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Biogeosciences
Discussions

Interactive comment on "Simultaneous measurements of CO_2 and water exchanges over three agroecosystems in South-West France" *by* P. Stella et al.

P. Stella et al.

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We are grateful to the reviewer for his pertinent comments which, notably, drew our attention to several mistakes made in the first version of the paper. We hope that our corrections improve the quality of this work.

Replies to comments:

General Comments

Reviewer:

This manuscript provides interesting data and analyses, in the contexts of both the Kyoto objective to manage land-use so as to optimize CO2 sinks, as well as water use and cycling by forest and agricultural systems. The (challenging) technical methodology is sound overall, comparing concurrent eddy covariance measurements over two forests (young and mature) with those from a maize crop in the same climatic and soil region.

1. The limited duration of the study (not reaching a full year of observations) is a weak point, but does not wholly detract from the value of these data, which are particularly interesting in terms of the eco-physiological analyses presented by the authors. However, the neglect of carbon fluxes associated with harvest/exports when discussing the long-term balance of atmospheric CO2 by such agroecosystems substantially weakens the conclusions. With some improvement in this regard, and the correction of some specific technical concerns, I believe the paper should be acceptable for publication in Biogeosciences.

Answer:

1a. See answers to Reviewer 2 who also raised the question of the limited duration of the study. 1b. Concerning the comment about carbon exports, see our answers to specific comments.

Specific Comments

Reviewer:

2. In equations, all variables should be comprised of a single letter in normal font size, with appropriate subscripts as necessary. For example, in eq (1) the photosynthetic photon flux density could be denoted Fpp, and in equation (2) the nighttime ecosystem respiration Ren. Otherwise, it can be difficult to distinguish a two-symbol variable (such as Re in equation 1) from the product of two (such as Bs in equation 3).

Answer:

Corrected

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Reviewer:

3. Page 2496, line 24: "GPP was calculated half hourly". It is unclear how the variability in GPP relates to variability to PPFD (as in Figure 2), because the authors have not specified the time scale of data for fitting equation (1). To be more specific, the question is: how often were the fitted variables a1, a2, and Red were allowed to vary? This information is important when interpreting the results, as in Section 3.2.

Answer:

This section was incorrectly written and has been rephrased. In this paper, GPP is not modelled as a function of a1, a2 and Fpp, but calculated half hourly as the difference between experimental half hourly values of NEEd and the mean daytime ecosystem respiration. The latter is determined fitting equation (1) at daily scale.

Reviewer:

4. In equation (3), the VPD is specified as the "water vapour density saturation deficit (kg m-3)". This is not consistent with the Penman-Monteith equation, which specifies fluxes in terms of the vapor pressure deficit (Pa), consistent with the principles of diffusion. The difference between using pressure versus density can become extremely important in the presence of strong temperature gradients between the leaf and the leaf boundary layer.

Answer:

The reviewer refers to the paragraph reported hereafter:

"The stomatal conductance (gs) can be determined from the water vapour flux by inverting the Penman-Monteith equation (Monteith and Unsworth, 1990):

gs = (DCO2/DH2O) (E/VPD) / (1 + (E/VPD) (Ra + Rb) ((Bs/Y) - 1)) Eq. (3)

where DCO2 and DH2O (m2 s-1) are the molecular diffusivity for CO2 and water vapour, respectively (DCO2/DH2O = 0,62), E is the water vapour flux (kg m-2 s-1), VPD the water vapour density saturation deficit (kg m-3), B the Bowen ratio, s the slope of the saturation curve (Pa K-1), Y the psychrometric constant (Pa K-1) and Ra and Rb the aerodynamic and boundary layer resistances (s m-1), respectively. "

First of all, let us note that using the notation VPD for the water vapour density saturation deficit (kg m-3) was a mistake, since, in other parts of the text (notably Section 3.2, including Fig. 2), we use the same notation for the vapour pressure deficit (hPa). In the revised version, we use for the former the notation "D" (greek symbol : delta), as Lamaud et al. (2009). We have also rephrased the first sentence of this paragraph as : "The stomatal conductance for CO2 (gs) can be deduced from the stomatal conductance for water vapour, the latter being inferred from the evaporation flux measurements by inverting the Penman-Monteith equation", and we have changed the reference to Monteith (1981). Now, let us precise how equation (3) was obtained.

Monteith (1981) expresses (p7) the Penman-Monteith equation as:

LE = (s(Rn - G) + rhoCp(qs(Ta) - qa)/(Ra + Rb)) / (s + Y(1 + (Rs/(Ra + Rb))))

where L is the vaporisation heat for water (J kg-1), E is the water vapour flux (kg m-2 s-1), Rn is the net surface radiative flux (W m-2), G is the heat flux inside the ground (W m-2), Cp is the heat capacity of dry air (J K-1 kg-1), rho is the air density (kg m-3), qs(Ta) is the saturation mass fraction of water vapour at air temperature (kg kg-1), qa is the mass fraction of water vapour in air at measurement height (kg kg-1), s is the slope of the water vapour saturation vs temperature curve (K-1), Y = Cp/L is the psychrometric constant (K-1), Ra is the aerodynamic surface layer resistance (s m-1), Rb is the laminar sublayer resistance (s m-1) and Rs is the stomatal resistance (s m-1). Since Rn - G = H + LE (surface energy balance equation, where H is the sensible heat flux), the equation can be reorganised, introducing the Bowen ratio (B = H/LE), as:

LE = (s(B + 1)LE + rhoCp(qs(Ta) - qa)/(Ra + Rb)) / (s + Y(1 + (Rs/(Ra + Rb))))

Introducing the water vapour density saturation deficit "D" (kg m-3) = rho(qs(Ta) - qa),

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and dividing the left and right terms of the equation by L(with Y = Cp/L), it gives:

E = (s(B + 1)E + "D"Y/(Ra + Rb)) / (s + Y(1+(Rs/(Ra + Rb))))

that we can reorganize as: gsH20 = 1/Rs = (E/D) / (1 + (E/D) (Ra+Rb) ((Bs/Y) - 1))

with: gsCO2 = (DCO2/DH2O)gsH20

Therefore, the Penman-Monteith equation, as expressed by Monteith (1981) with qs(Ta) - qa (kg kg-1), directly leads to our equation (3).

N.B. We make the complete demonstration available for the reader in Word file.

References:

Lamaud, E., Loubet, B., Irvine, M., Stella, P., Personne, E., Cellier, P.: Partitioning of ozone deposition over a developed maize crop between stomatal and non-stomatal uptakes, using eddy-covariance flux measurements and modelling. Agr. Forest. Meteorol., 149, 1385-1396, 2009.

Monteith, J.L.: Evaporation and surface temperature. Quarterly Journal of the Royal Meteorological Society 107, 1-27, 1981.

Reviewer:

5. Page 2499, line 9: Figure 2 presents Rg in energetic units (Watts), rather than PPFD (usually given in quantum units), as in Figure 1 and throughout section 3.2. If the authors wish to establish PPFD as one of the "factors affecting GPP", they should either present this variable in Figure 2, or justify Rg as a substitute.

Answer:

The use of Rg in Figure 2 was indeed a mistake, since we referred to PPFD in the whole section 3.2. In the revised version, we have plotted PPFD (now denoted Fpp) in Figure 2.

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6. Page 2499, line 10: Rather than "recorded", I would suggest that GPP was "modeled".

Answer:

The term "modeled" is not valid for PPFD and VPD. We have rephrased the sentence as "Figure 2 presents time series of Fpp, VPD and GPP for each site."

Reviewer:

7. Page 2500, lines 3-6: The manuscript is particularly erratic concerning soil water content (SWC), and should be made coherent in this regard. In Section 3.2 (results), SWC is mentioned three times to justify observed differences in water stress. Likewise, the methods section (p2493) mentions numerous sensors to quantify SWC. However, the manuscript presents no results regarding the SWC data. Since the authors explain differences between the two forests in terms of VPD (but specifically not SWC), their arguments would be more convincing if the SWC data were also presented, however summarily. This need is highlighted by the arguments on page 2501 (lines 6-7), which de-emphasize the importance of water stress for this study.

Answer:

We thank the reviewer for this remark which led us to further analyse the influence of SWC on the results from the two forest sites. We found that, in fact, the difference in SWC was the major reason for the difference in GPP of the two forests during the first weeks of August. For this reason, as well as to take into account a comment by Reviewer 2, Section 3.2 has been totally rephrased.

Reviewer:

8. Figure 4: The negative values of respiration are disorienting, and not really necessary. Respiration should contribute in a negative sense to NEE, so that the negative sign really ought to appear at page 2496 line 20, in equation (1).

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Answer:

Corrected

Reviewer:

9. Section 3.4: The authors attribute major variations in respiration to the role of the temperature. Surely the fact of irrigation alters the energy balance (and hence temperature) for the maize crop, including the underlying and respiring soil. At least, information regarding the hour of day when the irrigation took place should be included in the manuscript, since the enormous heat capacity of water can allow it to play a dominant role in determining the temperature of any organism/ecosystem, even without considering phase change.

Answer:

First of all, let us note that Fig. 4 of Section 3.4 (Fig. 5 in the revised version), showing the variation of Re with Tsoil at La Cape Sud, presents data from the period with bare soil, after the maize harvest, when, of course, the irrigation had been stopped. Concerning the period when the field was irrigated, we observed no correlation between soil temperature (even at 1 cm depth) and irrigation. This is not surprising since, owing to the regularity of the irrigation, the soil water content near the surface (10 cm depth) always remained at a high level (about 35%), without no strong changes during rainfall or irrigation sequences.

Reviewer:

10. Section 3.6: Considering the authors' stated goal to "characterize the respective contribution of various ecosystems ... to global carbon dioxide ... exchanges", it is quite surprising to this reviewer that the role of harvest has been excluded from the analysis, particularly in the case of the maize crop. I believe that a more complete analysis/discussion could be made following the examples given by Anthoni et al. (2004, Global Change Biology, 10, 2005-2019), or of Aubinet et al. (2009, Agricultural

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and Forest Meteorology,149, 407-418), each of which demonstrates clearly that the including harvest/exports in the annual balance can change flip the source/sink status of crops.

Answer:

The comment by Reviewer 1 (and also Reviewer 2) would be more relevant if we had compared three agricultural ecosystems, or even an agricultural field and two natural, perennial, forests. In the first case, it would be necessary to determine the amount of carbon removed from harvest (which is strongly different between maize, wheat, soja...) to compare the actual carbon balances of the different crops. In the second case, the carbon balance of the crop, including the export of C by harvest, could be directly compared to the accumulated NEE of the natural forests since the latter are not subjected to harvest. However, this is not true for the Les Landes forest where maritime pines are dedicated to biomass production for heating, woodwork or paper manufacturing. Therefore pine forests of south-western France are also harvested, as crops, but not at the same time scale. They generally sustain several clearings during the first twenty years and a complete harvest after about 40 years (as it will be the case for the mature forest of Le Bray in 2009 or 2010). In the Les Landes region, comparisons of carbon balance of agricultural and forested areas can only be performed at the scale of the life cycle of the pine stands. This is of course beyond the topic of this paper. On the question of carbon balance, our aim is to compare over one year the environmental impacts of the three ecosystems, through the analysis of Net Ecosystem Exchange (NEE), not Net Biome Production (NBP = NEE + Export of C), which cannot be determined at yearly scale for the forests. This is specified in the revised version of the paper. However, to allow the reader to get a true sense of the annual carbon balance of our agricultural field (i.e the NBP of the field at yearly scale), we have determined (following Hollinger et al., 2005), and given in the revised version of Section 3.6, the amount of carbon removed from grain harvest (-530 gC m-2; so, with NEE = +160 gC m-2, NBP = -370 gC m-2).

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References:

Hollinger, S.E., Bernacchi, C.J., Meyers, T.P.: Carbon budget of mature no-till ecosystem in North Central Region of the United States. Agr. Forest. Meteorol., 130, 59-69, 2005.

Interactive comment on Biogeosciences Discuss., 6, 2489, 2009.