

## ***Interactive comment on “Deposition of nitrogen and phosphorus on the Baltic Sea: seasonal patterns and nitrogen isotope composition” by C. Rolff et al.***

### **Anonymous Referee #1**

Received and published: 29 September 2008

This manuscript reports the results of a ~2 year programme sampling rainfall at four sites in the western Baltic for nitrogen and phosphorus deposition and nitrogen isotopic composition. The study appears to have been thoroughly done, with appropriate care taken to control for potential contamination of rain samples collected over relatively long time periods. The work is well reported and the discussion of the results is generally clear and concise. In my opinion the manuscript is suitable for publication in Biogeosciences, although there are a few minor points which I think might need some clarification.

Since the abstract must stand alone, I would suggest including some definition of "Baltic

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Proper" there to help readers who might be unfamiliar with this term.

In section 2.2 I would suggest including a citation for the persulphate digestion method for total nitrogen and phosphorus analysis.

At the top of page 3020: "... because of draught.". I think this should be "... because of drought."

Also on page 3020: "Monthly precipitation (Fig. 2) ranged from zero at SH in August 2002... ", but this particular data point does not appear to be plotted in Fig. 2.

Bottom of page 3020: "Pollman et al. (2002) found replicate precision.... The same limit was adopted... " I am not quite sure whether this means that Rolff et al. have excluded all data  $> 80\mu\text{g L}^{-1}$ , or that they excluded data with low replicate precision (and, if so, how was this assessed?).

On page 3022-3 the regression of  $\delta^{15}\text{N}$  on percentage of DIN as ammonium is used to derive end-member isotopic signatures for nitrate and ammonium. I am not sure that the authors have done enough at this point in the manuscript to alert the reader that these "end-members" are likely to show some variability in their isotopic signatures. Later in the manuscript (page 3030) this extrapolation is stated to give the "long-term average isotopic composition of nitrate and ammonium", and I think this is probably correct and a better statement of the results of the extrapolation. Perhaps some citations to other works (either for nitrate and ammonium nitrogen isotopic composition in rainfall or aerosol) can be added to clarify this.

I am not entirely convinced by the authors' suggestion that the winter nitrate peak at the northern sites is caused by  $\text{NO}_x$  emissions from shipping.  $\text{NO}_x$  emissions are almost certainly the source, but why should the source be shipping? If there is some evidence that shipping, and not other sources (such as higher terrestrial combustion emissions during the winter, or seasonal changes in atmospheric transport pathways), is the dominant source then this evidence should be presented. Otherwise this looks

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

rather speculative.

In Table 1 most of the Average data for nitrogen species appears to have been shifted one column to the right.

---

Interactive comment on Biogeosciences Discuss., 5, 3013, 2008.

**BGD**

5, S1822–S1824, 2008

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S1824

