

Interactive comment on “Evaluation of a regional air-quality model with bi-directional NH₃ exchange coupled to an agro-ecosystem model” by J. O. Bash et al.

Anonymous Referee #2

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The main innovation of this study is to couple an agro-ecosystem model to an air quality model. This is an important and very interesting development. This paper is one of a series which this group of authors is presenting, which pushes the science forward, and which should have important policy implications. The paper will surely make a good contribution to ACP, but I found it a little confusing in its presentation.

I have a number of specific comments below, but in general, it is not easy to know which parts of this manuscript are describing new developments, and which are repeating material from other papers by this group of authors, for example those of Cooter et al. (2010, 2012) and Walker et al. (2012). The Cooter paper for example also makes use

C4409

of EPIC, so when this paper says that it is the first study to use an agro-ecosystem model, is it?

Also, the writing could be clearer. The authors have a fondness for long sentences, often without the use of commas, and containing more than one subject. The text should be carefully checked, with a view to using clear simple English.

1. P.11376, lines 11-14. This paragraph is hard to understand. Clarify.
2. P.11376, line 24. Find a more relevant citation. The Sutton reference does not address health issues in any detail.
3. P.11377, line 7. Here the Sutton reference would fit very well though.
4. P.11378, lines 1-3. The cited reference (Erisman et al. 2007) cannot logically be used to support a statement, based partly on a 2011 paper, otherwise on two earlier papers that Erisman doesn't cite, that recent changes leave a gap of 30%.
5. P.11378, line 9. It could be good to give a more recent ref than 1993 also, e.g. Fowler et al., 2009.
6. P.11378, line 12. I would say that current AQ models do not generally include a mechanistic description. The study of Wichink-Kruit et al. (2010) did include bi-directional exchange.
7. P.11378, line 20. plural, alters
8. P.11378, lines 17-21. Long sentence, difficult to follow.
9. P.11380, Section 2 intro. This text should make it clearer how the current study differs from Cooter et al. (2010) and 2012. The 2010 paper was already using CMAQ, bi-directional exchange, and at least some of EPIC.

C4410

10. P.11380, line 23. Which depths do these layers represent?
11. P.11380, Equation 1. Is the factor h_m really needed? The basic equations should be as simple as possible, and units can as well be per m^2 rather than per ha. If a units term is needed, it is clearer to put it in the numerator than the denominator. Actually why have two denominators in two styles?. (I know Massad had this, but in general it is not good practice. Also the style a/bc can be misread $(a/b).c$ or $a/(b.c)$, explicit parentheses never harm.
12. P.11380, Equation 2. Where is the layer structure in this equation? What happens with diffusion/transport between layers? This looks like the equation for a single layer model to me.
13. P.11381, Section 2.2. Again, it is not obvious how much of this is new.
14. P.11381, Eqn. (3). Why is the canopy compensation point physically located at $0.5 h_c$? This is below the aerodynamic displacement height, so not entirely obvious for a big-leaf approach. Between which points is Ra calculated?
15. P.11381, line 12. Again, unnecessary complications with units. Why is V_m needed? There is nothing in this equation containing liters, and the authors have anyway switched units compared to those used by Nemitz et al. (2001). The switch of the meaning of χ from volume mixing ratio in previous papers (e.g. Nemitz et al, and even the companion Walker et al. 2012) to the mass concentration used here is also confusing. I would suggest that the authors find a different symbol for the mass-concentrations (C perhaps). Or just re-write in the same units as normally used.
16. P.11381, line 17. What is a "cropping practice".
17. P.11383, Why run such an old year as 2002? Surely the quality and quantity of data has increased since then?

C4411

18. P.11383, line 21. Use SI units.
19. P.11384, Eqn. (9). Doesn't this bias-adjustment lead to a mass error, since the scaling of deposition fields is done after the model runs are finished? How large were these bias-correction factors, and what were their seasonal variations?
20. P.11384, the bias correction of rainfall from WRF is obviously important to this manuscript, and some discussion appears later, but the lines around Eqn. (9) give too little information.
21. P.11385, Section 3.1: a Table would help to summarize all the percentage changes discussed here, then the number of significant digits could be reduced in the text. (I get nervous with modelers who can predict changes to 3 significant digits.)

Also, since almost no information has been given on the extent of the bias correction (e.g. "may be too high" is a little vague), it is not possible to really understand these paragraphs.

This paragraph doesn't read very well anyway. Emissions are for example 'too high' or 'lower', and I can infer that they mean with respect to the base-case, but it should be clear.
22. P.11386., line 3 biased high or low?
23. P.11387, Sect. 3.3. Are these discussions about total nitrate aerosol, or fine mode? (How important is coarse mode nitrate in these simulations?)
24. P.11387, Sect. 3.3. The discussion of nitrate biases pre-supposes that the model should get the ammonium-nitrate equilibrium right. There are some recent papers which suggest that the partitioning coefficient used in models is likely different to that found in observations (e.g. Aan de Brugh et al., 2012). How do such considerations affect your discussions?

C4412

25. P.11390, line 1. No need to use the word successfully. Presumably there would not be a paper in any other case.
26. P.11390, why is the last line giving conclusions from another paper. The conclusions should be from this paper, or at least explain why another paper is referred to.
27. Figures: the map-plots should be larger (2 per page), as the contents of the bullets are difficult to see. (Maybe it would be worth zooming in on some interesting areas?)
28. Fig. 5. I didn't find this plot very useful, most of the points are buried in the bottom corner, and curved regression lines don't help. Re-plot as log-log, so we can see the full range of concentration changes better.

References

Aan de Brugh, J. M. J., Henzing, J. S., Schaap, M., Morgan, W. T., van Heerwaarden, C. C., Weijers, E. P., Coe, H. & Krol, M. C. Modelling the partitioning of ammonium nitrate in the convective boundary layer *Atmos. Chem. Physics*, 2012, 12, 3005-3023

Fowler, D., Pilegaard, K., Sutton, M., et al., Atmospheric composition change: Ecosystems-Atmosphere interactions *Atmos. Environ.*, 2009, 43, 5193-5267

Wichink-Kruit, R., van Pul, W., Sauter, F., van den Broek, M., Nemitz, E., Sutton, M., Krol, M. & Holtslag, A. Modeling the surface-atmosphere exchange of ammonia *Atmos. Environ.*, 2010, 44, 945 - 957

Interactive comment on *Biogeosciences Discuss.*, 9, 11375, 2012.

C4413