

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.

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This study concerns the development and application of a highly detailed physically based patch-scale land-atmosphere energy, water and carbon balance model for a semiarid ecosystem with high plant-interspace heterogeneity. The model represents an expansion of the the model developed by Gong et al. (2016. Ag. Forest Met) that compared patch scale water and energy exchange into soil-plant C exchanges. The model represents most of the C stocks and fluxes that you expect to be relevant for dryland ecosystems, but which are not normally represented in ecosystem C models like photodegradation, biocrust photosynthesis and respiration, gas and liquid phase

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co₂ transport, etc. The model is used to simulate bare and plant shaded surfaces as well as biocrust covered surfaces and compared to measurements.

The model was shown to be very capable of accurately simulating measured soil temperatures, soil moisture and soil respiration (Rs). The main findings were that total CO₂ production in the soil and Rs could deviate substantially from one another due to root uptake, crust respiration and photosynthesis, and variations in CO₂ dissolution, emphasizing the processes beyond heterotrophic and autotrophic respiration and highly heterogeneous nature of CO₂ cycling in patchy ecosystems. These results shed some light on the importance of these other processes that are not commonly represented in ecosystem models as well as our ability to represent them in ecosystem models.

The paper is well written. The authors do a great job in discussing the background literature in the Introduction as well as tying their findings to previous studies in the discussion. The paper is very long, but this should probably be expected given the highly detailed modeling work that is being presented. Overall, there is nothing fundamentally flawed with the paper and I expect that this work will be of interest to ecosystem modelers, particularly those interested in dryland ecosystems.

My main complaint about the paper involves equifinality of the model results and the lack of data to be able to validate their findings on the relative roles of the different component fluxes. A model with far fewer parameters and processes is likely to be equally as capable of simulating soil moisture, temperature, and Rs for these cases or tests so how can one have much confidence that extra capabilities of the model (to represent the individual fluxes and transports like photorespiration, crust photosynthesis/respiration, CO₂ uptake by roots) are valid? Table 3 is great, but it could be entirely fictitious. While I'm excited to see models being built with these processes considered, I'm wonder how we can build confidence that they are any better than simple, more empirical models already out there.

A couple of relatively minor issues: 1. There could be better setup in the introduction.

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What are the objectives and rationale of this study? Major questions or hypotheses? 2. Figure 8 used as an estimate of photorespiration. How do you separate the effects of greenhouse effect under the clear chamber versus the shade effect of the opaque one? In other words, the opaque chamber shields the surface and reduces the heating when the chamber is closed. The clear chamber, by allowing solar radiation in and blocking thermal radiation out, is going to be heated much more potentially during the measurement cycle, potentially increasing heterotrophic respiration. Is R_s higher because of higher temps or because of photorespiration?

Text specific comments: L13. This sentence is an unusual way to open up an Abstract. I am wondering if it could be replaced with a sentence that provides context and rationale for the study L54. cannot L55. periods L58 intensive? Also, why would water and CO₂ transport be more intensive in the drylands? L62. Here's another paper with the relevance of abiotic C with fluxes on the diurnal time scale. Hamerlynck, Erik P., et al. "Nocturnal soil CO₂ uptake and its relationship to subsurface soil and ecosystem carbon fluxes in a Chihuahuan Desert shrubland." *Journal of Geophysical Research: Biogeosciences* 118.4 (2013): 1593-1603.. There are several papers out that seem to show that inorganic C uptake is unlikely to be a very significant flux...see e.g. review in Schlesinger, William H. "An evaluation of abiotic carbon sinks in deserts." *Global change biology* 23.1 (2017): 25-27. L69. matter L70. "could maintain inactive"? L77. Might consider H. Throop's work here, e.g., Throop, Heather L., and Steven R. Archer. "Resolving the dryland decomposition conundrum: some new perspectives on potential drivers." *Progress in botany*. Springer Berlin Heidelberg, 2009. 171-194. If you can't find this chapter, she has several articles about photodegradation. L79. periods L95 Define "global change" L104. "works" L114-115. This sentence seems out of place. If this represents an advance of Gong et al. you should cover what this model development is. L118. How about "Model Overview" L119. modeling work was based on measurements? L128. Don't understand this sentence L456 "probably"? L491. Later on, Test 4 is mentioned, but it should probably be included in this paragraph L503. component L521. "was more pronounced" L523-525. I don't know of many soil

water probes that are good at measuring frozen water content. Are you sure the measurements are valid during these times? L534 4b? L554. All the variables need to be clearly redefined in the Table caption so that this paragraph is much easier to understand. L565. Compared to L566 ,irrespective of the size... L573. "compared" L598. "our model capably reproduced the time series for the water and energy fluxes at ..."

L605 Suggest using another heading before this paragraph, something like "modeling uncertainties" L705. provides L708 caution L709 Our simulations showed that a L773. uptake L785 Are the model and data available for others to use ? Table 3. All terms need to be defined in this table caption including Fs, Fft PCt Figure. 1. The photo is really too small to see much of anything. Suggest deleting this so there is more space for the conceptual figure Fig. 4. Ppt is not labeled or given a scale Fig. 6. Greek letter is not defined in the caption. Fig. 7. This figure is very hard to see. Could you use more colors for the different symbols so that it is easier to see?

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