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Interactive Comment

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

Anonymous Referee #1

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General Comments

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 measurments over two forests (young and mature) with those from a maize crop in the same climatic and soil region. 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.

Specific Comments

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 F_pp, and in equation (2) the nighttime ecosystem respiration R_en. 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).

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 R_ed were allowed to vary? This information is important when interpreting the results, as in Section 3.2.

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.

Page 2499, line 9: Figure 2 presents R_g 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.

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

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.

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).

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.

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 and Forest Meteorology,149, 407-418), each of which demonstrates clearly that the includ-

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ing harvest/exports in the annual balance can change flip the source/sink status of crops.

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