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Interactive comment on "Modelling the diurnal and seasonal dynamics of soil CO_2 exchange in a semiarid ecosystem with high plant-interspace heterogeneity" *by* Jinnan Gong et al.

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General comment "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 efïňĆux 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.

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This is an interesting and relatively new contribution to the modelling literature on this 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 scientiïňĄc value of the paper. The authors could frame the work around questions such as: (a) What is the difference in diurnal to seasonal variability in CO2 ïňĆux between soils with and without a biocrust? (b) Are there signiïňĄcant differences between the CO2 ïňĆux from plant covered and interspace soils? (c) What are the relative contributions of different processes to total soil CO2 eﬁňĆux? Are the process of CO2 production and emission tightly coupled during wetting and drying cycles? The paper would also beneïňĄt 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 ïňĄt to the measured soil CO2 ïňĆux 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 signiïňĄcant ïňĆux 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 beneïňĄcial for the study if you put the sensitivity analyses in the context of climate and anthropogenic change? What are the likely changes in temperature and moisture for this region? And what is the implication

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for the carbon balance and viability of the vegetation of this ecosystem in the future? ”

Response to general comment: We are very grateful for the efforts of reviewer on improving this work. Indeed, this paper has been organized as modelling-oriented. The main motivation is that, although there has been many studies and data on different flux components (they all seems very important somehow), there hasn't been any system that could integrated those knowledge, or a “playground” to explain the C dynamics or make extrapolations, e.g. to a different space-time or scenario. Therefore, model development became the primary objective. We do agree that, for results and discussion, better structuring is very much in need. Within the reach of this work, we have re-organized the analysis around two aspects, i.e. i) the roles of componential C processes in regulating soil CO2 effluxes, and ii) the plant-interspace differences in the C fluxes. Sections and paragraphs in results and discussion has been rearranged accordingly, and extra comparison (e.g. Table 5) has been added to aid the second question. We also performed a test as suggested, to see if exclusion of biocrust-related processes reduced the ïňĄt to the measured soil CO2 ïňĆux at C3 (Line 676). We hope these revisions could help the reader to better understand the emphasis and the findings. Please find the red marks in the supplementary file for those revised places.

We understand the excitement of reviewer to upscale and extrapolate the current results, for large scale, long term and general implications. Extrapolating the modelling for large scale and long term applications, and scenarios analysis for climate change and sustainable management are our future purposes as well. However, these discussions are largely out of the reach of current model. For example, as described in section 4.3, the growth dynamics of shrub vegetation is not included in modelling yet. Therefore, the changes in leaf area, shading and energy partitioning, evapotranspiration and root biomass are largely unknown and hard to set for longer term iterations, climate change scenarios or vegetation management. Our ongoing work addresses this issue and trying to complete the full picture of C-N cycle in such ecosystems. Then we might have a better stand for deeper discussions on those topics.

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MAIN SUGGESTIONS Materials and methods Figure 1: could you incorporate a small map showing the study region? I am sure readers would ïňĄnd that useful. Figure1 also is not very clear unlessI zoom in, therefore I think the size/resolution needs to be improved for reading on paper.

Response: We appreciate the comment and has separated Figure 1 to two different figures (Fig. 1 and 2). In Fig. 1, we added a map showing the study region (Fig. 1a), changed the site picture (Fig. 1b) for better quality, and added photos showing the soil collars (Fig. 1c - 1e). Model framework has been moved to Fig. 2.

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

Response: Both. This study was based on a different location from that in Gong et al. (2006). The two locations are about 1km apart.

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

Response: It is the name of model.

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

Response: We agree that the flux symbolism is somehow quite messy. Therefore, we have revised all the symbols and names to keep them consistent. FB actually should be FT in this case.

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 ïňĄnd a description of the data here in this section on model parameterization if you were just scanning through section headings.

Response: Good suggestion. We have separated that paragraph to section 2.3 named

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" Micrometeorological and soil CO2 efflux measurement".

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.

Response: The crust pictures have added to Fig. 1 (c - e).

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 proïňĄles 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 the photodegradation coefïňĄcient 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 coefïňĄcient not included for example)?

Response: That section (numbered as 2.4.2 in revised paper) has been checked and more information has been added. Photodegradation coefficient kp was indeed missing from Table 2 and now has been added.

Lines 377 and 379: I think the 2nd Q10() in equation 28 should be Q10(θ) and the same in equation 30? How did you come to deïňĄne equation 31 in this way? Based on the aforementioned studies? Which method did you use to perform the ïňĄts shown in Figure 2 and equations 32 and 33?

Response: The 2nd Q10() in equation 28 has been reivsed to Q10(θ). For equation 31, actually there were no available numerical descriptions on such an rain effect, therefore we decided to add one to the algorithm. This equation has been tested in sensitivity analysis (see test for parameter np), which shows this equation may not be an important source of uncertainty. Future modelling may also consider to simplify this algorithm (Line 609). Fittings in figure 2 (Fig. 3 in revised manuscript) were performed

by Matlab curve-fitting toolbox. The information has been added to Line 410 and 417.

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?

Response: The title has been changed as suggested (Line 322). Like forward/backward Euler, PECE method can be found in many textbooks related to ordinary differential equations, e.g. Butcher, John C. (2003), Numerical Methods for Ordinary Differential Equations, New York: John Wiley & Sons, ISBN 978-0-471-96758-3.

Lines502-504: How/why did you choose which parameters to include in your sensitivity analysis?

Response: The reason to choose the tested parameters has been better demonstrated in section 2.5.3.

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 ïňĆux (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.

Response: This suggestion has been taken with gratitude. We separated the contents as suggested, and organized the tests to two part: i) to demonstrate the roles of componential C fluxes in regulating surface efflux; and ii) to find how the plant cover and interspace are different in the flux rates and sensitivities. see Section 2.5.3-2.5.4.

Results What is shown on the bottom of Fig4a? 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.

Response: The figure 4 and 5 has been revised following the suggestions. 3-day moving average trends were used to show the temporal dynamics better.

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

Response: The figure 4 and 5 has been revised as suggested. 3-day moving average trends were used to show the temporal dynamics better.

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.

Response: The scales has been set to same in Fig. 5 and Fig.7.

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?

Response: Indeed, the pattern became different in C3 compared to C2 and C1. It is probably caused by biases from photosynthesis & photodegradation, which were introduced to system in Test 3. We have corrected the description in the manuscript (Line 560-563).

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

Response: Table 3 caption has been revised as suggested.

Figure 7: You mention FS in the caption but FT in the text and ïňĄgure legends. Also, you refer to net CO2 sequestration by the biocrust in the legend – isn’t this FB (or FCt), of have I misunderstood? It would be helpful to the reader to make sure all the abbreviations you use for the ïňĆuxes are uniformly used across the text and ïňĄgures. In fact, I would suggest adding an extra table with all the component ïňĆux abbreviations

and their long name/description, given that there are many. This may help the reader cross-reference between the ïňĄgures, tables and text.

Response: There have been several place with unnecessary naming which has complicated the whole thing. We have revised all the symbols and names to keep them consistent. Please see in the revised figure (Fig. 8).

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

Response: Yes all values listed in Table 4 are % changes. This point has been clarified in figure footnote.

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 ïňĆuxes, does that mean that parameter or even that process is not actually important for modelling the ïňĆux? 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 to determine the full impact of the parameter values.

Response: Yes the analysis of parameter sensitivity is to understand which parameter is more important and more likely to be the main source of uncertainty, as we have many site-specific parameters. Those parameter of high sensitivity then need to be use with extra cautious, when applying the system to another space-time. For site-specific parameters like Ts, θ, Mtot etc., we modified the values by the same degree ($\pm$10%), so that their effects on C fluxes are easy to compare. This is a bit different in purpose than the Morris method. On the other hand, "full" impact is difficult to define as well. Some parameters, e.g. root biomass, may vary by several folds from one collar to another (see Wang et al., 2015, Biogeosciences). Also an artificial parameter

setting (e.g. assuming extreme values for many parameters) may seem unreal (e.g. a combination of very low moisture content and very high root biomass). Therefore, performing an Morris analysis like suggested could be difficult, while the upper/lower bounds of parameters are unclear in a combination.

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 beneïňĄt 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 ïňĄnal "Conclusions and future perspectives" section.

Response: We greatly appreciate these advices. In order to better structure this part, we combined apart of the discussion on model validity with the result section 3.1, and let the rest discussion part (i.e. section 4.1 and 4.2) to focus on answering the two questions we proposed. The model uncertainties are discussed in the final section 4.3. Still, we would like to provide a deeper and more thorough discussion on model uncertainties and challenges instead of a general and brief one, in order to be more precise and informative about the problems we haven't solved, or those could be important to

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further studies.

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?

Response: Appreciated. We checked all possibly mistaken forms in revised manuscript. The "dryland soil" sentence has been revised, see Line 59-61.

Line 104: “However, both models focus on the patterns at the regional-scale with very simpliïňĄed ecosystem processes and neglect stand-scale heterogeneities of water energy budget, and have not yet been validated by ïňĄeld measurements.” I would turn this sentence into a positive one to highlight what you will do to add to the ïňĄeld 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 ïňĄeld measurements...”

Response: we have revised the part as suggested, see Line 107-109.

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

Response: Citation to Sponseller has been added. Cable et al. 2013 was removed.

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

Response: Yes, it should be NPP here. Corrected.

Line 429: litterfall Line 456: probability, not probably?

Response: Corrected.

Line 464: “The model simulation employed half-hourly meteorological factors” “the

model was run with half-hourly meteorological variables”

Response: Corrected as suggested (Line 468).

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

Response: Corrected (Line 499).

Line 506: “It was also studied” “Furthermore, we studied” Line 507: “regarding the” “due to”

Response: This paragraph has been revised (section 2.5.3).

Line 521: pronounced

Response: Revised as suggested (Line 536).

Line 703: “should” instead of “shall”

Response: This paragraph has been revised.

Lines 705-709 reads more like “Conclusions”

Response: This section has been revised.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-95/bg-2017-95-AC1-supplement.zip

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