Biogeosciences Discuss., 8, C3204–C3207, 2011
www.biogeosciences-discuss.net/8/C3204/2011/

Biogeosciences Discussions

Interactive comment on "Ecological research and large scale land-atmosphere feedbacks: lesson from the Bouchet's complementary relationship" *by* E. Lugato et al.

E. Lugato et al.

e.lugato@ibimet.cnr.it

Received and published: 21 September 2011

We agree with both reviewers that the current structure of the paper does not highlight properly hypothesis and the main take-home message. We decided to discuss here the main concerns raised by the reviewers and to address the minor concerns in a deeply revised manuscript.

Overarching hypothesis are the following:

1) a direct extrapolation of manipulation experiment results from plot scale to regions risks to be inadequate to properly describe the complexity of land-atmosphere feedback. This hypothesis is based on the simplified description of Bouchet's comple-

mentary relationship theory (CR). According to CR, in water-limited environments not affected by large-scale advection, any change induced externally on actual or potential evaporation or rainfall is constrained by a univocal solution involving the two remaining factors. When related to manipulation experiments, which are inevitably made on relatively small plots, the CR tells us that effects observed at plot scale are not able to produce the same land-atmosphere interactions which are generated at larger scale. The paper intends to validate the CR looking at the relationships between trends in rainfall and pan evaporation in a well-defined water limited region (Australia);

2) Models that simply assimilate field data from manipulation experiments without taking into account land-atmosphere feedback are inadequate to predict future climate scenarios;

Thus, the take-home message is that a new paradigm is required to bridge the gap between field experiments and modeling. The CR could be of guidance in creating such a bridge.

Referee 2

1) We are aware that the use of weather stations, which are not located at close distance from eddy covariance towers, may introduce bias in the analysis. However, we verified that paired stations were both comprised within 200 mm isolines of annual average long term pan-evaporation available for Australia at www.bom.gov.au.

2) Computational method of ETw: we better summarize here assumptions and procedures. These will be detailed in the revised manuscript. To overcome arbitrary assumptions on the choice of ETw, we calculated this parameter with a no linear fitting of ETp vs P at monthly interval. When the fitting is not statistically significant, we proceeded in two ways: i) when energy limited conditions occurred (months in which ETp is constant throughout all precipitation range), we impose ETa = ETp for all precipitation range; ii) we discarded months in which minimum ETp is higher than the corresponding precipitation value as asymptote calculation is problematic.

3) We agree that a forged result may come out if three uncorrelated groups of data are pooled together in a single graph. This is definitely our case since the data showed in figure 3 clearly separate the contribution of the three analyzed stations. Despite some differences, due to two outliers in AU_How, a significant correlation holds for each of them (in the revision we will report three separate correlations with all statistics). The different performances will be discussed in the revised paper as suggested by the reviewer.

4) We entirely agree with the reviewer. Our intention, as said before, was to underline that models, simply assimilating field data from manipulation experiments without taking into account land-atmosphere feedback, are inadequate to predict future climate scenarios. However, the example we did using LPJ was not strong enough since the formulation of evapotranspiration in LPJ is merely with medium complexity. Thus, we decided to delete this part from the revised manuscript.

5) i) We totally rewrote this part in the revised manuscript. The data of figure 5 are clearly showing that there is not a significant trend in Epan and P across 26 years. Instead, if a sub-period is arbitrarily selected (i.e. 1999-2009), significant and well-correlated trends between Epan and P may be observed. This observation is clearly consistent with CR (increase in P = decrease in Epan and viceversa) but cannot be used, as done elsewhere (Jung et al, 2010), to infer abrupt changes in the global water cycle.

ii) We are not addressing here global implications but rather we are underlining what the reviewer is saying: it is not possible to use regional trends over short time scales to explain global trends as done by Jung et al (2010): “after 1997, coincident with the last major El Nino event in 1998, the global evapotranspiration increase seems to have ceased until 2008. This change was driven primarily by moisture limitation in the Southern Hemisphere, particularly Africa and Australia.”

iii) This, of course, is another possible interpretation which seem marginal in Australia.

Other studies have made the hypothesis that wind-speed changes rather than precipitation might have driven changes in pan-evaporation (Roderick et al., 2007 –GRL 34-L17403)

iv) The data reported in figure 5 are used in the discussion to highlight risks associated to the choice of sub-periods. For this we consider that area average over Austrialia is meaningful.

Interactive comment on Biogeosciences Discuss., 8, 6077, 2011.