

Our response to Referee 1 comments on 31.01.2022 (<https://doi.org/10.5194/bg-2021-294-RC1>)

Title of the manuscript: Improving the stomatal resistance, photosynthesis and two big leaf algorithms for grass in the regional climate model COSMO-CLM

Dear authors,

The subject of the manuscript is for sure very relevant for the climate modelling community dealing with land-surface processes and their interactions with the atmosphere. However, I'm sorry to conclude that this manuscript does not fulfil the expectations I have on a scientific documentation of theory, experiments and results. The reason is partly that the English language is now at such a level where it becomes difficult to actually understand what the authors wish to describe in some background and results.

Answer: Ok, thank you for these comments. We corrected the English language and sent the manuscript to an English editing service. We hope that the manuscript is now clear in its meaning and the English in a good state. Please see the revised manuscript.

The reasons are also that the structure, motivation and balance of the text and results are not satisfactory. For example, the Introduction section lacks clarity (see below), Section 2.5 "Statistical analysis" refers to details that are described in later sections. It is not clear what is the objective with Section 4 "Results and discussion".

Answer: Ok, thank you for pointing this out. We restructured our manuscript for a better readability. We clearly state the motivation of our study in the "Introduction" section now. Moreover, we added current problems of COSMO-CLM and the purposes of our research to the "Introduction" section. We checked the manuscript for a better balance and moved content to the appendix if the balance is exceeded. In particular, a part of the statistical approach in the "Methods" section is moved to the appendix. Please see the revised manuscript. The Section 4 "Results and discussion" is separated into a "Results" section only. We added the discussion to the conclusions and call this section "Discussion and conclusions", now.

From all presented experiments and results I expect to find some indication in the end on how these experiments are ranked with respect to performance, but no such ranking is reached, only a conclusion that experiments indicate in general improved performance compared to the reference experiment.

Answer: Ok, the reviewer raises a good point, and we agree that a ranking is necessary, which need to be discussed. We added ranking and performance measures now to the manuscript, which are discussed. For example, the results show that experiment CCLMv4.5 ranks best based on the root-mean-squared-error and bias compared to the GLEAM data set. We also conclude this from the KGE and DAV performance indices. Experiment CCLMv4.5 has an updated algorithm for stomatal resistance and leaf photosynthesis based on the Community Land Model 4.5. This algorithm has a regulation function of night values of stomatal resistance depending on soil water stress function that is more realistic and distinguishes experiment CCLMv4.5 from CCLMv3.5 or CCLMv4.5e.

Thus, there is no balance between all presented details including the statistical analysis and the overall outcome of the results. Therefore, as an overall judgement I must recommend major revision. Both language level and structure, motivation and balance need major improvements in my opinion. The background and motivation for this manuscript as given in the Introduction

is not clear enough as it is written now. For example, in lines 40-42 you state that “the evapotranspiration simulated by ... TERRA-ML ... was found to be systematically underestimated from April to October during the growing season.” But you give no reference to this statement, and it is not clear over which region or regions this conclusion refers to. Is it Europe only or also other regions? Are there no publications available where this underestimation is shown? You refer to published evaporation and transpiration fractions, but I miss any comment on how these fractions are estimated by TERRA-ML. Later on (lines 51-52) you state that “plant transpiration is calculated in current version of TERRA-ML with errors (Stockle, 2001)”. Here it would be good to also say what kind of errors you mean. I would also say that even if TERRA-ML, now based on empirical stomatal conductance parametrization, would have given good evapotranspiration in validations of hindcast simulations it can still be motivated to introduce a more advanced stomatal conductance formulation since an empirical formulation may not be valid in changing climate conditions including rising CO₂ levels. But if you wish to motivate your work based on bad performance you need to show this bad performance more clearly. Overall, the Introduction section now gives a bit jumpy feeling between very overview style paragraphs (lines 38-38, 53-63, 70-74) and on the other hand very TERRA specific comments (lines 40-42, 48-53, 63-68). Also, all the version details in lines 75-83 do not clarify much. I would recommend that you revise the Introduction to find a better balance between background, TERRA details and motivation for your work.

Answer: Ok, we thank the reviewer for this thoughtful comment. Major changes are introduced for the “Introduction” section. We agree that the current TERRA-ML version based on empirical stomatal conductance parameterization gives correct values of evapotranspiration in validations of hindcast simulations. Nevertheless, COSMO generally underestimates evapotranspiration. We added this to the “Introduction” section and added the references (Shrestha and Simmers, 2019, Regensass et al., 2021). But as the reviewer mentioned an introduction of a more advanced stomatal conductance formulation is necessary since an empirical formulation may not be valid in a changing climate including rising CO₂ levels. We made this point now more clear in the “Introduction” section. Further, we have rewritten the “Introduction” section and hope that the background, TERRA-ML and motivation of our work have a better balance to the reader now. Please see the revised “Introduction” section in our manuscript.

We rewrote the sentence from lines 40 – 42: “However, the evapotranspiration simulated by the multilayer land surface scheme TERRA-ML of the Consortium for Small scale Modelling – COSMO (<http://www.cosmo-model.org/>, last access: 09 September 2021) was found to be systematically underestimated from April to October during the growing season” and combined this phrase with the phrase from Line 49. The phrase from Line 49 was deleted. We write now: “Nevertheless, the evapotranspiration simulated by the multilayer land surface scheme TERRA-ML of the Consortium for Small-scale Modelling (COSMO. <http://www.cosmo-model.org/>, last access: 03 February 2022) was found to be systematically underestimated based on the averaged diurnal cycle of evapotranspiration over Europe during the growing season for the vegetated land surface (Schulz et al., 2015; Shrestha and Simmers, 2019).”

The sentence from the Lines 51 – 52 “However, the plant transpiration is calculated in current version of TERRA-ML with errors (Stockle, 2001), which are related to the simplified parametrization scheme for stomatal conductance (g_{st}) or its reciprocal – stomatal resistance (r_s). The new one is: One of the possible causes of underestimated evapotranspiration is that

transpiration in TERRA-ML is calculated with inaccuracy due to the simplified stomatal resistance parameterization scheme.

Detailed comments:

Line 30: In my mind, for such a very general statement like “The land surface processes significantly affect the conditions in the low-level atmosphere” it is more appropriate and respectful to refer to well recognized reviews in the area like e.g. Betts et al. 1996 than to one’s own very recent paper.

Answer: Ok, thank you for this information, it is a good idea. We added the Betts et al., 1996 work to our manuscript.

Line 49: What do you mean by “not sufficiently represented”? Please be more specific.

Answer: Ok, we agree. The phrase “One of the possible reasons of the underestimation of evapotranspiration is connected with the fact that in TERRA-ML the vegetation is not sufficiently represented in the surface energy balance (Schulz et al., 2015).” was deleted. Also, the sentence from Lines 40 – 42 was deleted. We combined these two deleted phrases and now we have a new one. This sentence is presented in the previous answer to the question about Lines 40 – 42.

Line 52: “Stockle” should be “Savabi and Stockle”.

Answer: Ok, Thank you for this comment. We adjusted the reference publication.

Lines 73-74: I find the sentence and statement “However, these schemes have not been implemented into production (exploitation) at convection-permitting scale” a bit strange. Okay, so you mean that dynamic vegetation should be implemented just because it is missing or for some other reason? Please be more specific.

Answer: Ok, the paragraph was rewritten. We think that a more advanced stomatal conductance formulation is necessary since an empirical formulation as it is now implemented in COSMO-CLM may not be valid in a changing climate including rising CO₂ levels. The implementation of our new algorithms was guided by the ideas and published materials (e.g.: documentations, model codes) of several existing dynamic vegetation models such as: CARAIB (Dury et al., 2011), Community Land Model (Oleson et al., 2010 and 2013), SURFEX (Le Moigne, 2018) and CHTESSEL (Nogueira et al., 2020). Special attention was paid to the successful examples of the CLM implementation into different regional climate models, for example into the WRF model (Van Den Broeke et al., 2017) or into COSMO-CLM (Davin et al., 2011; Davin and Seneviratne, 2012). The last version is called COSMO-CLM² and the main focus of this version is coupling to different models, since COSMO-CLM (v4.8) and CLM3.5. In their work (Davin et al., 2011; Davin and Senevirante, 2012) have coupled COSMO-CLM with CLM and found improvements with respect to land surface fluxes, including an improved magnitude of radiation fluxes and a better partitioning of turbulent fluxes, but the multi-layer soil model TERRA-ML used in COSMO-CLM was fully replaced in COSMO-CLM² with the CLM3.5 parameterization scheme. The COSMO-CLM² was created and tested, but Davin et al. (2011) did not perform the convection-permitting scale simulations (Prein et al. 2015), due to high computational costs (Stöckli et al., 2008 and 2011). All our improvements have been directly implemented in TERRA-ML of COSMO-CLM that allowed us to improve TERRA-ML and save all the advantages of COSMO-CLM (for example, convection-permitting scale). These

changes distinguish our research from the research of (Davin et al., 2011; Davin and Seneviratne, 2012) for coupling the COSMO-CLM and CLM models.

Line 85: It is not clear now what “these limitations” exactly refer to. Please be more precise.

Answer: Ok, we added more information about limitations (e.g. empirical approach, no connection between carbon dioxide and stomatal resistance, etc.) and change the orders of the sentences in the manuscript making the phrase “these limitations” more appropriate.

Line 115: The formulation “atmospheric parameters under the soil” is probably not correct I assume.

Answer: Ok, the reviewer raises a good point. The new sentence is: The surface and soil processes are calculated in the multi-layer soil model TERRA-ML (Schrodin and Heise, 2002) consisting of two parts. The first one considers hydrological processes including snow melting and freezing. The second one includes algorithms intended for calculations of bare soil evaporation and plant transpiration, which are computed for non-vegetated and vegetated areas, respectively.

Line 123: Hmhm, just wonder if the factor L_d , representing Leaf Area Index, in Eq 58 in Dickinson et al. (1993) is missing here or it is just a different definition of transpiration?

Answer: Ok, Thank you for this comment. We added the new, more detailed description of the COSMO-CLM algorithm for transpiration. The new paragraph looks like:

The BATS-based formulation of the plant transpiration is presented in Eq. (1):

$$T_r = f_{plnt} (1 - f_i) (1 - f_{snow}) E_{pot}(T_{sfc}) r_a (r_a + r_f)^{-1} \quad (1)$$

where T_r is plant transpiration, r_a and r_f are atmospheric and foliage resistance, respectively, f_{plnt} , f_i , f_{snow} are fractional areas covered by plants, intercepted water, and snow, $E_{pot}(T_{sfc})$ is potential evapotranspiration. In accordance with the official documentation of COSMO-CLM model (Doms et al., 2018) stomatal resistance is a part of foliage resistance, which is equal to:

$$r_f^{-1} = r' C_F \quad (2)$$

where r' is reduction of transpiration by stomatal resistance equal to $r' = r_{la} (r_{la} + r_s)^{-1}$, C_F is parameterized by $C_F = f_{LAI} r_{la}^{-1}$. f_{LAI} – is the leaf area index, The detailed formulations of stomatal resistance algorithms are presented in next subsection 2.2.

Line 124: In Eq 1 it says “Tr” but here “Trk”. Please make it consistent.

Answer: Ok, we agree. The parameter name was corrected to “Tr”.

Lines 169-171: Although it is very precise to divide the text in “Current” and “New” formulation subsections it is from a stylish perspective a bit awkward when the “Current” section is represented by only one sentence. Therefore, I would recommend to remove the subsections here. The same comment is valid for Section 2.4.

Lines 184-187: See comment for lines 169-171.

Answer: Ok, Thank you for this note. We think that this division the Method section on “Current” and “New” formulations is important and allows to understand more clearly the

differences between methods. We also agree with the Reviewer, and we updated the section “Current”. *The new subsection “Current” for leaf photosynthesis is:*

In the current model version of TERRA-ML, there are no algorithms for estimating leaf photosynthesis. In the reference version of COSMO-CLM model, this algorithm is not needed for calculations and plants are represented by the following vegetation parameters, which are read in by the model as external 2D fields coming from remote sensing data. The vegetation parameters, which are read in, are leaf area indexes, plant coverage, minimum stomatal resistance, root depth and roughness length.

The new subsection “Current” for radiation fluxes is:

In the current version of COSMO-CLM a canopy layer is presented as a “one-big leaf”. In this approach, all leaves of the canopy have the same plant physiological properties and relative responses to the environment as any single unshaded leaf in the upper canopy. Additionally, in COSMO-CLM there are several assumptions simplifying this approach. The first one is water vapor flux between the plant foliage and the canopy air is equal to the flux between air inside and air above the canopy ($T_v = T_g$). The second one is the foliage temperature to be equal to the surface temperature (Doms et al., 2018).

Line 196, Eq 8: Please replace “sun” with “sha”.

Answer: Ok, we agree. The equations were corrected

Lines 287-292: Very complicated paragraph where I assume the main message is simply “Gridded observational data sets (E-OBS, HYRAS, GLEAM) were interpolated to the COSMO-CLM grid for comparison.”, right?

Answer: Ok, we thank the reviewer for this thoughtful comment. The paragraph was corrected the new one is: As an additional instrument for validating model results with the new formulations, we compared COSMO-CLM results with the gridded observational data sets. It allowed us to get more precise statistical scores because of the models and gridded observational data sets represent average values than processes in specific points (Osborn and Hulme, 1998). In the analysis, we used the gridded data sets with information about precipitation, temperature, and evaporation for validation of COSMO-CLM parameters. The gridded observational data sets (E-OBS, HYRAS, GLEAM) were interpolated to the COSMO-CLM grid for comparison.

Lines 316-349: I don’t see the point to spend a considerable part of the discussion on how values look for the inactive vegetation periods (wintertime and night-time). In my mind the most interesting part is how they differ during summer daytime. But this part cannot be analysed by these figures since one cannot distinguish any differences due to the y-axis scale. I would recommend to focus your analysis more on the summer daytime part.

Answer: Ok, we thank the reviewer for this thoughtful comment, we agree with it. All stomatal resistance plots were recalculated in accordance with this comment. The figures 2, 3 were updated. In addition, we have added additional information about stomatal resistance data from TRY database over Germany. The other plots are also updated with the main focus on changes during summer months.

Lines 351-371: You start the paragraph by concluding that “stomatal resistance ... is a highly intermittent phenomenon, extremely localized on the leaf level, and varies with leaf positioning on a plant and from leaf to leaf and from plant to plant” but then you compare your model results with observations from literature based on “located in the North America region” with no further comments on if these observations can at all be considered to be representative for your model results. Thus, this first sentence and your final comparison does not make sense to me.

Answer: Ok, the reviewer raises a good point in these 2 comments. We agree that the main focus in Section 4.1 should be shifted to the analysis of summer day-time values of stomatal resistance, because of that all plots were updated, table 3 – recalculated. The text of the section was rewritten. Also, we added more information about published data from North America and we assume that our experimental stomatal resistance data for C₃ grass can be compared with the in-situ data published earlier due to several causes: 1) Vegetation in published data is presented by grass and includes the *Lolium perenne* species; 2) The North American regions presented in the research situated similar climate conditions. The phrase “stomatal resistance ... is a highly intermittent phenomenon, extremely localized on the leaf level, and varies with leaf positioning on a plant and from leaf to leaf and from plant to plant” was corrected and the new phrase is “Stomatal resistance validation of the reference and experimental results presented in timeseries format is a formidable task. Due to measuring stomatal resistance (conductance) is a resource-intensive task, especially for its continuous quantification over time and there are no long in-situ time series or datasets including daily stomatal resistance data.”

Lines 374-421 (Section 4.2 and Figures 4-5): The comparison between model results and GLEAM datasets in Figure 4 shows that the difference between the GLEAM datasets are often as big or bigger than the differences between the model versions, especially for AEVAP. Thus, in my mind it is difficult to draw any further conclusions from this comparison other than perhaps that ZVERBO for the new model versions is better than CCLMref. The statistical analysis with all numbers presented is not necessary to reach this conclusion I would say. And the analysis gives no indication on which of the new model versions are better or worse, right?

Answer: Ok, thank you for this comment. The all experiments were recalculated, the figures replotted. The main focus changed to the analysis of changes between the algorithms from May to September.

Lines 423-437 (Section 4.3 and Figure 6): As for the section on “Evapotranspiration and evaporation” the statistical analysis with all detailed numbers of sensible and latent heat fluxes is not needed to reach your conclusion (visible from the figure) that “experiment results are similar to the CCLMref data”. Thus, in my mind unnecessary long details for this conclusion.

Answer: Ok, thank you for this comment. Yes, it is true that difference in latent and sensible heat fluxes between the experiments and the reference is small. Nevertheless, the experiments show slightly better results, and we think that it should be demonstrated in the manuscript. However, this Section 4.3 was relocated to the Appendix. We corrected the text in the Results section in accordance with the section results. Also, the text and figures of this section were rewritten and replotted. We spend more attention on the analysis during the months May to August, where vegetation is in its active phase.