

Interactive  
Comment

## ***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 #2**

Received and published: 11 December 2008

#### General Comments

The paper describes and tests a model - or rather, combination of models - to predict the carbon dioxide concentrations and fluxes over the Southern half of the Netherlands with a mesoscale (km) spatial resolution and on a timescale resolving diurnal variations. The paper is well-structured. All components of the model are described with adequate, but not exhaustive, detail. Each component of the model and input data (RAMS, SVAT, soil data, land-use, emissions data) has been published and tested elsewhere; the contribution of the authors is to bring them together for the new purpose.

The authors embark on a solid approach to test their model's performance, using time

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series and vertical profiles at fixed points, as well as quasi-snapshots on horizontal paths (from aircraft data). Unfortunately, the discussion of the results is in many places a bit short, and the authors' criteria of what constitutes good agreement or good model performance remain obscure (see Specific Comments). The presented discrepancies between simulated and measured fluxes and concentrations leave the reader unsure about what has been achieved. The Discussion parts, therefore, need considerable revision, and should try to address the following questions: Which inputs provide the largest uncertainties to the results? What is the overall uncertainty of CO<sub>2</sub> fluxes - can that be quantified? What purposes can the model serve in the future, at this level of uncertainty?

Further, the experimental data are restricted to a few summer days. What is therefore missing in the model assessment is the seasonal dimension. Could perhaps some comments on seasonal patterns be added to the discussion? Is summer (with active vegetation everywhere) the most complex situation to model accurately? Or is winter, with increased relative importance of anthropogenic emission sources, in fact trickier because of the patchy spatial distribution of these sources? (At least the flux tower data should be available year-round for testing these points - I am not suggesting the authors should add substantial amounts of new data to this paper, but if they have done some test runs at other times of year then I would encourage them to comment on the aforementioned questions.)

A number of figures lack proper axis labels, and some figures (or their labels) should be enlarged.

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### Specific Comments

Abstract: First sentence is Intro material, unnecessary. Suggest to delete "A large... question:" and begin instead "This paper is a case study to investigate what the main controlling factors... at a regional scale are. ..."

p.4162 r12: delete "tall towers" (because profiles from aircraft are used, too).

p.4162 r14: "generally does a good job" is vague and casual language. Suggest "simulations are in good qualitative agreement with reality" (or else, be more specific about which features are simulated with what level of agreement).

Next row: Reviewer disagrees with "The validation of the model demonstrates that surface fluxes of heat, water and CO<sub>2</sub> are reasonably well simulated." Sensible heat is not presented, and latent heat fluxes do not agree well.

p.4162 r18: delete "simulated" at the end of the row (redundant).

p.4167 bottom, Section 2.3: not essential for this paper, but the unfamiliar reader would be grateful for one sentence explaining why Fig. 5 shows such a prominent peak in October.

p.4170, Section 2.6: Who operated the flux towers and the aircraft? Were there two aircraft, one for eddy fluxes and one for vertical profiles? Contributing organisations should be mentioned here, and crucial researchers, technicians and pilots should be named in the Acknowledgements.

p.4170 r5 "low-flying": please specify altitude.

p.4171 r4: Does "bi-linear" mean linear in space and in time? Please clarify.

p.4171 r16f: "a number of days" - "other days": please be accurate and specific. On the last 3 days, the model underestimates radiation. But on the 2nd and 4th day, the opposite is true. Does that variability imply that the "overall... 25 %" is a random result, due to the selection of days? It should also be pointed out that the 2nd day is used for later comparisons to aircraft, and that on this day the model seems to predict cloudless conditions while in reality there was changing cloud cover.

p.4172 r12-15: ".. follow... quite nicely", "looks reasonable", "in accordance with the measurements" are vague and casual statements and should be revised. What is prob-

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ably intended to say here is that measured and modelled data largely co-vary in time. When it comes to actual flux values, the authors do not say what they consider to be the threshold between agreement and disagreement. Also, this reviewer is concerned about systematic differences in Fig. 9. For all days at Haarweg, and all but the 2nd and last at Loobos, both the uptake and the emission of CO<sub>2</sub> are underestimated by the model. Why is there a daytime underestimate of photosynthetic flux even on days when radiation is overestimated? And why the consistent nighttime underestimates? Further, to investigate the effects of such biases, could the daily integrals of the CO<sub>2</sub> flux from model and measurement be tabulated? It is hard to gauge from the figures, but daily integrals may well disagree by 50 % either way.

p.4173, around Eq. (1): I might be wrong, but it looks to me as if the data in Figs 10 and 11 have been divided by mean  $F$  rather than by  $\sigma$ . (I do not think it is arithmetically possible that, in Fig. 10, almost all ( $F - \text{mean } F$ ) lie within  $\pm 1$  std. dev.; neither is it plausible that a substantial amount of points lie outside  $\pm 5$  std. dev., as in Fig. 11.) Please check, and also add correct axis labels to the figures.

p.4173 middle, discussion of latent heat fluxes: The large std. dev. is probably due to sampling statistics, as the aircraft traverses convective cell structures at random. But this does not explain the huge systematic differences in the morning. Could these be due to the way the nascent CBL is simulated? In Fig. 8, all three panels show the steep morning rise on 16 July to occur earlier in the measured data than in the model data. Would the model data of, say, half an hour later provide a better match, indicating that the poor agreement is largely due to a timing problem in the simulation of BL growth? - Alternatively, could there be errors in the aircraft hygrometer that are dependent on sun angle? Some more effort trying to understand the huge differences would be nice.

p.4174 r6: "heat" flux is not shown, should it be "latent heat"?

p.4174 r14-16: "Looking at..." Agree with this sentence, but why does the modelled SURFACE value seem to correlate better with the aircraft data than the modelled flying

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altitude value? What was the altitude, what model grid cell(s) were used, and how? Again, could this be due to a timing problem with BL growth simulation?

p.4175 r9: "This contrasts with..." I do not think there is a contradiction. In Fig. 14, for the morning of 16 July, measured [CO<sub>2</sub>] drops earlier than modelled, which is in agreement with Fig. 13 at 60 m. On 16 and 21 to 23 July, there is a clear timing mismatch between measured and modelled concentrations, while the amplitudes are in good agreement. By contrast, on other days, the timing seems to be in agreement, but modelled daytime concentration is lower than measured. Is on these days photosynthetic activity overestimated because radiation is overestimated? How does that fit with Fig. 9, where photosynthetic flux is generally underestimated (see comment above, p. 4172)?

p.4175f, Section 3.3: Compared to the rather small changes observed in the sensitivity tests, the [CO<sub>2</sub>] mismatches in Fig. 14 (of order 15 ppm) appear substantial. How much can the model predictions then be trusted, and for what kinds of future studies can the model be used with confidence?

p.4176 r.7-9: "decent job" and "very promising" are, again, vague and casual statements, probably intending to say that many qualitative features in time and space are well-simulated. But against which criteria should the model's performance be measured? What was the purpose here? Was it achieved, and with what uncertainty? What is the "promise" for the future?

p.4177 r.14: "although... showed" does not make sense, delete. Please clarify if "absence of turbulent diffusion": refers to a residual NBL.

p.4177 r.21ff: "simple footprint models" do not deal with clouds at all. Clouds do not change the footprint, they change the energy input at the surface. It is not a shortcoming of the footprint model that this occurs, but an added complexity when trying to apply them. Please reformulate this passage.

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p.4177 r.25: Has "(Hutjes et al.,2008)" been submitted? If yes, please update the reference. If no, then it is not a valid citation!

p.4178 r.2: "... dynamics seem to be reproduced well by the model": suggest to add "except in the early morning", because of the observed timing problems.

p.4178 r.5: should "is slower" rather be "occurs later"?

p.4179 r.13: should "It also suggest" be "This study suggests"?

Next row, insert "concentrations" between "measure" and "close".

p.4179 r.26ff: "The effect of better representations...": Is that really important? Would that not, for the most part of the study region, only correct for a near-constant offset?  
- The paper ends here with this, in my view, marginal point, rather than any solid conclusions or recommendations.

p.4179: What Section 4 lacks is an outlook: what can this kind of combined model be used for in the future? And also, the authors claim at the outset that there is a "scale mismatch" between local and continental scales. What, exactly, is that mismatch, and what does this paper contribute to resolve it?

p.4180 r.4f: should "in the field" better be "from the flux towers"?

p.4180, Acknowledgements: who provided the aircraft profile data?

p.4182 r.20f: "to be submitted" is not a valid reference. Update or remove.

p.4184 r.20f: "Ter Maat..., 2007": should probably by now be published or in press, 2008 or 2009. Please update.

Table 2: what does "optimized" mean? Explain (or refer to text, if explained there).

Table 3: It would be useful to add canopy heights and measurement heights to the table.

Table 4, caption: add "in  $W\ m^{-2}$ " after "latent heat fluxes".

Table 5, caption: similarly, give units of the CO<sub>2</sub> fluxes.

Fig. 6, caption: For clarity, I suggest to replace "these emissions in time for mobile emissions (left) and for non-mobile emissions (right)" by "these emissions: mobile emissions (left) vary diurnally, non-mobile emissions (right) vary seasonally".

Figs 7 to 9 and 14: It appears that date labels are placed at 00:00 hours, that should be said somewhere (since alternative choices, 12:00 or 24:00, are possible).

Figs 8 and 9: It would help the reader if each panel was labelled not only with the place name, but also with the vegetation type it represents.

Fig. 10, caption: after "deviation from average" should probably be inserted "divided by the standard deviation" (in accordance with Eq. 1). However, see comment above, p.4173, why I doubt that these data are correctly presented.

Figs 10 and 11: all panels should be enlarged and provided with complete and correct axis labels.

Figs 12 and 13: Axis labels at the top of the respective right panels should be moved to the bottom, and the left-hand axis should be labelled "Height (m)".

Fig. 12: It seems that the model gives far too strong an inversion and too low BL depth in the morning hours. Why? And would that explain the large discrepancies in the early morning latent heat flux (see earlier comments)?

Figs 15 and 16: Tick labels should be enlarged, and axis labels are missing.

Fig. 16: what time period / time of day does the figure represent?

Fig. 17: Tick and colour bar labels are hardly legible, should be enlarged. Axis labels are missing.

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Technical Comments

p.4162 r22: "test" should be "tests".

p.4163 r7: "lead" should be "led" or "has led".

p.4163 r23: "h" in "biospheric" is missing.

p.4170 r2: "Campaign wise" should be one word.

p.4172 r24: "spatially" should be "spatial".

Next row, "of the flights" is redundant, delete.

p.4174 r17: replace "observational also" by "field" and "have been" by "were".

p.4174 r22: "an" should be "and".

p.4175 r27: "story is" should be "results are".

p.4176 r4: "forested" should be "forest", and "contributes slightly more" should probably be "reduces".

Next row, "this" should be "that".

p.4177 r.17: replace "try to fly" by "flew".

p.4177 r.23-27: "lagrangian" (twice) and "eulerian" should start with capital letters.

p.4178 r.18: "relative low simulated CO2 air" should be replaced by "air with relatively low simulated [CO2]".

p.4179 r.15: replace "are controlling" by "control".

p.4179 r.19: "heterogenic" should be "heterogeneous".

p.4179 r.25: delete "more" after "further".

Fig. 1 caption: replace "Schematization" by "Schematic".

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