

Interactive comment on “Nitrous oxide water column distribution during the transition from anoxic to oxic conditions in the Baltic Sea” by S. Walter et al.

G. Uher (Referee)

guenther.uher@ncl.ac.uk

Received and published: 15 August 2006

General comments:

This manuscript presents data on the distribution of nitrous oxide following the renewal of bottom water via intrusion of oxygenated North Sea water into the south-western basins of the Baltic Sea. Thanks to its very fortunate timing, this unique study covers a variety of redox conditions from oxygenated to ‘old’, stagnant bottom waters along the cruise track, ideal for a discussion of the effects of natural variability on N₂O distribution in these coastal waters. N₂O data are complemented by a set of ancillary data including DIN species, oxygen and other water mass characteristic. Therefore, this

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

comprehensive data set is ideal to support the detailed discussion of N₂O distribution within the context of hydrography and N cycling, and clearly warrants publication in *Biogeosciences*.

Both N₂O and ancillary data are well presented, discussed in considerable detail and give a clear picture of the observed N₂O distribution in relation to North Sea water intrusion. Where the discussion addresses the conundrum of the main N₂O source (presumably nitrification), the discussion is necessarily more speculative, chiefly due to the absence of N₂O cycling rates. Still, I believe the authors make the best of the available data set. It would be tempting to suggest a much more detailed discussion of the temporal development of N₂O following the salt water intrusion. Such a discussion, however, would require very detailed observations / modelling of the hydrographic event together with knowledge of microbial cycling rates, which the data set does not provide. This leaves me with only one issue, namely the somewhat unclear description of 'N₂O production resulting from the salt water intrusion'. In section 3.3 ff. this production is deduced from a comparison of inventories. However, no clear descriptions of source and sink processes are given, nor is it made clear that the result must be considered 'net production' rather than gross nitrification. An at least qualitative discussion of relevant N₂O sources and sinks here would enhance the transparency of the manuscript.

Further specific and editorial comments are listed below.

Specific Comments:

p. 732, line 26: perhaps 'salinity-induced' of thermohaline would be more fitting.

p. 734, lines 3-4: please clarify that this sentence relates to bottom waters only.

Nitrification rate calculations, p 736, Table 2 and discussion: the formula given in Methods contains a typo: it requires division by fractional N₂O yield r (not multiplication!). I also find the conversion factor in the denominator ($1\text{E}-9$, mol \leftrightarrow nmol) somewhat con-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

fusing. Likewise, you could justify the inclusion of further conversion factors for tons <> g and for cubic km <> L? I recommend that the factor 1E-9 should be removed, and also that fractional N₂O yield r is reported as 0.003 and not as percentage value. In table 2, the data for the E Gotland basin are correct, but not those for the Bornholm basin, where the actual data in table 2 give a rate of 0.062 nM/d rather than the 0.059 nM/d in the manuscript. This is obviously a minor point but should be clarified before publication.

Results section 3.2 ff and figures. Could you please add a note to figure captions that 'negative' oxygen represents H₂S?

P 738, section 3.2.1. Fig 4a does show a weak N₂O max that coincides with the oxygen minimum, i.e. the statement that oxygen has 'no clear influence on N₂O' is slightly misleading.

p.739, line 7. 'completely oxygenated' should be amended, these waters are far from 100 percent saturation.

p. 740, section 3.3. In the main, this section and Table 1 give N₂O inventories and don't attempt to present a closed budget in terms of N₂O sources and sinks. Perhaps a title similar to 'Estimated N₂O inventories before and after NSW inflow' would be more appropriate. I would like to suggest that this section (together w. corresponding text in Discussion) should be recast. First describe inventories, then discuss possible effects of advection and other processes, before moving on to a 'nitrification estimate'.

P 741, section 4.1, please change 'non-biological' to 'hydrographic'.

Conclusions re advection: rather, you found no excess-N₂O resulting from advection, presumably because NSW was close to equilibrium with overlying air. An improved discussion of before/after inventories and relevant sources/sinks would illustrate this much better than the existing text.

Stats in figure captions: please give sample number ($n = ?$) together with coefficients

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

of determination.

Interactive comment on Biogeosciences Discuss., 3, 729, 2006.

BGD

3, S364–S367, 2006

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S367

EGU