Atmos. Chem. Phys. Discuss., 5, S1333–S1338, 2005
www.atmos-chem-phys.org/acpd/5/S1333/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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 #3

Received and published: 22 June 2005

General Comments:

The authors tackled a difficult task, the measurement of all major components of dry and wet deposition of inorganic reactive nitrogen species. They did so under rustic field conditions in the Amazon region of Brazil. Their results demonstrate that the deposition of reactive nitrogen under these conditions may be substantial. They show that wet deposition is more important than dry deposition although the vast majority of the work treats dry deposition. This imbalance is awkward but reflects the author's concerns with the technology of measurement and the modeling of dry deposition. Overall, this is a valuable paper but the authors should pay closer attention to error analysis. Additionally, the authors should not extrapolate from three months when air in Rondonia is relatively polluted to a full year. The comparisons to global models are inappropriate.

Specific Comments:

The analytical work follows precedents established in other work and appears in all aspects to be strong and appropriately described and defined. The inclusion of analytical precision estimates in Table 1 is very useful. I commend the authors for putting all of the analytically information in one convenient and informative table. Inclusion of references to the methods in the table would make this an even more valuable resource. The discussion of the models for inference of dry deposition flux is well done. Analysis of errors beyond the analytical errors is not as complete as might be hoped. The authors chose a to illustrate errors by selection of bounding cases with high and low flux estimates. This is commonly done where errors are difficult to define but I find the authors high and low bounds to be extremely conservative.

One example of conservative high and low estimates can be given for the case of NO_2 the most important species for dry deposition. The surface resistance for NO_2 is taken between 200 and 300 s m^{-1} based on Kirkman et al. 2002. Kirkman reported this resistance as having median daytime and nighttime values of 209 and 229 with interquartile ranges of 182 and 149 for day and night respectively. Even if we accept the interquartile range as an adequate definition of error, the surface resistance of NO_2 might easily range from only slightly above 100 to somewhat more than 300. Interquartile range is not a usual definition of error and it is not conservative. One might guess that given the loose treatment of errors, the estimates of dry deposition could easily vary by more than the factor of $\sim$2 given in the conclusion. Without a more detailed analysis, it is hard to know whether the error might not be closer to a factor of 10. The ranges are plausible but they are not carefully justified based on rigorous grounds for error analysis. The authors should attempt to be more conservative in their error analysis. They should justify the selection of ranges based on the distributions of concentrations and resistances used in their models and the probabilities of these ranges. Where distributions are irregular, boot-strapping analysis may be an option for estimation of probabilities.

There are errors that are barely quantified but deserved more attention. For example, the flux of ammonia from excreta appears to be an afterthought in this work. The single estimate for the flux of ammonia from excreta (8% of N) is based on a personal communication from L. Bouwman (Is this A. F. Bouwman as Lex is known in the scientific literature?). There is a considerable literature on ammonia volatilization that has been completely ignored. For example see the introductory paragraph from Frank and Zhang (1997) quoted below.

"Ammonia volatilization from ungulate urine can be a major pathway of nitrogen (N) loss in grazed grassland (Woodmansee et al., 1978; Schimel et al., 1986). Measured losses of ammonia-N mostly range 10%–40% of the urea-N added to plots; although amounts of 2–90% have been reported (e.g., Musa, 1968; Stewart, 1970; Denmead et al., 1974; Vallis et al., 1982; Bouwmeester et al., 1985; Schimel et al., 1986; Ruess and McNaughton, 1988). High variation in ammonia loss is a complex function of several interrelated factors that include soil texture (Schimel et al., 1986), organic matter (O'Toole et al., 1982), pH (Ernst and Massey, 1960), cation exchange-capacity (CEC; Campbell et al., 1984), soil micrometeorology (Sherlock and Goh, 1984), and vegetation (Ruess and McNaughton, 1988)."

Ammonia emission from excreta given the authors estimates would total 3 to 4 kg N ha^{-1} y^{-1}. This is as large as all estimated dry deposition. The net flux of ammonia depends on the correct estimation of the compensation point. This in turn requires a correct estimation of the ammonia flux from excreta. Might the loose treatment of the ammonia flux from excreta inject considerable uncertainty into the final estimate of total ammonia exchange?

The extrapolation from three months of measurement to an annual flux is not justifiable. The month of September is the smokiest month of the year and is not representative of

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the full dry season. October is a transition but it is still quite smokey and even November taken as representative of the wet season is not nearly as clean as other wet season months. Data taken from MODIS on Terra show monthly average optical depth for a 5 degree by 5 degree region that approximates the location of Rondonia state (Figure 1). These data were retrieved through NASA's Giovanni system <http://daac.gsfc.nasa.gov/techlab/giovanni/index.shtml>. Annual deposition flux may be substantially overestimated based on these three months where the air is quite dirty..

Even if the annual estimate were justifiable, the comparison of this area of Rondonia to regionally averaged values for the Amazon is not appropriate. The area studied in the center of the state of Rondonia is one of the most heavily perturbed areas in all of the Amazon. The high density of cattle pastures in this area, leads to a very high frequency of vegetation fires in the dry season making this one of the more polluted areas of the Amazon region. The authors write (p. 3162, line 1), "Not surprisingly, our N deposition estimate for the Amazon pasture site is a factor of two to eight higher than model predictions for the Amazon region (Fig. 13). Because this is not an especially useful comparison, Figure 13 should be eliminated from the paper. Furthermore, the conclusion that (p. 3163, line 8) "the contemporary net N deposition to tropical ecosystems may be underestimated by at least a factor of two" should be moderated. The tropical forest region should not be compared directly to the most perturbed section of the Brazilian Amazon.

Technical Comments:

The region in question is not rainforest but rather it is moist forest based on the commonly used Holdridge Life Zone system. The term "primary" forest is inappropriate. It may be more appropriate to refer to old growth forests. See Clark (1996) for a discussion of term related to tropical forests.

It is unnecessary to refer to "(micro)-meteorological data." These are simply meteorological data. (See p. 3146, line 17).

It would be useful to learn what portion of the data was excluded because of the turbulence considerations discussed on p. 3149. What bias may this have on the dry deposition estimates?

For Figure 3 it would be very useful to see a graph of D_r for the various species.

Figure 12 should be converted to a Table giving the absolute values of dry and wet deposition rather than simply percentages. Error estimates associated with the values would also be helpful. These should be given only for the study period and not as annual values as discussed above.

Figure 13 should be deleted because the comparison is inappropriate for reasons discussed above.

References

Clark, DB (1996) Abolishing virginity. Journal of Tropical Ecology 12: 735-739.

Douglas A. Frank & Yimin Zhang (1997) Ammonia volatilization from a seasonally and spatially variable grazed grassland: Yellowstone National Park. Biogeochemistry 36: 189–203.

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

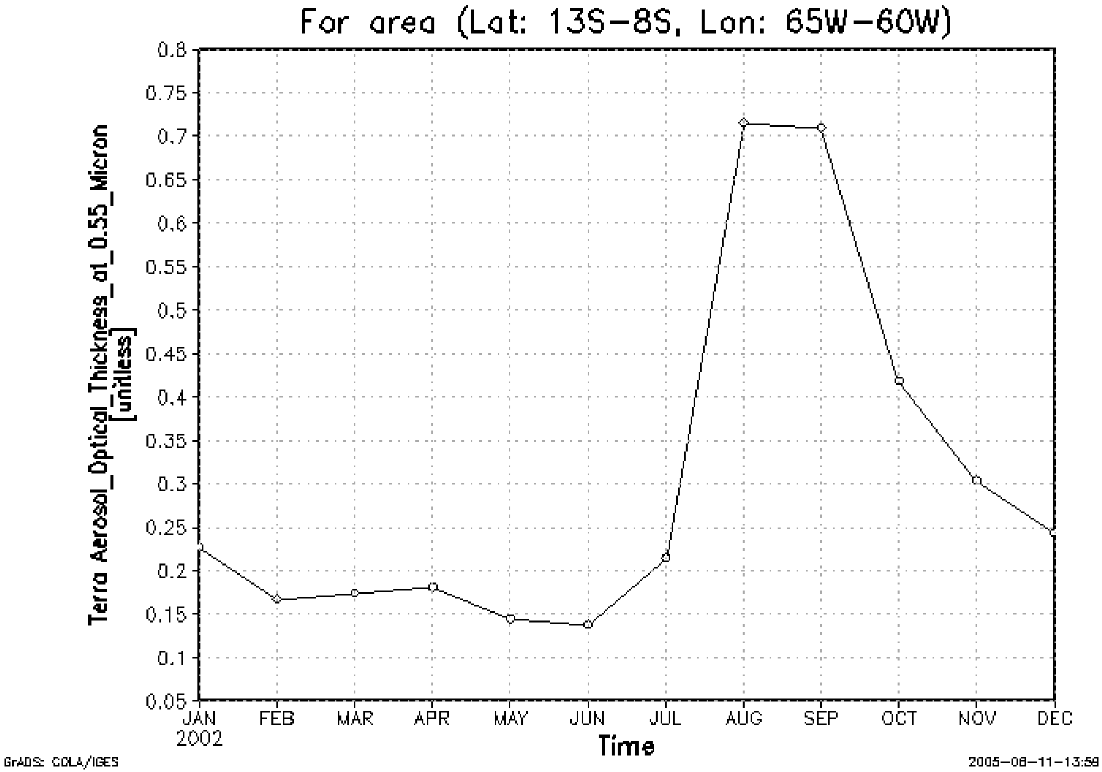

Figure 1: Review Figure 1.

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