Atmos. Chem. Phys. Discuss., 4, S229–S231, 2004
www.atmos-chem-phys.org/acpd/4/S229/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Real-time measurements of ammonia, acidic trace gases and water-solubleinorganic aerosol species at a rural site in the Amazon Basin" *by* I. Trebs et al.

Anonymous Referee #2

Received and published: 10 March 2004

General Comments

This paper reports on automated semi-continuous measurements of gases and particles in the Amazon Basin for a period spanning dry biomass burning to wet clean conditions. A significant fraction of the paper deals with inlet and instrument descriptions, sources of measurement uncertainty, and lower detection limits. The data analysis focuses mainly on seasonal and daily trends. Apparently a more detailed analysis will be carried out in a subsequent paper. The paper has scientific merit and presents novel data, however, I do believe that with minimal effort the analysis could be strengthened. For example, correlations (r2) between various gases and gases and particles could be presented in a table. Correlations are alluded to in the manuscript but not specifically

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

given. For example, during the biomass burning season correlations between various gases may suggest a similar biomass source (e.g., NH3, HCl, HNO3). Similarly, correlations between NH4+, and NO3- etc may help demonstrate a similar biomass source for these compounds. Another area of interest would be associations between particulate anions and cations. It is stated that NH4+ concentrations are greater than all other ions. This leads to the question of whether NH4+ is greater than SO4= plus NO3- (when concentrations are in equivalence). A plot of the ratio of NH4+/(SO4= + NO3-) with time along with other compounds (NH3, NO3- etc) may be insightful. These types of ion balances could provide some interesting insights, such as, could there be NH4+ associated with organic acids, a compound also thought to be emitted in biomass burning. Unfortunately, by only measuring the cation NH4+, this type of analysis may be limited.

For the most part the scientific methods are sound. However, there is an issue relating to the sampling efficiency calculations of section 3.2.2. The calculated HNO3 losses in the sampling inlet are highly questionable given that the Reynolds number is 4500 and Gormley & Kennedy is used to estimate the diffusional wall losses. This equation only applies for laminar flow, that is when Reynolds numbers are less than ˜2000. At the very least it should be pointed out that turbulence will enhance the losses, and thus considering this, the given HNO3 losses are a lower estimate. The best solution would be to calculate the losses under turbulent conditions to give a range in calculated losses (laminar versus turbulent).

The paper would also be strengthened if other data were also available and presented, including total particle mass and any other compounds that could serve as a biomass burning tracer.

Many of the equations presented are well known and could be excluded from the paper and only referenced. For example, equations 1, 2, 3, and 5. Just summarizing the results would likely be sufficient.

ACPD

4, S229–S231, 2004

It is unclear from the text if the aerosol measurements were of fine particles (PM2.5) or total (fine plus coarse). Apparently a cyclone was used at times, or was it all the time? This is unclear. The size of particles quantified should be presented with the aerosol data (e.g., the authors could include Şfine particleŤ in the appropriate figure captions).

Overall, the paper is well written and well organized and easy to read.

Technical Corrections

Pg 5, end of 1st paragraph Şare little sensitive to artifactsŤ do you mean Şnot prone to artifactsŤ?

Pg 5 end of section 2. After the discussion of various limitations/artifacts associated with the various measurement methods, a brief discussion or reference to any instrument intercomparison studies would be of interest.

Pg 7, was polyethylene actually one of the plastics tested by Neuman et al for HNO3 loss?

Last paragraph of section 3.2.2. on pg 9. Observing no changes in the diel variation doesnŠt really prove the accuracy of the measurement (a systematic error would not be observed). Did the concentrations not change significantly from inlet on to no inlet. Maybe the sampling time is too long for a comparison between consecutive measurements.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1203, 2004.