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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.

I. Trebs et al.

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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 is therefore hardly valid.

Response: The authors are aware of the fact that the field site does not represent the entire Amazon ecosystem. The authors admit that the argumentation chosen in the manuscript is quite strong and will be changed in order to draw rather moderate conclusions.

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”.

Response: Surely, this paper does not give the evidence. However, there is a bunch of previous studies that were cited in this manuscript, e.g.:

Andreae, M. O., Artaxo, P., Brandao, C., Carswell, F. E., Ciccioli, P., da Costa, A. L., Culf, A. D., Esteves, J. L., Gash, J. H. C., Grace, J., Kabat, P., Lelieveld, J., Malhi, Y., Manzi, A. O., Meixner, F. X., Nobre, A. D., Nobre, C., Ruivo, M., Silva-Dias, M. A., Stefani, P., Valentini, R., von Jouanne, J., and Waterloo, M. J.: Biogeochemical cycling of carbon, water, energy, trace gases, and aerosols in Amazonia: The LBA-EUSTACH experiments, J. Geophys. Res.-Atmos., 107, 8066, doi:8010.1029/2001JD000524, 2002.

Artaxo, P., Martins, J. V., Yamasoe, M. A., Procopio, A. S., Pauliquevis, T. M., Andreae, M. O., Guyon, P., Gatti, L. V., and Leal, A. M. C.: Physical and chemical properties of aerosols in the wet and dry seasons in Rondonia, Amazonia, J. Geophys. Res.-Atmos., 107, 2002.

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Trebs, I., Meixner, F. X., Slanina, J., Oties, R. P., Jongejan, P., and Andreae, M. O.: Real-time measurements of ammonia, acidic trace gases and water-soluble inorganic aerosol species at a rural site in the Amazon Basin, Atmospheric Chemistry and Physics, 967-987, 2004.

which show that different seasons exist with different pollution levels typical for different times of the year (seasons) in the Amazon region. This is well known for about 20 years now. Considering that in other studies, measurements of only some days are scaled up to one year, the authors believe that upscaling of reliable on-line measurements performed during 50 days should be a reasonable approximation for conditions during the whole year. We will add some statements about the assumption that September is representative for the entire dry season, October for the entire transition period and November for the wet season.

Reply to specific comments:

p. 3133, l. 1: “doubled” since when?

Response: since the 1960s; this will be added

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

Response: The atmospheric surface layer is also called the “constant flux layer” and typically represents the lowest 10 % of the atmospheric boundary layer. In this layer, fluxes of momentum, heat, water vapor and other scalars are considered to be constant with height. (see common textbooks of micrometeorology Stull, Oke etc.)

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

Response: the sentence will be changed. North Sea receives urban pollution. The sentence was misleading.

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?

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Response: no, such information does not exist.

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

Response: about 10 %, a comment will be added to this paragraph.

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

Response: This is a misunderstanding. The fact that Kirkman et al. observed net deposition of NO_2 does not justify the use of the inferential model for other N compounds. The sentence says that "...justifies the application of the inferential model for NO_2 in our study".

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

Response: This will be added.

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?

Response: It is true that storm sizes can vary from year to year, which is one of the reasons why we used long-term rainfall amounts to obtain the annual deposition rate. However, the rainfall amounts as well as the air mass trajectories during the SMOCC period, it means September, October and November, did not vary significantly variation in comparison the same period during other years, consequently these months can be considered as being reliable representative of the meteorological conditions during this period in this area.

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

Response: The number of cattle on the pasture is relatively constant during the whole year, thus the upscaling should be independent of this factor.

p. 3157, l. 21: “is” should be “are”

Response: This will be changed.

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).

Response: Hourly upward fluxes and downward fluxes were separately summed up for each season. The statement is simply that 7.3 - 9.8 kgN ha^{-1} yr^{-1} are deposited and 2.7 - 6.8 kgN ha^{-1} yr^{-1} are emitted. The word “net” should not be used in this context. The formulation will be revised. Some of the discussion and Figure 13 will be changed.

NO was not included in the paper because this has been studied in detail by Kirkman et al. 2002. N_2O was not in included because it was not measured. However, HONO was included to actually show that HONO deposition was small at the site and because this trace gas has never been measured before in a tropical region. That the flux is small is therefore quite a new finding.

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.

Response: This will be mentioned.

p. 3161, l.3: The comparison with results by Kirkman et al. might be expanded. When

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

Response: Kirkman et al. 2002 performed their measurements in 1999 (from 24 September to 27 October, dry-wet transition period). The up-scaling procedure of Kirkman et al. was different. They wrote: "Assuming, despite the low data counts, that measurements during LBA-EUSTACH-1 and LBA-EUSTACH-2 were representative for the wet and dry seasons and that these seasons are about equal in length, this constitutes a net NO_2 sink of 0.73 kg N ha^{-1} yr^{-1}." If we perform the same procedure for our measurements in October 2002 (transition period) we calculate a value of about 1.0 kgN ha^{-1} yr^{-1} of NO_2 deposition. The NO_2 concentration measured in October 2002 was about 30 % higher than measured by Kirkman et al., which explains the difference. Some more comments on this will be added to the manuscript.

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.

Response: see comment above.

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".

Response: This has been explained in detail in the beginning of section 4.4.

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