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Interactive comment on "Two perspectives on the coupled carbon, water, and energy exchange in the planetary boundary layer" *by* M. Combe et al.

Anonymous Referee #1

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OVERALL

This is a straightforward but rather limited study of L-A feedbacks combining water and energy with the carbon cycle in two diverse types of surface models coupled to a single column model. While the results are and discussion are physically sound, there is very little that could be considered new or unexpected here, and the experiments are very highly controlled including limited sensitivity studies. The novel approach lies in the use of two distinct land surface/crop model types being set up side-by-side to better understand their differing feedbacks with the overlying PBL. The bulk of this is shown in the final section of results, which represents the major contribution here. Certainly, there are a few nuggets that are of value that the community should be aware of regarding the competing feedbacks in drying soil and subsidence with respect to

carbon and the PBL. On the flip side, there appears to be a bit of extraneous material in the uncoupled runs that could be removed and keep the paper on focus a bit more. Overall, I feel this work should be published, but after considering the comments and suggestions below.

SPECIFIC COMMENTS

5278, L20-25: What is the result of the competing impacts on drought from this perspective: a) dry soils, so increased H and deeper PBL growth, vs. b) subsidence , which allows the soil to dry over time but at the same time decreases PBL growth. Is the subsidence that sustains drought actually a negative feedback on PBL growth that normally would help dry the soil further (by diluting the moisture and promoting more soil drying)? I see you get to this at the very end, but the idea of competing feedbacks could be introduced earlier and in the abstract.

5281, L15: Does the wind profiler provide a better estimate of PBL height than the Cabauw radiosondes? Under daytime/convective conditions as well?

L23: How big is the closure gap relative to previous ECOR studies and closure attempts in the literature? (e.g. 10-20%, or 5%, or 40%?)

L28: Is the Bowen ratio constant throughout much of this golden day? Would evaporative fraction be a better measure since it is fairly constant over the midday period?

5283, L18: How is 'if the water supply is sufficient' determined?

5284, L1-5: This is a single day study though, no? Could a lot of this be easily prescribed for this day (based on observations)?

5285, L1-5: There has been a lot of recent work on the G/Rn value and its sensitivity to LAI, crop type, and soil moisture. Often much larger than 10%, and often asymmetric during the daytime.

5286, L1-5: Is it possible to compare soil heat flux from each of the two models to see

if they are comparable (and how good the 10% estimate is)?

5287, L5-10: So is this explicit coupling (with land models and MXL at same timestep of 1 minute, same as the MXL physics?). Ok I now see A-g is at 1 minute, and GECROS is at 5 minutes. What are the physical and numerical implications of this?

L10-15: So, in essence, you performed an offline spinup for GECROS but not for A-g. Why not for A-g? Understood that GECROS is designed for seasonal crop growth, but in terms of getting the models in an equilibrated state before the coupled day simulations, it would help if they were both run (and forced identically) for a period leading up to the coupled run. Your specification of soil moisture might preclude some of this, but soil temperature still needs a sound vertical profile initialization.

L15-20: What is the soil type at this location? You can get good estimates from literature/tables on WP and FC based on soil type.

L20-25: Is this relationship actually linear? Just so I understand, basically 'tuning' the FC and WP values in the model to match the obs?

5288, L1-10: Ok good - so you are scaling the soil moisture such that it is consistent across models.

5289, L-15: This uncoupled GECROS work seems like an aside in this work. It isn't mentioned in the abstract or introduction as the focus there is on the coupled interactions. I can see why it would be run uncoupled to get a better initial condition for the golden day in terms of crop growth, but otherwise you may want to consider removing this component of the analysis. It can be mentioned, but not presented and I do not think the core results later on would suffer at all.

5291, L5-7: The idea that the uncoupled run does not perform well because it lacks atmospheric feedback may be flawed here. In an uncoupled land 'analysis' such as this you would usually expect better results (closer to obs) than an unconstrained fully coupled system because here you are forcing the model with best available observa-

tions. Aren't the observations used on the diurnal scale as well (so the model is being forced at the diurnal scale)?

L15: Nice result to validate the augmented GECROS model, but not central to the theme of this paper. Figs 1-3 and Table 1 could likewise be removed.

5292, L16: What exactly is satisfactorily?

L20-22: What if you did the reverse and prescribed with GECROS soil moisture? Might be worth an additional simulation or two.

5293, L6-10: Bowen ratio or Evaporative Fraction could be brought in here to ease the discussion on the fluxes.

5294, L1: Why is this? Not sure I follow regarding the surface layer.

5295, L5: Was this done for all experiments or just now/here? This should have been mentioned earlier in the setup (sorry if I missed it) if it is the former.

Also, it is now evident that you are really 'tuning' this model (soil moisture, SWdown, advection) to the observations and this is a very controlled setup. This potentially limits the greater applicability of these results, because it is unlikely these models would ever be run under these conditions and specifications again (they are far from 'free-running' at this point).

L19, 20: PBL height sensitivity is also a function of how MXL performs its turbulence calculations, and how PBL height is defined by the model or user, etc.

5295: Through the end of Section 3.2, Nothing surprising or new in the results to this point. IN addition, highly controlled experiment and tuned models make applicability very limited.

Section 3.3: Excellent, interesting, and important results.

5299, L25-30: Regarding water-stress response, it is widely known that the SM-ET

relationship is unique in most models. The critical soil moisture value at which above is freely evaporating vs. soil limited evaporation below is known to be a critical formulation (and in some models a simple parameter value). Look into the recent work of Koster et al. (2013/2014) which shows how the ET/SM response function is the most important component of a land model and what happens when it is altered.

Fig. 9: How was the entrainment estimated by the model (or was it derived based on profiles)?

Interactive comment on Biogeosciences Discuss., 11, 5275, 2014.