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Biogeosciences Discussions

BGD

6, C680–C684, 2009

Interactive Comment

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.

Y-L Li

yuelin.li@uni-bayreuth.de

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Dear Editor,

Many thanks for your valuable comments. We have incorporated your suggestions into our revised manuscript. I briefly report the changes that we have carried out in the following list,the details will be given in the revised manuscript soon.

A) Responses to the Editor's general comments

General Comments:

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.

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 < -12umol m-2 s-1, 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.

Response:

Thanks a lot for your review. As we responded to the two anonymous Referees, this work is just a beginning of eddy flux measurements in the south of China. We hope it plays an important role being a part of the growing ChinaFlux Network, we also hope to publish our results soon and it will encourage us to further our study. We agreed that missing night CO2 flux data analysis is a defect for readers to understand our research, we will rethink the nighttime flux data so that we could present our results more clearly, at the same time, we realize that a long period measurements data provide a bigger picture of the inter-annual variability, we tried our best to present the data in 2003-2005 in the revised version. Afterwards, we revised our manuscript via Reichstein et al (2005) methods. Based on more data, we justified our methods and conclusions as you had suggested.

B) Responses to the Editor's specific 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 temper-

BGD

6, C680–C684, 2009

Interactive Comment

ature; “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 gapfilling ? – 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

Response: Agreed. They have been corrected in our revised manuscript.

C) Summary

All your comments are valuable, which help us to improve the manuscript considerably. We feel that we have been able to answer almost all of the questions of the reviewers. Here, I would like to request our editor Dr. Wohlfahrt give us more time to fix our problems, it will take a little bit longer time for our English-speaking co-author to improve the English. We hope that these changes are appropriate to permit publication of the results in Biogeosciences.

BGD

6, C680–C684, 2009

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Sincerely,

Yelin Li (for the authors)

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

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6, C680–C684, 2009

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