Biogeosciences Discuss., 6, C399–C402, 2009
www.biogeosciences-discuss.net/6/C399/2009/
© Author(s) 2009. This work is distributed under
the Creative Commons Attribute 3.0 License.

Biogeosciences Discussions

Interactive comment on "A comparison of CO_2 fluxes via eddy covariance measurements with model predictions in a dominant subtropical forest ecosystem" *by* J.-H. Yan et al.

G. Wohlfahrt (Editor)

georg.wohlfahrt@uibk.ac.at

Received and published: 12 May 2009

Yan et al. report a year worth of eddy covariance CO2 flux measurements above a subtropical rainforest in China and compare these measurements with the results of a model (CBM).

One of the reviewers recommended minor, the other one major revisions to be necessary for making the paper acceptable for publication in BG – both reviewers felt that the scientific significance of the paper is only fair. I actually think it is rather poor than fair and thus believe that fundamental revisions, i.e. more or less a complete rewriting of the manuscript, will be necessary to make the paper publishable in BG. Any revised

manuscript, should the authors decide to do so, has to be in large parts fundamentally different from the BGD paper and must include all of the reviewers and my recommendations. Any revision not satisfying these requirements will be rejected. Should the authors not decide to submit a revision, I would like to thank them for choosing Biogeosciences as an outlet for their research.

General comments: Overall, the study appears to me premature and preliminary (as the authors admit on p. 2916, l. 19) - this needs to be changed – the paper must make a significant contribution to the field. On the experimental side we need a clear and transparent description of the methods, which have to be state-of-the-art (see comments by reviewers). Focussing just on daytime CO2 exchange is a major restriction which reduces the significance of the manuscript (see comments by reviewers), and in fact because it is so unusual their results might be misunderstood (e.g. Fig. 4). Here the authors are encouraged to seek other ways of getting a proper handle on nighttime fluxes and ecosystem respiration and thus eventually NEE. Most importantly we will need uncertainties on the numbers reported and a defensible justification for how nighttime NEE is derived. Currently it seems the authors have chosen nighttime NEE so that it will fit with annual NEE determined by other methods. On the modelling side we will need a clear and transparent description of how parameters were derived, which parameters have been calibrated and if so how this was done. Next, we need an estimate of uncertainty introduced by parameter selection. Currently, the authors simply stick in some numbers and report a single magic number as their output – this is an excellent example of how not to do modelling. Also I wonder what is to be learnt from their modelling exercise beyond just comparing measurements and model results. Does the model tell us something about the processes driving NEE at this site or does the mismatch between measurements and simulations indicate some model deficiencies? From Fig. 6 it appears to me that the model does not predict NEE < -12 μmol/m2s – this seems to be a clear indication of a problem with model parameterisation or possible structure. With a process-oriented model it should be possible to go beyond the descriptive discussion as presented on p. 2923-2925. Finally, any revised

manuscript must be checked by a native speaker to improve the English – because the manuscript is full of mistakes I do not mention all of them in the following.

Detailed comments: (1) p. 2914, l. 22-23: reference for this statement missing (2) p. 2915, l. 1: “vegetation surfaces” – what does this mean ? (3) p. 2915, l. 4: “Rannik et al.” – there are more suitable references out there (4) p. 2916, l. 4: why “must” if differ ? (5) p. 2916, l. 22: you did not develop the model in this paper (6) p. 2917, l. 3-24: what is forest height; report full species names upon first mentioning (7) p. 2918, l. 2-22: at which height were measurements made – 27 or 38m? “sonic temperature” instead of just temperature; “mixing ratio” instead of “mixed ratio” – although the Li-7500 actually measures molar density; what about density corrections and self-heating of the instrument; you probably used “linear detrending”; “belowground” instead of “underground”; the IRTS-P measures infrared surface temperature; specify heights of layers both above- and belowground (8) p. 2918, l. 24: was the CBM model used for gap-filling ? – the two references by Wang do not deal with gap-filling (9) p. 2919, l. 1-3: how come a canopy model simulates soil fluxes ? (10) p. 2920, l. 3-7: belongs to methods section; how long were short gaps that were filled by linear interpolation ? (11) p. 2920, l. 12: “half-hourly” instead of half an hour (12) p. 2920, l. 19-26: belongs to methods section (13) p. 2921, l. 3: why this period – looks pretty arbitrary (14) p. 2921, l. 8-13: what is the point of showing these diurnal courses ? (15) p. 2921, l. 13-15: belongs to discussion (16) p. 2921, l. 18-19: belongs to discussion (17) p. 2922, l. 9: what are the slope and y-intercept of a linear regression, i.e. bias ? (18) p. 2922, l. 13: to what does this difference amount to on an annual scale ? the model underestimated NEE ! (19) p. 2923, l. 2-23: much too descriptive – need to go beyond; use statistical analysis or other tools (20) p. 2923, l. 27 – p. 2924, l. 2: belongs to methods section (21) p. 2924, l. 2-29: the assessment of nighttime NEE is very crude and seems pretty arbitrary – this way you can get any correspondence you like; this needs to be made objective and defensible; if the ratio of soil to ecosystem respiration is 65-80% then NEP should be at least 186-300 gC/m2 (22) p. 2925, l. 1-6: I can hardly imagine that changing these two key parameters has no effect; what do you mean with

better correspondence ? (23) Table 1: what does DZ mean ? (24) Table 2: “Standard deviation” instead of “Stand deviation”; add units (25) Fig. 1: units are W/m2 (26) Fig. 2: units of soil temperature are not readable (27) Fig. 3: “solid” instead of “dashed” line (28) Fig. 6: “solid” instead of “dashed” line

---

Interactive comment on Biogeosciences Discuss., 6, 2913, 2009.