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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.

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Answer to Reviewer 2 of the manuscript (*bg-2014-123):* TWO PERSPECTIVES ON THE COUPLED CARBON, WATER, AND ENERGY EXCHANGE IN THE PLANETARY BOUNDARY LAYER.

First of all, we would like to thank Reviewer 2 for his/her valuable comments. We have addressed all the comments raised by the referee in the response point by point and introduced the corresponding modifications in the manuscript. Below, we repeat the Reviewer's comments in normal font. Our replies are in bold-face and changes in the original manuscript are in italic.

Overall:

The paper investigates crop surface process and links to the atmosphere using three models, focused on a single site and a single day. It sets out to investigate surface–atmosphere coupling effects on atmospheric CO_2 concentrations. This is an important topic, which should help interpretation of atmosphere data with respect to governing processes.

The paper sets out 2 science questions, but it never specifically answers these in the discussion. The questions also seem poorly linked. Q1 is a process question, and is linked to the most interesting outcomes of the study, re surface–atmosphere linkages. It would help to set out some hypotheses related to this question that could be tested. Such an approach would improve the structure and flow of the paper. It would also help to evaluate more than a single day of data and modelling. A more detailed set of sensitivity studies could then be used to show the nature of surface–atmosphere interactions. Q2 is a more technical question about modelling plant physiology, which is not really addressed in the paper (there are no plant physiological data presented or used to test the models, for instance). I would suggest that Q2 is dropped, and that a single LSM is used, rather than 2 compared. The evaluation of the 2 LSMs is incomplete and perhaps infeasible given their different initialisations and parameterisations.

We appreciate the very concrete suggestions of Reviewer 2 for modifications to our manuscript, but we note that changing the research questions, adding new hypotheses, increasing the time scale, dropping one model entirely, and refocusing on sensitivity analyses is in fact a request for a completely new study. We hope the reviewer understands our reluctance, as we could not even maintain

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Interactive Discussion

Discussion Paper

the title ("Two perspectives on ...coupled exchange...") in that scenario.

Rather, we believe we can address the concerns within the current paper structure. This is achieved by improving the linkage of the research questions (see below), and by clearly answering them in the conclusion part (p. 5301, l. 14–27 and p. 5302, l. 1–8), rather than in the discussion section.

Reformulated research questions:

1. *What are the essential processes at the surface and upper atmosphere governing the coupled carbon, water and energy budgets in the diurnal crop-atmosphere system?*

2. *Which modeling perspective can best reproduce these essential processes, and what does it teach us about the level of complexity needed in a diurnal land-surface scheme?*

Connected to our reformulated research question number 2, we have stressed more explicitly the value of comparing two models, stressing that one is generic, and the other one is specialized (see below). This also reduces the claim that plant physiology is an integral part of the study as noted by the reviewer.

p. 5279, l. 3 – 18:

In order to investigate the differences between the generic and specialized representation of crop biology, we use (...).

p. 5283, l. 5–7:

The Genotype-by-Environment interactions on CROp growth Simulator (GECROS) is a land-surface model specialized in crop carbon-storage (i.e. a crop yield forecast model). We use version 1.0, which was released by Yin and van Laar (2005).

p. 5285, l. 20–21:

BGD

11, C3162–C3177, 2014

The A-g_s model is a generic meteorological-oriented land-surface model. It is a single big-leaf model that relates plant CO_2 assimilation to the stomatal conductance ($g_s = 1/r_s$) via a CO_2 gradient (see Eq. 3).

p. 5301, l. 26–p. 5302, l. 8:

As a result, we recommend using meteorological-oriented land-surface models (...). However, to simulate longer periods of crop-atmosphere interactions, we recommend to adopt a merging strategy to use the distinct advantages of both the generic meteorological-oriented land-surface models (sound surface energy balance) and the specialized crop carbon storage-oriented models (crop phenology, nitrogen stress implementation and prognostic carbon pools).

In addition, although we agree that analyzing more days would be interesting, the number of "golden days" (i.e. mixed-layer days) suitable for analysis in our full dataset is only 3. Two of these are under very similar circumstances, yielding little additional value, and the third one occurs on a day with substantial instrumental difficulties. We regret not being able to comply with this request.

Finally, we would like to thank Reviewer 2 for his/her suggestion to extend the sensitivity analysis. However, this analysis would answer a different research question than the ones we are addressing (see above), as it would tackle the quantification of interactions in the system. We strongly believe that the core added value of our study is to identify and map essential processes, and to see how they can be represented by two modeling perspectives, when compared to a set of very comprehensive observations. We do not think there is room for another research aim with a full sensitivity analysis, but we are now considering this idea for future research.

Specific comments:

BGD

11, C3162–C3177, 2014

C1: p. 5289: I am not sure this creation of random weather is satisfactory, as there are expected to be significant changes in mean climate across a month – mixing up a month therefore is unlikely to create realistic climate particularly in spring, when days are lengthening rapidly. It would be better to use a range of historical climate drivers to explore this sensitivity. This component of the paper could anyway be dropped if the focus is shifted entirely towards surface-atmosphere interactions.

Responding to the concerns of both Reviewer 1 and 2, we decided to remove the seasonal sensitivity analysis of the uncoupled GECROS model (methods section p. 5288, l. 27–p. 5289, l. 12, results section p. 5291, l. 8–24, and Fig. 3), as it is not contributing to answering the two reformulated research questions. Also, in order to better keep the focus of the paper on the diurnal scale and the two-way couplings, we decided to move section 3.1 to section 2.2.3, renamed "Modifications to GECROS used in this paper and validation". This section will thus contain the former section 2.2.3, plus former section 3.1, except from its last paragraph (p. 5291, l. 8–24) and Fig. 3, which will be removed from the paper. The only Results sections appearing in the article will then be "3. Results", "3.1 Intercomparison of coupled models against observations", "3.2 Sensitivity analysis of an upper atmosphere forcing". In this way, we believe section 2.2.3 will prepare the reader to accept GECROS in the comparison against A-gs, after our modifications to the code have been introduced.

C2: p. 5290: Is there any testing of the drought effects represented in the model? It would strengthen the analysis to see that the model can recreate observed changes in processes during periods of low soil moisture.

We agree with Reviewer 2 that making sure GECROS can reproduce the carbon exchange in drought situations is an important prerequisite for using it in a broad set of environmental conditions. Unfortunately, to our knowledge, the testing of the GECROS model under dry conditions has been

very limited, and focussed only on wheat (Biernath et al., 2011; Buck et al., 2007). However, this shortcoming does not impact the quality of our study because both our uncoupled and coupled simulations with GECROS were made in a non-drought situation.

C3: p. 5291, l. 1: How are these large errors correlated with other drivers? Is there information in this mismatch that could explain crop-atmosphere interactions at this point?

As Reviewer 2 points out, if we make errors on key drivers of the surface exchange with GECROS, these are likely to propagate in a full coupling mode with MXL-GECROS. However, note that we have tried to free ourselves from important model errors by constraining long-term drivers (e.g. SWin, LAI, SMI) with observations on the 4th of August 2007.

In addition, we are happy to report that, after making our modifications to the GECROS code, we have briefly investigated correlations between model mismatches and a few of their drivers. Fig. 1 of this response shows that our coarse longwave radiation scheme is mainly responsible for model errors of Qnet. These model errors partly propagate in the LE and NEE fluxes. However, they do not propagate in the SH flux, which suggests that there is a more significant source of errors for SH, still undefined as of today. In addition to Qnet, we hypothesized that errors in NEE could be partly due to errors on the leaf temperature, but this does not appear when we try to correlate SH with NEE (SH is used to calculate leaf temperature in GECROS). Finally, it is clear in Fig. 1 of our manuscript that errors on LAI have caused the large mismatch of NEE after DOY 240. We added this last piece of information in the caption of Table 1 in our manuscript:

p. 5312:

Seasonal statistics of the daily integrated Qnet, LE, SH and NEE from Fig. 2.

Statistics are computed from sowing to maturity dates. We present the observed and modelled means and standard deviations, the root mean squarred error (RMSE) between the model and the observations (in the same units as the mean) and the R^2 between the model and the observations. Note that the large error on NEE is partly due to the inability of the model to reproduce the LAI after DOY 240 (see Fig. 1).

C4: p. 5291, l. 8: This testing would be better with realistic weather from historical data, see above.

We decided to remove this part from the study, see comment C1.

C5: p. 5292, l. 3: It would be better to start this paragraph with a topic sentence – what is the key message here? The ms would read better with a focus on topic sentences throughout.

We rewrote two paragraph openings as follows:

Section 3.2.1 (p. 5292, l. 3–23):

Overall, Fig. 4 shows that both MXL-GECROS and MXL-A-g_s calculate reasonable magnitudes and temporal evolutions of the surface fluxes for the observed maize crop, but MXL-A-g_s performs slightly better than MXL-GECROS.

Section 3.2.2 (p. 5293, l. 24–p. 5294, l. 10):

Figure 5 shows MXL-A-g_s outperforms MXL-GECROS, when simulating a fully coupled atmosphere.

C6: p. 5292, l. 19: So it seems the A-gs model has been fitted to better match the data.

We inferred the initial soil moisture index (55.5%) from the oberved Bowen ratio because soil moisture was a very uncertain parameter and it has a direct control on the energy fluxes. We used A-gs to do this because the SMI derived with GECROS (4.4%) was unrealistic for that date and location.

BGD

11, C3162–C3177, 2014

Interactive Comment

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C7: p. 5293: What are the implications for model evaluation of ignoring Phase A?

It is important to note that our response to this comment takes into account changes made to the manuscript after responding to Reviewer 1. For his comment C4, we checked our code and found a minor mistake in the energy gap correction of our observations. We have thus corrected information related to the observations in Fig. 4, and Table 2, and we have updated section 3.1.1 to integrate those new numbers. In the end, this does not change our conclusion that MXL-A-gs performs better than GECROS for the surface fluxes if we ignore Phase A. Because Phase A is a transition period during which the atmosphere is not yet strongly convective, the models have difficulties reproducing it. Also the correction method for SH and LE does not give realistic results with a negative SH flux. Therefore, we have excluded Phase A for the model evaluation in the revised manuscript. In order to prepare the reader for the evaluation of the surface fluxes, we have placed our description of the three phases of the day (A, B, C) before concluding on the models performance.

C8: p. 5296: But it seems A-gs performs best because it was tuned to do so. Would it not be possible to tune the other model to perform as well? i.e. is there any intrinsic reason why A-gs is better? Can that be proven? Why not use a single model?

It is possible to do the adjustment of soil moisture with the MXL-GECROS model. But as mentioned in comment C6, the same adjustment of the modeled Bowen ratio with the initial soil moisture done by MXL-GECROS yields a SMI of 4.4%, which is unrealistic for our location and date. This is caused by the differing water-stress responses used by the two models. In order to explain our choice of initialization procedure better for SMI, we implemented the following changes in the manuscript:

p. 5287, l. 17 – p. 5288, l. 8:

BGD

11, C3162–C3177, 2014

The dataset from Jans et al. (2010) provides the soil volumetric water content on the 4th of August 2007, but no precise estimate for the soil wilting point and field capacity at the maize site. Thus, we define rough estimates of the wilting and field capacity for our soil type, a loamy fine sand. Then, we choose to use one model to adjust the initial soil moisture within the boundaries of these estimates for the wilting point and field capacity, in order to obtain the observed Bowen ratio (ratio of sensible to latent heat flux). This regulation of the Bowen ratio with soil moisture is caused by the occurence of water-stress in the model. We choose the meteorological-oriented model, MXL-A-g_s, over the carbon storage-oriented model, MXL-GECROS, because the first downregulates photosynthesis linearly between wilting point and field capacity, whereas the latter experiences no water stress above a soil moisture index of 11 % (SMI gives the relative position of the actual soil moisture in between the wilting point and field capacity, see Eq. 6).

$$SMI = \frac{W_{actual} - W_{wilting\ point}}{W_{field\ capacity} - W_{wilting\ point}} \quad with\ W\ the\ soil\ volumetric\ water\ content \tag{6}$$

After the adjustment of soil moisture with MXL-A-g_s, we obtain a SMI of 55.5 %. We regard this SMI of 55.5 % as a reasonable estimate, considering the observed soil moisture on the 4th of August 2007 and the range of variations of soil moisture over the year. Note that the same adjustment done with MXL-GECROS would have yielded an SMI of 4.4 %, which is clearly unrealistic. We apply the same wilting point, field capacity, and absolute soil moisture for both MXL-A-g_s and MXL-GECROS (see Appendix Tables A2&A3). Thus, both models operate with the same soil type and SMI, but they will yield different Bowen ratios and surface energy balances because of their difference in water-stress implementation.

C9: p. 5297: This sensitivity section is the most interesting and novel component of the paper, and would benefit from more detail.

We agree with Reviewer 2 that an extensive sensitivity analysis would be an

interesting subject of research, in order to quantify the interactions occurring in the crop-atmosphere system. However, in our paper, the sensitivity analysis has a different purpose: it is aimed at demonstrating examples of essential interactions of the crop-atmosphere system (related to research question 1), and not to quantify all effects of the system. In this paper, we want to give the priority to this "mapping" of the processes, and to the comprehensive comparison of the two modeling approaches with the observations. We appreciate very much the encouragement of the reviewer, and we are now considering to do a complete analysis of the non-linear effects associated to the coupling between the vegetation and atmosphere.**

C10: p. 5297, l. 17: WUE should relate to GPP (photosynthesis), not NEE. WUE is expected to rise as water becomes limiting, as plants become more conservative (i.e. opposite response to what is observed here) – please comment.

We agree with Reviewer 2 that NPP and GPP are most often used to calculate WUE, however, NEE can also be used (e.g. in Tallec et al., 2013). It is important to understand that each definition gives a different point of view on the surface and that, depending on the definition, WUE can remain constant, increase or decrease in a drought situation (Reichstein et al., 2007; Marshall et al., 2007; Beer et al., 2009). To address the concern from Reviewer 2, we calculated the intrinsic water-use efficiency (iWUE = NPP/gs) of our maize crop, which is a real measure of the plant water conservatism. As expected by the reviewer, when our soil dries, iWUE increases. As a consequence, and to prevent further misunderstandings, we have decided to replace our definition of WUE by the iWUE in our manuscript (see also Fig. 2 in this response):

p. 5297, l. 14–20:

Thus, as a result of two very different feedback mechanisms on net photosynthesis, but also on evapotranspiration (see previous paragraph), we obtain an in-

BGD

11, C3162–C3177, 2014

Interactive Comment

crease in intrinsic water use efficiency (iWUE = NPP/g_s) of respectively 11 and 18 μmol CO_2 mol H_2O^{-1} for the high subsidence and soil moisture depletion cases, compared to the control case (i.e. +3% and +6% on average, see Fig. 8b). This means both forcings make plant carbon exchange, and by extension plant carbon storage, slightly more water-efficient.

C11: p. 5297, l. 21: Please express NPP and GPP as gC m-2 d-1 (not g CO2).

We agree with Reviewer 2 and implemented the suggested change for the units of NPP and GPP.

C12: p. 5297, l. 24: Please use another word than "aggravate".

We agree with Reviewer 2 and opted for the term "worsen".

C13: p. 5297, l. 25: Extrapolation from a single day seems unwise. It would be better to restrict discussion to the modelled period. Comparison against multiple days of data would strengthen the paper considerably.

We did not intend to extrapolate the results of our one day to a full season. On a single day, subsidence does aggravate soil moisture depletion. Thus we rather meant to say that if the drought situation is prolonged on a few days, subsidence would contribute to strengthening the soil moisture depletion, hence a short-term crop yield loss. We rewrote the concerned paragraph as follows:

[In] the previous paragraph we showed that the increased subsidence associated with high pressure systems forces the surface to evapotranspire more (5% increase in EF). High subsidence thus worsens soil moisture depletion (-1 % SMI), which ultimately will contribute to a yield decrease if the drought situation is prolonged.

C14: p. 5299: The role of correct soil moisture values for model validity is well made. I wonder if the experiments could be reformulated to show the sensitivity of BL dynamics to variation in initial soil moisture values? That might make this point more generally valid.

In our study, we already design a sensitivity experiment where the initial SMI is decreased by 5%, which could happen in a few days of soil drying (p. 5289, l. 23–24). In the results, we show that the 5% decrease in soil moisture shifts the evaporative fraction by 5% (p. 5297, l.3–4), which leads to the stimulation of the ABL height (h) shown in Fig. 7 of our manuscript. We clarified this point by implementing the following changes:

p. 5297, l. 1–4:

On the other hand, for the lower soil moisture case (...) [the] decrease in surface conductance leads to a reduction of EF of 5 % throughout the day (see Fig. 8a), and finally to a reduction of h of 40 m (see Fig. 7a).

C15: p. 5299, l. 26: The comparison of A-gs and GECROS models seems disconnected from the atmospheric coupling investigation.

We agree with Reviewer 2 that the connection was not clear from the context. This text in fact discusses part of the answer to research question 2, in evaluating the two models advantages and inconvenients. To make this more clear, we implemented the following changes:

p. 5299, l. 1–4:

We note that the satisfactory performance of coupled models depends on the correct initialization of the model (...). In our study (...). For the prospect of going from a diurnal to a seasonal scale study, we regard data assimilation of soil moisture, as done e.g. in Boussetta et al. (2013), Hong et al. (2009), and de Wit and Van Diepen (2007), as a promising solution.

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11, C3162–C3177, 2014

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

BGD

11, C3162–C3177, 2014

Interactive Comment

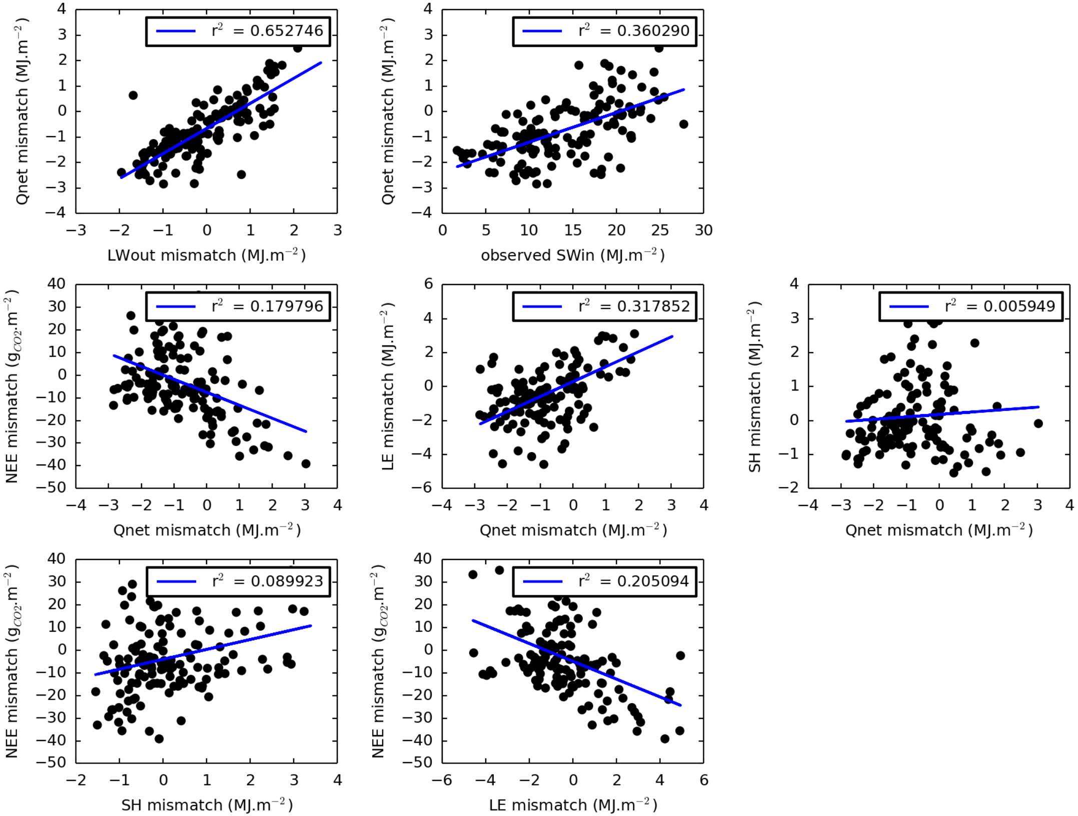

Fig. 1. Looking for correlations between the model mismatches of Qnet and LE, SH, NEE. Model mismatches have been calculated by substracting the observed values to the modeled values.

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Discussion Paper

BGD

11, C3162–C3177, 2014

Interactive Comment

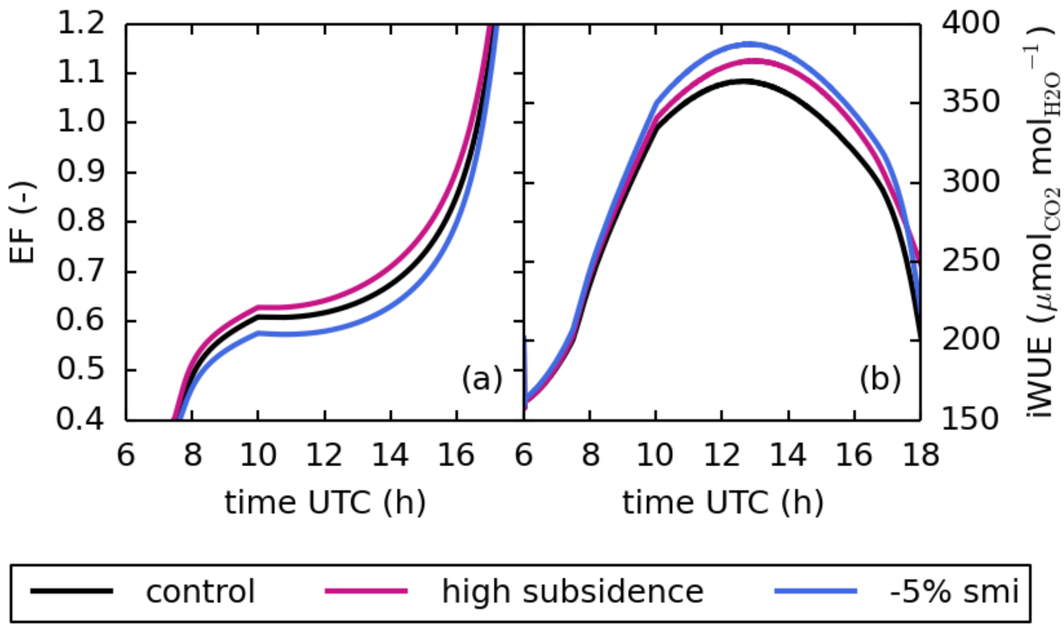

Fig. 2. Updated Figure 8 for the manuscript, where we replaced WUEeco by iWUE.

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