Answer to Reviewer 1 of the manuscript (*bg-2014-123):* TWO PERSPECTIVES ON THE COUPLED CARBON, WATER, AND ENERGY EXCHANGE IN THE PLANETARY BOUNDARY LAYER.

We would like to thank Reviewer 1 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:

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.

Before responding to this overall comment of Reviewer 1, we would like to briefly summarize our efforts in addressing all of the reviewers' concerns:

- **we have removed extraneous material (methods section p. 5288, l. 27–p. 5289, l. 12, results section p. 5291, l. 8–24, and Fig. 3) from the manuscript;**
- **we have reformulated our research questions to address concerns from Reviewer 2;**
- **we have applied changes to the structure of our manuscript;**
- **we have performed additional numerical experiments to check the influence of the coupling time step on model output;**
- **we have conducted additional statistical and graphical analyses to address a few specific comments;**
- **we have made modifications to the text, where specific comments demanded clarifications.**

All of these items are hereafter presented and discussed.

More specifically, we would like to thank Reviewer 1 for his/her qualification that our results and discussion are physically sound, and regret that we did not communicate the many novel aspects of our work more clearly. We take this chance to reiterate them. First, our modeling study combines the three major cycles (carbon, water and energy), which not only interact at the surface but also are influenced by the boundary layer dynamics, in which entrainment and subsidence play an important role. This allows our simulated variables to react to surface conditions (soil moisture, crop LAI, vegetation cover), but also to non-local environmental conditions (free-troposphere lapse rates, subsidence, advection). In addition, we benefit from a very comprehensive set of observations of the soil, plant, 2m-atmosphere and boundary-layer height, which clearly helps us to reproduce and understand the reality of a coupled crop-atmosphere system. Finally, the two modeling perspectives being compared side-by-side indeed adds to these novel aspects, and we appreciate this recognition by Reviewer 1. It strengthens our conviction to keep both models in our study, despite the suggestion from Reviewer 2 to remove this novel aspect.

Moreover, we do not believe that our controlled setup reduces the quality of our intercomparison. The design of our modeling framework is meant to facilitate the daily study of the land-atmosphere interactions, rather than to be widely applicable. This is why we used a model for the atmospheric mixed-layer and not a numerical weather prediction model. All initial conditions are prescribed directly from observations, or indirectly deduced from them (e.g. we adjust soil moisture to match the observed Bowen ratio, and we adjust the free-troposphere temperature lapse rate to match the observed ABL growth curve). Using a very comprehensive set of observations, our controlled setup ensures us to have the best, most realistic start of run possible.

Finally, we feel that part of section 3.1 is needed to prepare the reader to accept GECROS

in the comparison against A-gs, after our modifications to the model have been applied. The reviewer's suggestion to remove section 3.1 has shown us how the flow of the paper could be improved here. We decided that, in order to better keep the focus of the paper on the diurnal scale and the two-way couplings, we will move the validation of the uncoupled GECROS to section 2.2.3, renamed "Modifications to GECROS used in this paper and validation". This section will hold the content of 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", and "3.2 Sensitivity analysis of an upper atmosphere forcing".

Specific comments:

C1: p. 5278, l. 20–25: 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.

We would like to thank Reviewer 1 for pointing out this correct contrast. We followed his/her advice to introduce it earlier in the article and implemented the following changes to the abstract:

p. 5276, l. 23 to p. 5277, l. 3:

> *We illustrate this with a sensitivity analysis where high subsidence and soil moisture depletion, typical for periods of drought, have competing and opposite effects on the boundary-layer height h. The resulting net decrease in h induces a change of 12 ppm in atmospheric CO_2 mole fractions. Also, the effect of such high subsidence and soil moisture depletion on the surface Bowen ratio are of the same magnitude. Thus, correctly including such two-way land-surface interactions on the diurnal scale can potentially improve our understanding and interpretation of observed variations in atmospheric CO_2, as well as improve crop yield forecasts by better describing the water loss and carbon gain.*

C2: p. 5281, l. 15: Does the wind profiler provide a better estimate of PBL height than the Cabauw radiosondes? Under daytime/convective conditions as well?

The accuracy provided with the measurements of the ABL height h at the Cabauw tower on the 4th of August 2007 (Fig. 5d) is the same for the wind profiler and the radiosondes. This accuracy was estimated from an intercomparison of the different ABL height measurements on that date (radiosounding in Cabauw, wind profiler in Cabauw, and radiosoundings from station De Bilt). It is therefore not per se an instrumental accuracy. Knowing that the accuracy on h given at the Cabauw tower was the same for both types of measurements, we chose to use the wind profiler records, as it provides the continuous diurnal evolution of h, with a frequency of 30 minutes, whereas radiosoundings are performed only twice a day at station De Bilt. However and to answer the question from Reviewer 1, the accuracy on h is much higher during daytime/convective days for both methods.

C3: p 5281, l. 23: 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%?)

In our study, the observed instantaneous energy gap averaged between 6 and 18 UTC is of 19%. Summarized findings of several campaigns presented in Foken (2008) show that eddy covariance measurements above crops in temperate regions have an energy gap ranging from 14% to 33% of the net radiation at the surface. Foken et al. (2010) states that eddy-covariance measurements performed above 9 agricultural fields and 5 other sites at the end of spring in Germany presented an energy gap of 20-30% on average. Finally, more specifically for maize, in Meyers and Hollinger (2004), the total heat storage was measured with the eddy covariance technique during daytime, for a full grown maize crop of Midwestern USA. It was estimated to be about 14% of the net radiation on average, although total closure of the energy balance was not achieved in their case (6% gap remaining). Our observed energy gap is thus comparable to the ones in other studies.

Table 1: Comparison of the Evaporative Fraction (EF) with the Bowen ratio (β)

Time period	EF = LE / (SH+LE)		β = SH/LE	
	mean	stddev	mean	stddev
6 – 18 UTC	0.82	0.19	0.28	0.24
8 – 16 UTC	0.71	0.05	0.40	0.09

In order to answer the question of Reviewer 1 directly in the article, we added the exact gap percentage of our observations, the extra reference from Foken et al. (2010), and inverted the order of the two following paragraphs:

p. 5281 l. 20 – p.5282, l. 10:

Because we want to focus on (...) we specifically pick (...) the 4$^{\text{th}}$ of August 2007 (...) peak of its growth (see LAI in Fig. 1).

On the 4 August 2007, the continuous measurements show a daytime energy gap of 19% between the net absorbed radiation and the sum of the surface (latent, sensible, ground) heat fluxes. This energy gap is typical for a crop like maize, mainly due to heat storage. In a minor extend, the gap can also be generated by photosynthesis, which can proceed at unusually large rates for maize, large-scale heat transport processes, and measurement accuracy (Meyers and Hollinger, 2004; Foken et al., 2010; Foken, 2008).

C4: p. 5281, l. 28: 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?

Inspired by the referee's comment about using the evaporative fraction or Bowen ratio to do the correction, we checked our code and found a minor mistake in the correction of our observations. We have thus corrected Fig. 4, Table 2 and section 3.1.1 in the revised manuscript. This does not change our conclusion that MXL-A-gs performs better than GECROS.

To answer the reviewer's comment, we agree that the Evaporative Fraction (EF) is a more stable variable than the Bowen ratio (β) during daytime, and during the midday period (i.e. the convective phase, from 8 to 16 UTC in the observations). We have placed evidence of this in Table 1 on page 3 of this response. However, this does not play a role in the correction of the fluxes because EF and β can alternatively be used to correct surface fluxes, using EF = 1 / (1 + β). The basis of the Bowen ratio correction method is to allocate the energy gap (or residual) into SH and LE while conserving the observed Bowen ratio (and also EF). This can be achieved by using the following formulas:

$$\begin{aligned} \mathrm{SH_{corrected}} &= \mathrm{SH_{observed}} + \mathrm{Residual} \times \frac{\beta}{1+\beta} \\ \mathrm{LE_{corrected}} &= \mathrm{LE_{observed}} + \mathrm{Residual} \times \frac{1}{1+\beta} \end{aligned} \quad (1)$$

In order to make the correction method more clear to the reader, we added these correction formulas and their consequence (unaltered Bowen ratio) in our manuscript. This Bowen ratio correction method has been widely used and published, thus we also added some extra references to justify the choice for this well-known method:

p. 5282, l. 1–2:

It has been previously used by Barbaro et al. (2014); Foken (2008); Twine et al. (2000); Beljaars and Bosveld (1997).

C5: p. 5283, l. 18: How is if the water supply is sufficient determined?

The amount of available soil moisture in the soil (actual soil moisture content - wilting point) needs to be higher than the amount of water required for potential evapotranspiration. We modified the manuscript to explain this better:

p. 5283, l. 17–21:

> *The actual photosynthesis and transpiration are obtained by evaluating the soil water content: if the available soil moisture is higher than the amount of water needed for potential transpiration, GECROS works at full potential level. Otherwise, GECROS transpires solely the available water supply, and reduces its photosynthesis and stomatal conductance accordingly.*

C6: p. 5284, l. 1–5: This is a single day study though, no? Could a lot of this be easily prescribed for this day (based on observations)?

We had difficulties finding the context for this remark from the indicated lines. If Reviewer 1 is asking us if we could just initialize GECROS at a date X (in the middle of the growing season) using observations, then we our answer is that it is impossible. This is because GECROS has to be started at emergence date, in order to calculate complex internal variables like leaf nitrogen content, LAI, plant organs carbon content, etc, which we cannot infer from our observations.

C7: p. 5285, l. 1–5: 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.

In our observations, if we calculate the ratio of instantaneous fluxes of ground heat flux (G) and net available radiation at the surface (Rn) between 6 and 18 UTC:

- **On the 4th of August 2007, G/Rn is on average 6%.**
- **On the 6th of August 2007, another "golden day" with similar conditions of LAI, vegetation cover and soil moisture, G/Rn is on average again 6%.**
- **On the 20th of August 2007, another golden day but this time with lower LAI and vegetation cover, and a moister soil, G/Rn is on average 18% of Qnet.**

If we calculate the ratio of the integrated fluxes of G and Rn during daytime, along the entire maize growing season, the $\Sigma G/\Sigma Rn$ ratio is on average 7%. Note that all ratios calculated here do not consider a surface correction for G, due to the fact that the ground heat flux plates were placed below the surface. G might be in reality very slightly higher than measured.

Knowing that, our GECROS parameterization of G being 10% of Rn seems to slightly overestimate the observed flux. This difference does not have a significant impact on our results.

C8: p. 5286, l. 1–5: 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)?

See Fig. 1 on page 5. In MXL-GECROS, the 10% estimate indeed overestimates G. For MXL-A-gs, G is well calculated.

C9: p. 5287, l. 5–10: 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?

In our study, the difference of coupling time step has a negligible impact on the evolution of temperature, humidity and CO_2 in the mixed-layer. To demonstrate this, we plotted two runs of MXL-A-gs using identical initial conditions but with differing coupling time steps in Fig. 2 on page 10 of this response. We implemented the following changes in the manuscript to inform the reader about this:

p. 5287, l. 6–10:

> *In addition, A-g_s and GECROS do their own internal calculations (...) respectively 1 and 5 minutes. Note that we have checked and validated that the 4 minutes difference in surface fluxes time step does not affect the coupling, because it stays almost instantaneous. All calculations start at 6 UTC, after sunrise when turbulent convection is already active, and last until 18 UTC, thus ensuring the atmosphere is well-mixed during that time.*

C10: p. 5287, l. 10–15: 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

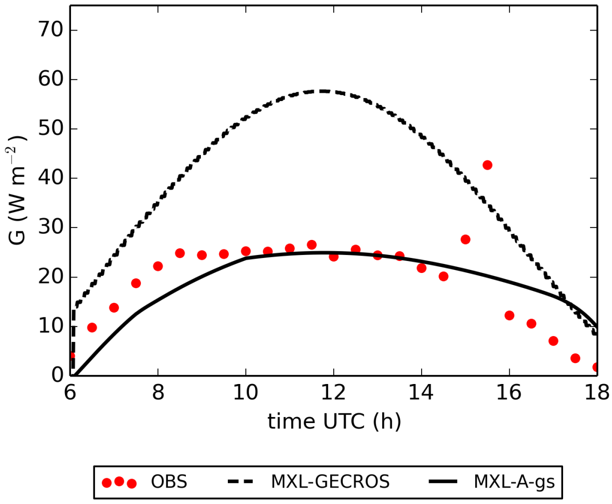

Figure 1: Comparison of the modeled and observed ground heat fluxes (G).

(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.

The requested identical spin-up for both models is not possible, because GECROS can not be run for periods shorter than a growing season, while our version of A-gs is inherently limited to one diurnal cycle. This means that GECROS depends on its own initial conditions on our studied day (except for soil moisture, which we impose), and A-gs depends fully on our specification of these conditions.

Indeed, like for soil moisture, we think it would be important to constrain soil temperature identically between the models if we were resolving the soil energy budget. However, we parameterize it as being 10% of the net available energy at the surface. Thus soil temperature only impacts the amount of soil respiration, which we have set to be identical between the two models (see p. 5288, l. 9–17). Therefore, both models behave as if the same initial temperature profile is applied.

C11: p. 5287, l. 15–20: What is the soil type at this location? You can get good estimates from literature/ tables on WP and FC based on soil type.

The soil type at that location is "loamy fine sand". We have set up rough estimates of wilting point, field capacity and saturation points following classifications like http://www.terragis.bees.unsw.edu.au/terraGIS_soil/sp_water-soil_moisture_classification.html. In order to clarify this point, we implemented the following changes in the manuscript:

p. 5287, l. 17–22:

> *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 the meteorological-oriented model, MXL-A-g_s, 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).*

C12: p. 5287, l. 20–25: Is this relationship actually linear? Just so I understand, basically tuning the FC and WP values in the model to match the obs?

Yes, the water-stress function of A-gs is linear (like the JULES model in Powell et al., 2013). However, please note that we set the wilting point and field capacity following a soil classification, and that only the soil moisture content is adjusted following the observations. We believe the previous modifications we made to the manuscript clarify that point.

C13: p. 5288, l. 1–10: Ok good - so you are scaling the soil moisture such that it is consistent across models.

Yes, in this regard, the settings of the boundary and initial conditions are consistent between the models.

C14: p. 5289, l. 1-15: This uncoupled GECROS work seems like an aside in this work. It isnt 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.

We agree with Reviewer 1 and have removed the seasonal sensitivity analysis of GECROS from our manuscript (removal of the methods on p. 5288 l. 27– p. 5289 l. 12, of the results on p. 5291 l. 8–25, and of Fig. 3).

C15: p. 5291, l. 5-7: 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 observations. Arent the observations used on the diurnal scale as well (so the model is being forced at the diurnal scale)?

The uncoupled GECROS model is not forced with diurnally evolving atmospheric variables. In fact, it is forced by one weather data input per day, from which a diurnal cycle is recreated for temperature and radiation only, assuming a Gaussian curve. However, we agree with Reviewer 1 that our hypothesis about the source of the GECROS model RMSE is incomplete. We thus implemented the following changes in our statement:

p. 5291, l. 3–7:

> *Note that the mismatch [...] can [...] be quite large [...]. Such mismatch could be produced by the incorrect simulation of key driver variables (e.g. Qnet and soil moisture) in GECROS, or by the absence of a diurnal-scale weather forcing (only one data input is given per day), or even by the lack of atmospheric feedback. This partly reinforces the aim of our study, which is to focus on understanding the diurnal two-way crop-atmosphere interactions.*

C16: p. 5291, l. 15: 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.

We have addressed this concern of Reviewer 1 in the "Overall" section.

C17: p. 5292, l. 16: What exactly is satisfactorily?

Following the changes made to the correction of our observations (see comment C4), we updated the numbers mentioned in this paragraph and included the exact percentage of change to make our statements more precise.

C18: p. 5292, l. 20–22: What if you did the reverse and prescribed with GECROS soil moisture? Might be worth an additional simulation or two.

In order to match the observed Bowen ratio using the MXL-GECROS model, soil moisture needs to be really close to wilting point (we match the Bowen ratio at SMI 4.4%). This is clearly unrealistic because it suggest a very dry soil. Using this value in MXL-A-gs would give us a very extreme drought situation. Such situation was not observed for our date and location. Using the modifications made for comment C11, we clarified our choice as follows:

p. 5287, l. 17 – p. 5288, l. 8:

> *The dataset from Jans et al. (2010) provides the soil volumetric water content on the 4^{th} 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 later 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. 2).

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

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* 4^{th} *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.*

C19: p. 5293, l. 6–10: Bowen ratio or Evaporative Fraction could be brought in here to ease the discussion on the fluxes.

We agree with Reviewer 1. We thus implemented the following changes:

p. 5293, l. 7–10:

During Phase B, MXL-GECROS is strongly underestimating the Bowen ratio, with an underestimated SH in accordance with its consistently higher LE flux. As a consequence and due to the coupling with evapotranspiration, photosynthesis is overestimated, as shown in NEE (considering that the soil respiration is low and identical between MXL-GECROS and MXL-A-g_s*).*

C20: p. 5294, l. 1: Why is this? Not sure I follow regarding the surface layer.

The surface layer is the layer of air located near the surface (first 10% of the ABL height in a mixed-layer situation). During daytime, the surface is very warm because it is absorbing shortwave radiation, and it also holds air saturated with water in the soil pores and plant stomatal sub-cavities. In the surface layer, the temperature and humidity adjust gradually from the surface (plant or ground) value to the lower mixed-layer value. In order to better explain the properties of the surface layer in the manuscript, we implemented the following changes:

p. 5293, l. 25 –p. 5294, l. 1:

Considering the general properties of the surface layer (a gradual decrease of temperature and humidity from the surface level to the mixed-layer level), the observed 2-meter atmosphere is thus expected to be slightly warmer and moister than the modelled mixed-layer atmosphere.

C21: p. 5295, l. 5: 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).

In the Methods section 2.3 "Simulation setup", we refer to the Appendix table where advected quantities are given. However, and to make to make this clearer for the reader, we will modify paragraphs as follows:

Section 2.3 (p. 5287, l. 13-16):

On 4 August, we initialize all our models following the available soil, crop and atmospheric observations from Jans et al. (2010). Note that we prescribe horizontal heat and moisture advection during the first hours of our numerical experiments. In addition, we use the C_4 *photosynthesis parameters published by Ronda et al. (2001) to initialize the A-*g_s *scheme.*

Section 3.1.2 (p. 5295, l. 5-6):

The occurence of heat and moisture advection on the 4^{th} *of August 2007 is noticeable because the observed diurnal range in temperature and the early morning increase in humidity are too large to be solely due to realistic crop sensible heat and evapotranspiration fluxes. We thus prescribe horizontal heat and moisture advection during the first hours of our numerical experiments (see Table A1).*

In addition, we have responded to the reviewer's comment about the controlled setup and applicability in the "Overall" section.

C22: p. 5295, l. 19–20: 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.

In the MXL model, the ABL height h is defined as the height of the minimum buoyancy flux generated by the entrainment of heat, which corresponds to the height of the maximum gradient in potential temperature.

In the MXL model, no turbulence calculations are performed. Instead, a well-mixed convective boundary layer, capped by an infinitesimally thin inversion layer, is assumed. The growth of the upper boundary of the model, i.e. the entrainment velocity, is calculated as the magnitude of the entrainment buoyancy flux divided by the virtual potential temperature inversion, minus the subsidence velocity. The entrainment buoyancy flux is parameterized as -0.2 times the surface buoyancy flux. To sum up, the ABL growth is controlled by the input of heat from the surface (SH), but can be countered by the occurrence of subsidence.

C23: p. 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.

We are pleased with the support for our Section 3.3, and have addressed the concerns about Section 3.2 in the "Overall" and C21 sections.

C24: p. 5299, l. 25–30: 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.

Since we are exploring the importance of the water-stress formulation in a follow-up paper (in preparation), we would be thankful if Reviewer 1 could give us the exact reference for these papers. We have found one author related to this topic, Randal D. Koster, but have been unable to find the suggested paper(s).

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

The entrainment velocity is parameterized in the MXL model. The entrainment flux of CO_2 is determined by the ABL growth rate (a function of the surface buoyancy flux and of subsidence, as explained in comment C22) and by the strength of the CO_2 inversion at top of the ABL (i.e. the CO_2 gradient between the ABL and the free troposphere). We implemented the following changes to the manuscript to explain this better:

Section 2.2.1 (p. 5282, l. 16 – p. 5283, l. 3):

> *Our atmospheric boundary-layer scheme is a box model, which describes accurately the development of the diurnal atmospheric boundary layer (ABL) when turbulence is strong (mixed-layer situation). (...) Entrainment fluxes are calculated. The MXL model has been widely tested and is a robust model for sunny days with few to no boundary-layer clouds, all conditions met by the 4 August 2007 over our maize field.*

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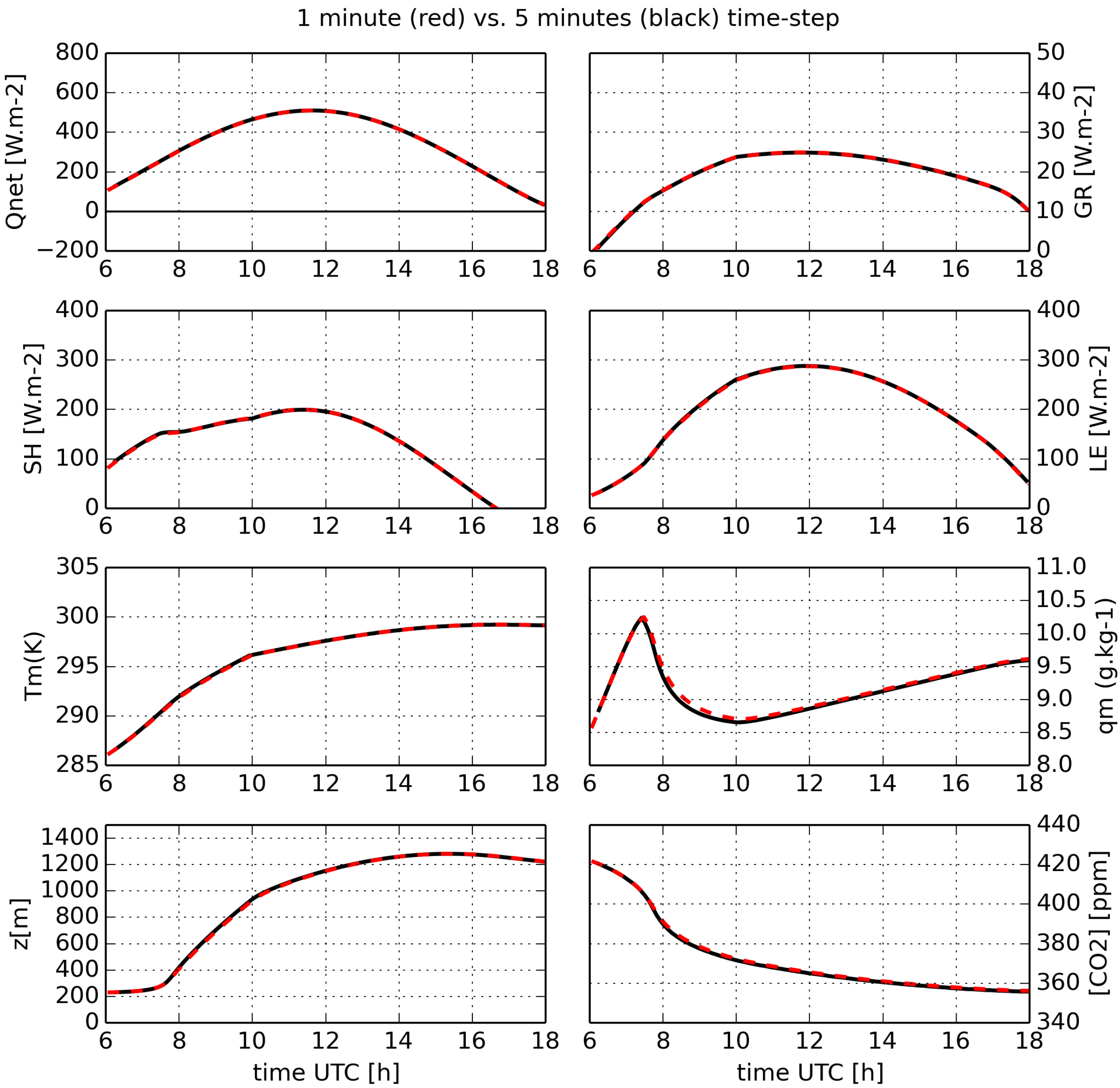

Figure 2: Comparison of MXL-A-gs runs with timesteps of 1 minute (red) vs. 5 minutes (black).

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 CO2 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 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:
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:

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.

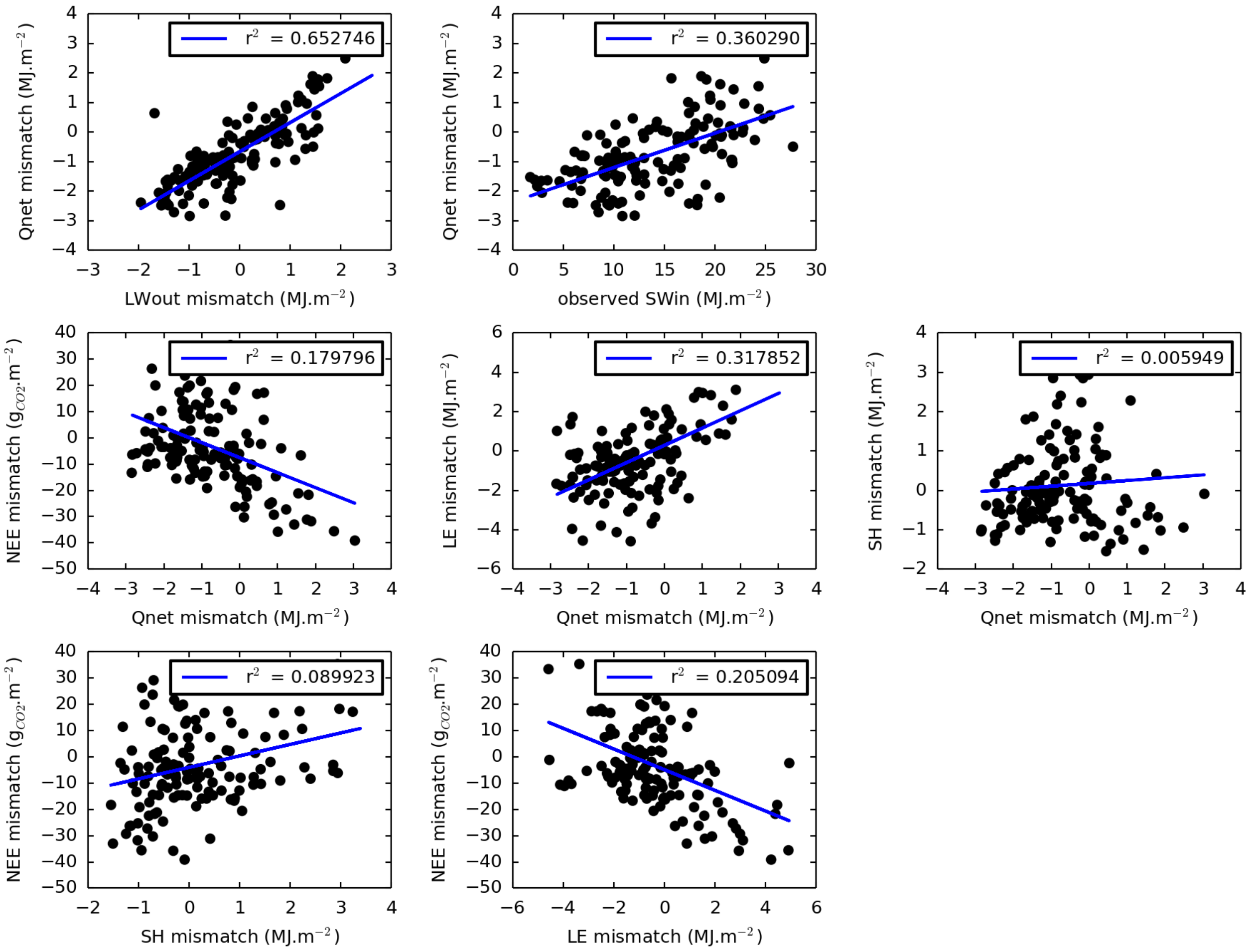

Figure 1: Looking for correlations between the model mismatches of Qnet and LE, SH, NEE. Model mismatches have been calculated by substracting the observed values to the modeled values.

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 on page 3 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.

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:

> *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).*

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

> *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 occuring 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 increase 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

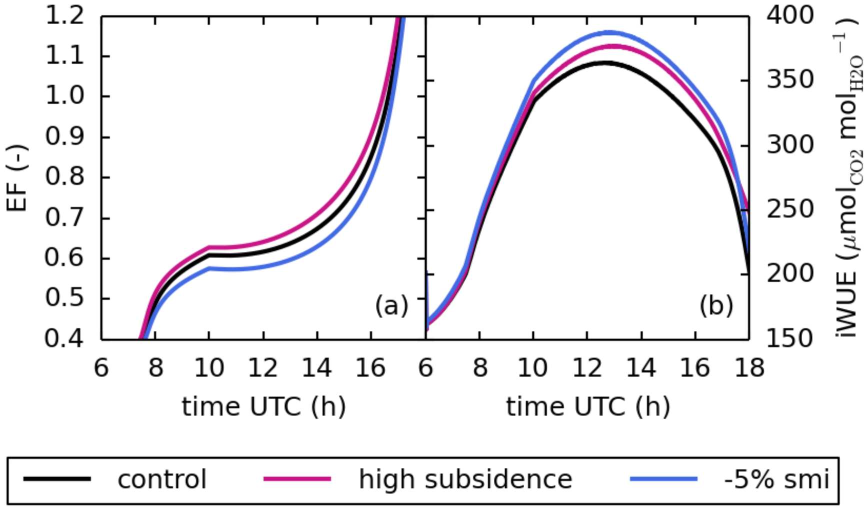

Figure 2: Updated Figure 8 for the manuscript, where we replaced WUEeco by iWUE.

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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