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Interactive comment on "Dry and wet deposition of inorganic nitrogen compounds to a tropical pasture site (Rondônia, Brazil)" *by* I. Trebs et al.

Anonymous Referee #2

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General comments:

The paper gives an account of measurements of concentrations of inorganic nitrogen compounds and inferential modeling of deposition to a remote pasture site in the Amazon basin. Measurements are made throughout different characteristic seasons and the results are scaled up to annual estimates.

The paper provides a very valuable account on a detailed study, showing the results of an advanced set of instrumentation and a very detailed treatment of the data.

The theoretical account and the evaluation of characteristic time scales for turbulent transport and chemical transformation is a very valuable part of the paper. This kind of

analysis is rarely seen before applying the inferential method(s) of calculating deposition. Also, the input parameters for calculating the deposition are chosen carefully and in case of doubt a range is used.

Most relevant nitrogen species are measured. However, a remark is given in the paper that organic nitrogen compounds dissolved in rainwater has been shown to contribute substantially to the overall deposition.

The N-deposition values given in the paper are representative of cattle pastures. These constitute about 15% of the Amazonian region, the remainder being tropical rain-forest. The comparison with and statements about predictions made for the Amazonian region by global CTMs are therefore hardly valid. I am also a bit concerned about the scaling up to annual values. The measurements covered about 50 days during September through November. It is claimed that this period covers different situations representative of a full year. However, the paper itself does not give such "evidence".

Specific comments:

p. 3133, l. 1: "doubled" since when?

p. 3133, l. 10: "to increase the net carbon sink" could be deleted.

p. 3134, l. 6: "elements" could be deleted.

p. 3134, l. 9: What is meant by "surface layer"? Atmospheric boundary layer?

p. 3134, l. 21: Delete "N receptor". Is "North Sea" an urban region?

p. 3135, l. 10: The study by Kirkman et al. was not made in the same year. Is there any information on inter-annual variation in N-deposition in the tropics?

p. 3137, l.22: Delete "flux"

p. 3139, l. 17: How large a percentage of the data were rejected (day/night)?

p. 3140, l. 7: Explain why "the observation of a net NO_2 deposition flux .. by Kirkman

ACPD

5, S1266–S1269, 2005

et al." justifies the use of inferential modeling in this study. Did Kirkman et al compare their measurements with the same kind of inferential model? Even if they made such a comparison and were successful their study could hardly "justify" inferential modeling for other N-compounds.

p. 3140, l. 13: It would be useful at this point to give the definition of the canopy compensation point.

p. 3146, l. 8: It would be good to give some kind of reliability of the representativeness of the VWM's measured during the experiment period for the full year. How does rainfall vary from year to year? Which influence would such a variation have on the upscaling to annual values?

p. 3152, l. 14: Is the cattle on the pasture all year around? How does this influence the upscaling?

p. 3155, Section 4.6: I suggest that a table is added giving the daily fluxes of each species in each of the three periods.

p. 3157, l. 21: "is" should be "are"

p. 3160, l. 19: I thought that bi-directional fluxes of NH_3 and HONO were already taken into account in the calculated net "deposition". If this is not the case how were the fluxes then separated? NO and N_2O were, however, not included in the calculations given in the paper. NO emissions were earlier in the paper judged to be of minor importance. However, they seem to be on the same order of magnitude as some of the small fluxes included (e.g. HONO).

p. 3160, l. 23: For completeness it could be mentioned that a normal loss of nitrogen is by emission of N_2, which is very seldom measured due to practical difficulties.

p. 3161, l.3: The comparison with results by Kirkman et al. might be expanded. When did Kirkman et al. measure their fluxes? If there is an overlap in seasons, a direct comparison for a specific season could be interesting. How did Kirkman et al. scale up

ACPD

5, S1266–S1269, 2005

to a full year? Could the rather large relative difference in annual estimates imply that the upscaling procedure (of either study) is not valid?

p. 3162, l.1: Whether it is a “surprise” or not is not really relevant. I do not think it is easy (and maybe not really relevant) to compare a plot study (a pasture) with a model estimate for a whole region, which includes a lot of other ecosystems.

Table 3: I t is a bit contradictory that the Scenario “high” means a low resistance. Maybe it would be better to write “Flux scenario”.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3131, 2005.