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Interactive Comment

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.

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We would like to thank the anonymous reviewer (Reviewer 2) for the critical, but nevertheless very helpful comments on our manuscript. (1) they made us clear that our site description was misleading given the incorrect impression that our site is on a steep slope in mountainous area. It is rather on a fairly large plateau at high elevation. (2) the comments inspired us to new analysis that provided clear evidence about the robustness of our results. We will first address these two points and then respond to the specific comments.

(1) The Bariri flux tower site is located at high elevation (1430 m a.s.l.), on a large

plateau surrounded by mountains. To illustrate the size of the plateau - the village Bariri is on the same plateau and about 7 km away. The area directly surrounding the flux tower has a gentle slope. The wind field measured with a 3D sonic anonemeter shows that the slope is about 3° (see Fig. 1 in supplement). Such slope is typical for many sites within FLUXNET and clearly gentler than at some of the FLUXNET-sites that neverthelss regularly publish their results in peer-reviewed journal incl. global syntheses, e.g. Renon (IT), Lägeren (CH), Niwot Ridge (USA), Griffin (UK).

While we agree that hilly terrain poses additionally challenges to eddy covariance measurements, we don't see supported by the literature that eddy covariance measurements are not possible at sites with gentle slopes similar to our site (about 3°).

We are on the contrary convinced that the reservations of Reviewer 2 are too strict and conservative. Asking for ideal measurement conditions would render virtually all real site conditions insufficient for the application of the method. Literature shows that this position is not shared among the bulk of micrometeorologists. At first glance, it is obvious to request exact realization of the requirements that arise from theory, in praxis the question is rather to which degree is the data quality affected by non-ideal site conditions? And here the existing experiences with the method have led to clear recommendation as to how to select and process the raw data in order to minimize the effects, e.g. on annual flux sums. We refer to the latest set of recommendations that have been published recently (Aubinet et al., 2012), with some contributions by authors of this manuscript.

All field data in our study have been selected and post-processed strictly according to the established recommendations for eddy covariance flux measurements. The raw data were screened for outliers and either corrected or rejected. The raw data screening identified clearly the data that were affected by rain, a known phenomenon when using an open-path sensor. The spectra of the remaining data represented the expected features in the inertial subrange well. The night-time fluxes showed a clear dependency on the development of turbulence. When the friction velocity (u*) fell be-low 0.25 m s-1 the average CO_2 flux was significantly lower than at u* > 0.25, beyond which no further u* dependency was observed. This result confirms the reservations of Reviewer 2, but restricts its effect to certain meteorological conditions, i.e. mainly at stable atmospheric stratification and low turbulence. At the same time the clear observed dependency gave a handle to avoid the effects by only including flux values that were measured at u* > 0.25 m s-1. In our manuscript, we mentioned the data processing and data quality assessment only briefly to avoid distraction of the readers focus from the main findings, i.e. the relationships between the CO_2 exchange processes, GPP and RE and climate anomalies. We will provide a more detailed description of the method in the revised manuscript.

Nevertheless, we are aware that even with all precautions taken, a slope may still result in an systematic underestimation of the night time fluxes. That is the reason, why we limit our analysis to monthly anomalies to further reduce a potential bias in our data.

(2) One may argue that even the monthly anomalies of NEE, RE and GPP might be biased by a night-time underestimation of fluxes, particularly if the underestimation of the night time fluxes itself would somehow be related to ENSO. As a consequence, we examined midday NEE, which are typically dominated by GPP and not by RE. As midday NEE data are direct measurements typically with turbulent conditions and thus without an extrapolation from nighttime data, they should be unaffected by potentially flawed data due to a slope or night time problem. Our new analysis based on midday NEE shows a similar clear relationship between monthly midday NEE and the ENSO index (see Fig. 2 in supplement) with an $R^2 = 0.59$. This analysis confirms our main results indicating that our analysis is in fact robust.

Direct response to the comments

Comment 4409 " 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" Comments 4410/4411 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 airflow 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 uncertainty 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.

Answer: Addressed already above

Comment to P. 4410/4411 "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? "

Answer: We would like to point out that our reported GPP values (3150 gCm-2 y-1) are reasonably close to the average for tropical forests (3551 +/- 160 g C m-2 y-1) as reported in Luyssaert et al. (2007). Additionally, we focus on anomalies where systematic errors (e.g. a potential cold air drainage causes a systematic underestimation of RE) are mostly removed.

Furthermore, our site is located at 1430 m elevation, where temperatures are much lower while insolation and water availability are still high. We can now compare our high elevation forest in Sulawesi with as example Tapajos National Forest (TNF) in Brazil in Southern America (http://daac.ornl.gov/LBA/guides/CD32_Brazil_Flux_Network.html). Both sites have similar location near the equator, they are characterized by very high annual insolation and precipitation. However, both sites have a different elevation and of course the different temperature conditions. Taking into account the elevation and temperature differences (about 1430 meters, 5-6°C for air and upper soil layer) we can use a simple approach describing RE as a function of temperature (Q10 or Arrhenius function) and estimate a significant difference in RE values between the two sites. GPP in both sites is quite the same taking into account similar insolation conditions (3260 g C m-2y-1 in TNF against 3150 g C m-2 y-1 in Bariri). Similar results for GPP can be obtained for major sites located in lowlands in SEA region (http://asiaflux.net/). RE due to higher temperature (by 4-7 °C) for forests with similar biomass and lower content of organic matter in soil significantly exceed the RE at our site (about 3100 g C m-2y-1 in TNF (Miller et al. 2011) against 2250 g C m-2 y-1 in Bariri).

Comments P. 4412 " 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 concentrations. 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."

Answer: We agree that open-path sensors are limited in their ability to measure under rainy or dew conditions. As done in virtually all similar studies, we rejected such data. The remaining data fullfilled all spectral and integral data quality criteria. However, in our study we did not use measurements made during dry periods for gap filling in wet periods, to avoid biases from statistical extrapolation from dry to wet meteorological conditions. Instead, we used a process-based model that considered e.g. evaporation from wet leaf surface, i.e. the Mixfor-SVAT model (Olchev et al., 2002; 2008). The model was validated using measured fluxes at various experimental sites (e.g. Olchev et al 2002, 2008), took part in model comparisons (Falge et al 2005) and showed a good performance to describe the temporal variability of fluxes even under wet rainy conditions. The model is also able to predict dew generation and evaporation. Based on this approach, we are confident that our gap filled ET and CO2 flux data do not suffer from respective systematic errors caused by the use of an open-path sensor. In our revised manuscript, we will describe the gap-filling approach and the model in more detail.

Comment: "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.".

Answer: Burba et al. 2008, pointed out that the suggested correction mainly applies in cold environments. Reverter et al. 2011 showed that the effect decreases with increasing mean annual temperature. While this suggests that the effect will be small at our site and will probably not bias our finding, we will nevertheless use one of the proposed methods to correct for this effect and mention the results in the revised manuscript.

Comments P. 4415 " 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!"

Answer: As it was already mentioned we didn't use the method suggested by Reviewer 2 for gap filling. On the other hand , precipitation is not the key factor that is responsible for evapotranspiration rate in most biomes, excluding of course the very arid areas. The rate of evapotranspiration is governed by available energy, temperature, water vapour deficit, wind speed, ecophysiological properties of vegetation, etc.. Tropical rainforest grow in a climate where precipitation significantly exceeds potential evaporation values, in some years even during relatively dry seasons.

The discussion about data uncertainty will be added to the revised version of the manuscript.

Comments P. 4413 " What does "mobile station" mean – did it not remain at the same place during the course of the study?"

Answer: The "Mobile station" is an autonomic meteorological station for continuous measurements of the main meteorological parameters (air temperature, humidity, precipitation, global and reflected solar radiation, soil temperature, wind speed and direction) outside the forest. We will rephrase the term.

Comments P. 4414 " 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."

Answer: The signs will be explained in the final manuscript version.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C2067/2015/bgd-12-C2067-2015-supplement.pdf

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