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

12 November 2014

General comments

This paper investigates the coupling between water, energy and carbon cycles using two different types of land surface models based meteorological surface-exchange (A-gs) and Carbon-storage vegetation (GECROS crop model) perspectives, both coupled to the same atmospheric mixed layer model in order to assess the contribution from different processes/forcings on the budgets of water, energy and carbon over a maize field.

The paper addresses important questions that are relevant to the wide community of land-surface and carbon cycle modelling. The first question regarding the contribution of upper and surface processes on the different budgets is addressed with sensitivity experiments at the surface and upper atmospheric boundaries by reducing soil moisture and increasing large-scale subsidence. The experiments show that both forcings can play an equally important role. They both change the latent heat fluxes (increasing/decreasing for enhanced subsidence/soil moisture depletion forcings respectively) and reduce the water use efficiency with the same magnitude vias different mechanisms. Despite atmospheric and surface forcings being equally important on the energy and water surface fluxes, for the carbon cycle the story is different. The reason for this being that NEE is only significantly affected by the change in soil moisture via changes in the stomatal conductance. This is because there is no stomatal response to the increasing vapour pressure deficit in the higher subsidence case. Is this a realistic assumption?

The results from the two sensitivity experiments show that the variation of atmospheric CO_2 is much larger for the increased subsidence than for the reduced soil moisture, despite subsidence having no impact significant impact on NEE; whereas soil moisture decrease does have a larger impact on NEE. This is because the changes in the subsidence and soil moisture lead to changes of a few percent in the CO_2 fluxes at the surface and mixed layer top (via NEE and entrainment); while the change of the mixed layer height is around 40% for increased subsidence and only a few percent for the decreased soil moisture. Is this large change in the mixed layer height realistic? If so, it emphasizes the importance of having an accurate upper atmospheric forcing when interpreting the variability of atmospheric CO_2. I think the higher sensitivity of the upper forcing on the change in the mixed layer height compared to the change in the surface fluxes should be emphasized as it is of key importance in order to understand/explain the variability of CO_2 in the well-mixed daytime planetary boundary layer.

Another interesing result from this study is that both NEE and entrainment fluxes play an equally important role in the evolution of the CO_2 in the mixed layer. These results confirm the challenging task that flux inversion systems face in order to be able to retrieve the surface fluxes of CO_2 from the observed atmospheric CO_2 in the planetary boundary layer.

The second question addressed is on the complexity of the models required to simulate interactions of a cropland with the atmosphere. Although the evaluation shows that the A-gs outperforms the crop model on a specific day, I think the comparison is not completely fair. The outperformance of A-gs depends on the tuning using atmospheric and land surface observations for a specific site and day. The comparison might lead to different results if the A-gs was adapted to run over longer time scales than one day without the observed forcings (e.g. within a climate model). This is already mentioned in the conclusions, but perhaps it is should be more clearly stated in the abstract.

Although the study concentrates on a single site and a single day, it uses an impressive comprehensive set of observations to assess all the relevant model parameters for the water, energy and carbon cycles. Moreover, such coupling and sensitivity studies require specific conditions, when boundary layer is well mixed and advection is not strong in order to minimize the intereference of non-local effect. The experiments and interpretation are both sound and the mechanisms that play a role in the experiments are well explained.

Other comments

- Fig 2: The importance of having a two way coupling compared to one way coupling is only shown with the GECROS model. It would have been interesting to also do the comparison with the A-gs on 4 August 2007.
- Section 2.3: How is CO_2 initialised in the boundary layer and free troposphere for the simulations on 4 August 2007?
- I find the term "diurnal" throughout the paper a bit confusing. This study is limited to the daytime well-mixed boundary layer. I think it would be clearer if "daytime" was used instead of or together with "diurnal".
- Section 3.3: Are the changes in atmospheric CO_2 of 12 ppm in the high subsidence case forcing changes in NEE via the CO_2 gradient term in equation 3? If so, the impact from the increased subsidence seems to show that there is a very small sensitivity.
- Figure 9: What do you mean by instantaneous change in boundary-layer height in the computation of CO_2 tendencies? Should it not be instantaneous value instead?