

Interactive comment on “Modelling the diurnal and seasonal dynamics of soil CO₂ exchange in a semiarid ecosystem with high plant-interspace heterogeneity” by Jinnan Gong et al.

Anonymous Referee #1

Received and published: 16 May 2017

Review of Gong et al. “Modelling the diurnal and seasonal dynamics of soil CO₂ exchange in a semiarid ecosystem with high plant-interspace heterogeneity”

Gong et al. present a model development and model validation study focused on simulating soil CO₂ efflux in semiarid soils. They have improved on previous models used for these ecosystems by incorporating abiotic processes related to lateral and vertical transport of CO₂ in heterogeneous canopies as well as biotic processes related to biocrust CO₂ production and photodegradation. They evaluate their new model against two years of site-based data from semiarid shrubland ecosystem in Yanchi, northwestern China.

This is an interesting and relatively new contribution to the modelling literature on this

C1

topic. The introduction is well laid out and clearly explains the context behind the work and the importance of including processes related to plant heterogeneity and biocrust dynamics in the model. From both the introduction and discussion it is clear the authors know the literature well and have a good handle on the gaps in understanding that need to be addressed. This is a comprehensive study with a number of interesting results.

Given this however, I would like to see some of the objectives framed as questions in the introduction, which would then be answered directly in the results/discussion. This would help to highlight the key points in the results section, link the results back to the context and would make the text less focused on a model description, parameterization and sensitivity study, although these aspects are important and described well in this paper. Such changes would serve to improve the structure, readability and scientific value of the paper. The authors could frame the work around questions such as: “What is the difference in diurnal to seasonal variability in CO₂ flux between soils with and without a biocrust?” “Are there significant differences between the CO₂ flux from plant covered and interspace soils?” “What are the relative contributions of different processes to total soil CO₂ efflux? Are the process of CO₂ production and emission tightly coupled during wetting and drying cycles?”

The paper would also benefit from a more thorough discussion of the importance of including processes related to biocrusts in regional to global scale biogeochemical models. Does the inclusion of biocrust-related processes improve the fit to the measured soil CO₂ flux at C3 compared to a model that does not include these processes (e.g. if you repeated the simulation without the inclusion of the new processes related to biocrust)? Does this represent a significant flux in semiarid ecosystem C balance at regional to global scales?

Finally, please see my comment below on the aim of the sensitivity study, other than to see how robust the model is to changes in parameters. However, in a more general context, I think it would be beneficial for the study if you put the sensitivity analyses in the context of climate and anthropogenic change? What are the likely changes in

C2

temperature and moisture for this region? And what is the implication for the carbon balance and viability of the vegetation of this ecosystem in the future?

MAIN SUGGESTIONS

Materials and methods

Figure 1: could you incorporate a small map showing the study region? I am sure readers would find that useful. Figure 1 also is not very clear unless I zoom in, therefore I think the size/resolution needs to be improved for reading on paper.

Line 127: You say 40% here but the value is 90% in the Gong et al. (2016) paper. Which one is correct?

Line 160: Please define PATCIS. Is it a model name, an acronym?

Section 2.2.3: for the sake of clarity/completeness, it would be good to have an extra equation here showing how all the flux components sum to provide the total net biocrust flux (FB) in equation (1).

I would have Section 2.3.1 as a separate Section (e.g. just 2.3) entitled “data” or “measurements”. It may not be immediately obvious that you would find a description of the data here in this section on model parameterization if you were just scanning through section headings.

Lines 308 to 309: it would be great to have pictures of these three sites to show readers new to the topic of biocrusts what they look like.

Section 2.3.3 (and throughout Section 2.3): For many parameters, there is a clear and adequate description for the functions used to derive them, but not all – some detail appears to be missing for some. For example, for lines 345-347: please could you give a little more detail on how the horizontal and vertical root biomass profiles were parameterized? For example, did the root biomass decrease linearly with distance from the center of the shrub crown? Another example for lines 351-352: how was

C3

the photodegradation coefficient calculated from the mass-loss rate. I am also a bit confused as to why only certain parameters are included in Table 2 and not all (e.g. why is the photodegradation coefficient not included for example)?

Lines 377 and 379: I think the 2nd $Q_{10}()$ in equation 28 should be $Q_{10}(\theta)$ and the same in equation 30?

How did you come to define equation 31 in this way? Based on the aforementioned studies? Which method did you use to perform the fits shown in Figure 2 and equations 32 and 33?

Section 2.4.1 title should mention the meteorological forcing data. A shorter title could be “model set-up”. Do you have a reference for the PECE method?

Lines 502-504: How/why did you choose which parameters to include in your sensitivity analysis?

Section 2.4.3: to avoid some confusion in the results later, I might have the parameter sensitivity as a separate test from the comparison between plant covered and interspace soil CO₂ flux (so add a test 5). I think this would help to emphasize the importance of the impact of plant heterogeneity in the text, given its importance in the paper title.

It might be useful for the reader to have a small table summarizing all the tests, which processes they include, which site they correspond to, what the observations are measuring etc.

Results

What is shown on the bottom of Fig 4a? Is that precipitation? It might be worth smoothing your hourly curves with a moving average window so we can see the variability better I would put precipitation on the hourly time series plots in Figure 5 as well.

Lines 540-541: It would be good to give the RMSE of C1 above as well for a compari-

C4

son.

Figure 6: I like the addition of the diurnal bias plots - they are very informative. I would put the same scale for all C1, C2 and C3 plots to enable an easier comparison between the tests.

Lines 542-544: Looking at Figure 6b it seems to me that the pattern of diurnal biases has changed for C3 compared to C2 and C1? There is now a positive bias around noon and a negative bias in the mid-morning and afternoon? Why do you think this is?

Table 3 caption: component fluxes. It would also be better to say "for areas with plant cover and without (interspace)"

Figure 7: You mention FS in the caption but FT in the text and figure legends. Also, you refer to net CO₂ sequestration by the biocrust in the legend – isn't this FB (or FCt), or have I misunderstood? It would be helpful to the reader to make sure all the abbreviations you use for the fluxes are uniformly used across the text and figures. In fact, I would suggest adding an extra table with all the component flux abbreviations and their long name/description, given that there are many. This may help the reader cross-reference between the figures, tables and text.

Table 4: are all the values listed the % change in C flux after manipulation compared to the base flux, or are some of the +/- values a change in the absolute C flux magnitude? Please detail this in the table caption.

Lines 578-583: I am a bit lost as to main message of the second part of the sensitivity analysis. What does the sensitivity analysis suggest about how important the parameters are? If changing the parameter values does not result in that much change to the fluxes, does that mean that parameter or even that process is not actually important for modelling the flux? How have you decided how much to change the parameter values? Perhaps it would be good to explore their full range (between their upper and lower bounds) in a proper sensitivity analysis (e.g. using the Morris method) in order

C5

to determine the full impact of the parameter values.

Discussion

Some of the results are repeated in the discussion. Given that the results section is very short, it might be better to merge the results and at least some of the discussion that is very pertinent to each particular result for each separate sub-section (e.g. validity of the results, ...) and separate out each with a sub-heading. That way the reader is not switching between different aspects of the modeling in the results before having to come back to consider the implications of these results in the discussion.

It is encouraging that the authors are aware and detail all the caveats of their work; however, the manuscript might benefit from a shorter, more concise discussion, particularly given the methods section is also (necessarily) long. A brief summary of the missing features of the model such as is given in the conclusions may be enough with a few extra sentences and references.

Be sure not to repeat sections of the introduction or results in the discussion, e.g. lines 726-744 is largely a repetition of context and results. I appreciate it is hard to keep the results and discussion separate, which is why I have suggested combining at least some parts of the discussion with the results in a "results and discussion" section. This would also help to reduce the length of the manuscript. Other more general parts of the discussion could be put in a final "Conclusions and future perspectives" section.

MINOR COMMENTS

In general: CO₂ production, not productions. Both some missing and unnecessary "the" in places (e.g. line 95, no 'The' is needed, and occasionally the plural of a word is used where it should not be (e.g. CO₂ productions). Check the text carefully.

Please could you explain this sentence more: "In dryland soils, the interactions between CO₂ transport and water cycle could also be intensive, due to the commonly high salinity/alkalinity of soils."? What do you mean by intensive?

C6

Line 104: “However, both models focus on the patterns at the regional-scale with very simplified ecosystem processes and neglect stand-scale heterogeneities of water-energy budget, and have not yet been validated by field measurements.” I would turn this sentence into a positive one to highlight what you will do to add to the field and incorporate that into your following paragraph. Something like switching this sentence to read “we will build on this work by including complex processes related to. . . . Furthermore, we have validated our new model with extensive field measurements. . . .”

Line 384: Sponseller, 2007 and Cable et al. references missing

Lines 409-411: Do you mean NPP and not NP?

Line 429: litterfall

Line 456: probability, not probably?

Line 464: “The model simulation employed half-hourly meteorological factors” “the model was run with half-hourly meteorological variables”

Line 496: “contributed to the soil CO₂. . .”?

Line 506: “It was also studied” “Furthermore, we studied”

Line 507: “regarding the” “due to”

Line 521: pronounced

Line 703: “should” instead of “shall”

Lines 705-709 reads more like “Conclusions”

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-95, 2017.