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Discussions

Interactive comment on "Response of CO_2 and H_2O fluxes of a mountainous tropical rain forest in equatorial Indonesia to El Niño events" *by* A. Olchev et al.

Anonymous Referee #2

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The paper by Olchev et al. attempts to use a four-and-a-half-year record of Eddy co-variance measurements above a mountainous tropical rainforest in Indonesia to detect possible effects of the El Niño Southern Oscillation (ENSO) on carbon and water fluxes between the forest and the atmosphere. Whilst the topic is relevant and innovative, given that this ecosystem type has hardly ever been investigated in terms of microm-eteorological flux measurements, both the methodology and the data analysis suffer from serious problems and shortcomings that weaken the results and conclusions of the study and thus make the manuscript unsuitable for publication in Biogeosciences.

In detail my major concerns are related to the following points:

P. 4409, last paragraph: Already here the paper fails to justify the choice of the eddy covariance method to measure fluxes in a mountain forest, given that, according to the theory, the method is restricted to flat, homogeneous terrain. This problem becomes even more important in the following sections.

P. 4410/4411, first paragraph of section 2.2: A slope of 5 degrees is quite a lot when it comes to turbulence measurements above tall forest canopies. Even gentler slopes have been reported to create massive advective problems, not only during nighttime with respect to a downhill flow of respired CO2 but also induced by perturbations in air-flow patterns (see e.g. the paper by Katul et al. (2006) in BLM about "the influence of hilly terrain on canopy-atmosphere CO2 exchange"). A realistic account of the uncer-tainty of the data caused by systematic errors due to the poor suitability of the site for eddy covariance measurements would be indispensible before interpreting any small variations in gap-filled monthly flux totals.

It does not help that the authors apparently chose to hide the annual sums of net carbon uptake (and presented only monthly totals instead), as this would have revealed at once how unrealistic the order of magnitude is. Looking at the monthly NEE totals shown in Fig. 2 it seems likely that the average annual total must have been something close to 1000 g C per m2 (or 10 t per ha), which is far outside any plausibility range, for example when comparing it to the Nature paper by Luyssaert et al. (2007) about the carbon budget of old-growth forests. The big question is thus how robust and certain the data in the present study are. Was perhaps a large part of soil respiration not seen due to advection? Or did the position of the tower in relation to the hill top create a problem like that described by Katul et al. (see above) that would depend on the prevailing wind direction and thus probably on ENSO as well?

P. 4412, first paragraph: Here we are facing the next serious problem. Understandably (since due to practical reasons in terms of power supply) an open-path gas analyser was used to measure the high frequency fluctuations of the CO2 and H2O concentra-tions. The point is however, that this sensor cannot measure in the rain. Due to the climatic conditions at the research site this must mean that there are data gaps during substantial parts of the investigation period. Filling these gaps with the algorithms described in the paper fails to acknowledge that the relation of ET to environmental factors depends on the wetness of the surface. In other words, when the good data are restricted to dry periods only, these cannot be used to fill the gaps during rainy periods without introducing a serious bias in the water fluxes (see e.g. the study by Ringgaard et al. 2014 in AgrForMet). The method is therefore unsuitable to detect possible ENSO effects (due to interannual variations in rainfall regimes) on ET, and even the gap filled CO2 fluxes remain questionable given that the gaps are not distributed randomly across the variable space.

In addition, the OP sensor is prone to sensor heating in the sun, for which various correction schemes have been suggested (e.g. the so-called Burba-correction). We would need to know how exactly the data were analysed (in terms of the corrections that were applied), rather than just being told that everything “followed existing rules” – of which there are many.

P. 4415, first paragraph: This is the direct result of the aforementioned problem: ET must inevitably be lower during rainy periods because gaps were filled with response functions derived from data measured under dry conditions! This is likely to be the main explanation for the low sensitivity of ET mentioned on page 4417, line 23.

The Discussion section is particularly disappointing as it is restricted to speculations about the variations in the monthly fluxes without mentioning the reliability and/or uncertainty of the data at all. Without such an examination the manuscript remains pointless and unsuitable for publication. If the problems mentioned above would only affect the absolute magnitude of the fluxes but not their variability, the paper might perhaps be rescued. However, the problem is that precisely the ENSO induced changes in weather conditions (such as e.g. radiation and precipitation) which are subject of this investigation, may have induced specific biases on the data. With the information given in the paper we are unable to judge whether the observed effects reflect real variations

in fluxes or just artifacts due to weather-dependent sensor or gap filling failures.

Should the authors choose to write a new manuscript based on these data, two minor points would also need some attention:

P. 4413, first paragraph: What does “mobile station” mean – did it not remain at the same place during the course of the study?

P. 4414, first paragraph: The signs of the deviations from the average monthly values are confusing. The signs of all fluxes considered should be explained somewhere earlier in the paper.

Interactive comment on Biogeosciences Discuss., 12, 4405, 2015.