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## ***Interactive comment on “Simulating carbon exchange using a regional atmospheric model coupled to an advanced land-surface model” by H. W. Ter Maat and R. W. A. Hutjes***

### **Anonymous Referee #1**

Received and published: 18 December 2008

The paper presents quite extensive comparisons an atmosphere-land-surface model with flux tower and aircraft observation data. It provides lot of interesting material but the paper is very superficial. In the present form it cannot be accepted to Biogeosciences and it needs major revision.

Major:

1. In several parts it is said that the model does a good job etc. (Abstract, line 14) although there is no real quantitative proofs of that. The plots mainly present model results and observation together without any statistical analysis, at least simple 1:1 plots would be presented and  $r^2$  values could be discussed. Abstract says that "...fluxes of

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heat, water....are reasonably well simulated", and then later it is said that "...latent heat fluxes are underestimated.", this is at least a bit contradicting. Also, looking Tables 4 and 5 and the great mismatch between the model and aircraft data I would not say in Abstract that "...regional meteorology is captured by the model."; P. 4172, line 12-13 says that "...except for some midday ...looks reasonable.", what about the last 3 days of the grassland site, the agreement is also very pure there. First sentence in Discussion and conclusions is very superficial and gives too good image of the results. More critical style and deeper (statistical) analyses is required in many parts of the paper.

2. How accurate are the aircraft fluxes? Is there any idea of their possible systematic and random errors?

3. Would it be good to make a sensitivity study for North Sea sink in Fig. 5, similarly to the sensitivity analyses to urban and biogenic sources/sinks? For example to put the dynamics which is now missing.

Minor:

1. Abstract line 6: say what is the region studied.

2. p. 4175 line 17: I think the absolute value of biogenic fluxes were increased by 20%, not the fluxes themselves because they are negative and increasing them would reduce the uptake?

3. p. 4177 line 22-23: I do not know what is meant by "...real clouds move and redistribute themselves"; clouds?, do you mean plumes? Lines 26-28 telling on Lagrangian and Eulerian approaches is mystical.

4. The paper ends with the results in Fig. 17 not discussed earlier and there are no general conclusions, I would move Fig. 17 much earlier and put something more general in the end.

5. Fig. 10. Dark blue and light blue lines are not distinguished very well.

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6. Fig. 15. I would suggest to subtract the control simulation from the simulation with the 20% increase so that the difference would be positive, then with the first glance a reader would get the idea that there is the increase; the similar comment for Fig. 16.

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Interactive comment on Biogeosciences Discuss., 5, 4161, 2008.

**BGD**

5, S2475–S2477, 2008

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