER'S COMMENT:

Ln 339: "aforementioned"

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R1C35: REVIEWER'S COMMENT:

Figure 5: I find the legend for this figure very hard to follow. The figure is split in half and the colored site identifiers are split into the halves making me think they are designating the different groups of profiles. The colors also change

in saturation and it is very confusing to determine how they are all related. The authors should rethink how they have done this, with the understanding that text in the caption should not be depended upon for understanding which lines are what.

AUTHORS' ANSWER:

Figure 5 has been modified, according to the reviewer's indication. The legend has been rearranged, so now she "Annual" and "DJF/JJA" seasonal tags better accompany the profiles in each case. Colored tags indicating the site of each profile have been moved to the bottom right corner.

2 Anonymous Referee 2

GENERAL COMMENTS:

R2C0: REVIEWER'S COMMENT:

The authors have addressed all of my concerns on the manuscript in a strong response to the reviewers and a thorough revision. The novelty and impact of this study is much more apparent in the revised manuscript. I only have a few specific comments that will need to be addressed before publication.

AUTHORS' RESPONSE:

The authors welcome the positive perspective of the reviewer on the response to the revision. We are grateful for the reviewer's minor technical comments. Please find below the comprehensive point-to-point response to your review.

SPECIFIC COMMENTS:

R2C1: REVIEWER'S COMMENT:

Throughout the paper has mixed tenses. The Abstract and much of the Introduction of your study is present tense, but the Methods have both present and past tense. It would be better to have past tense throughout.

AUTHORS' ANSWER:

Many of the verbs in present tense in the abstract, introduction, data, and methodology sections were changed to past tense to keep consistency, following the reviewer's indication.

R2C2: REVIEWER'S COMMENT:

Line 23 – 'have'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R2C3: REVIEWER'S COMMENT:

Line 68 –'Previous research has assessed…'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R2C4: REVIEWER'S COMMENT:

Line 177 – for consistency to line 170 '2 m air' would be clearer as '2 m above the ground surface'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication, adding the word "temperatures" in front to specify which variable is being compared: "The SAT-GST seasonal coupling is explored by quantifying the offset and correlation of seasonal (December-January-February, DJF; June-July-August, JJA) and annual ~~2 m air~~temperatures at 2 m above the ground surface with GSTs taken from the top level temperatures in the BRH 2 m set up" (see line 164 of the annotated manuscript).

R2C5: REVIEWER'S COMMENT:

Line 296 'being the stronger daily cycle and weaker annual cycle…'

AUTHORS' ANSWER:

From what it is indicated by the second reviewer here, and in the first reviewer's comment R1C28, it seems the message and construction of this paragraph were not clear. It has been modified following R1 and R2 indications to: "On the other hand, note that SAT variability differs from one site to the other, being the amplitude of the daily cycle ~~stronger~~greater and the annual cycle ~~weaker~~smaller at CTS than at HRR, as illustrated by ~~a more prominent amplitude (i.e. greater differences)~~greater differences of SAT variations in July 2019 with respect to those in January 2019." (see line 282 of the annotated manuscript).

R2C6: REVIEWER'S COMMENT:

Line 297 – 'by greater differences of SAT variations'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R2C7: REVIEWER'S COMMENT:

Line 336 – delete ', for instance'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R2C8: REVIEWER'S COMMENT:

Line 340 – (Fig. 8; Solow, 1987, 1995).

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R2C9: REVIEWER'S COMMENT:

Line 408 – suggestion is – 'in interactive changes between soil material, texture, moisture and hydrology'.

AUTHORS' ANSWER:

The text has been changed according to the reviewer's suggestion: "As it has already been mentioned, soil properties are especially heterogeneous near the surface, due to interactive changes in soil material ~~and~~, texture (Cermak et al., 2017) ~~and hydrological (i.e. soil moisture content) changes~~, moisture and hydrology (Pollack et al., 2005; Gao et al., 2008; Tong et al., 2017)." (see line 392 of the annotated manuscript)

R2C10: REVIEWER'S COMMENT:

Line 413 and Line 467 –' an intra-monthly'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

R2C11: REVIEWER'S COMMENT:

Line 415 – change 'how' to 'that'

AUTHORS' ANSWER:

The text has been changed according to the reviewer's indication.

References

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