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Interactive Comment

Interactive comment on "Hydroxylamine (NH_2OH) in the Baltic Sea" *by* S. Gebhardt et al.

Anonymous Referee #2

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General comments:

This manuscript reports hydroxylamine (HDA) and nitrous oxide concentrations from various stations along a cruise track through the south-western and central the Baltic Sea. The focus of the MS is on the distribution of HDA. This is most welcome, because the distribution and cycling of HDA in the marine environment is still poorly documented. Possible inter-relations between nitrous oxide and HDA are also explored, adding further relevance to this manuscript. Given the scarcity of original data on HDA and the important roles of HDA in nitrogen redox transformations, this manuscript is clearly relevant to the wider remit of this journal. The manuscript presents high quality data, based on sound methodology that is described in some detail on pages 712-714. I only have a brief comment regarding HDA determination. While HDA conversion yields are somewhat low in comparison with previous work, this has been rigorously tested, and the use of such data is therefore quite legitimate in my view. The authors, however, comment that HDA conversion may vary with sample composition. Given that

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Interactive Discussion

Discussion Paper

their cruise track covered contrasting waters within the Baltic Sea, a short note by the authors, commenting on possible implications for HDA yield would be much appreciated. One of the strengths of this manuscript is that it reports data from a wide range of conditions along a cruise track from the south-western to the north-eastern Baltic, and thus provides opportunities to study pronounced differences in dissolved oxygen levels, riverine inputs and North Sea water inflow. These pronounced differences are most apparent in the depth profiles in Figure 3, particularly with regard to the effects of North Sea water inflow on the distribution of deep water anoxia. However, the discussion remains very brief (i.e. shorter than 'methods'), does not fully address the interplay between hydrography and HDA distribution, and therefore does not always do justice to the high quality data in this report. For example, salinity data are not reported, even though they are referred to on p 715, and vertical & horizontal nutrient distributions are not discussed. The report also emphasises the contrasting behaviour of "shallow, well-mixed stations" in the south-western Baltic (see text p 715 and Figure 4), but does not include these essential data in Figure 3. In my view, this manuscript could be strengthened considerably by a more comprehensive discussion and by inclusion of the above mentioned station data in Figure 3. Further specific comments are provided below. However, given both relevance and quality of the data reported here, I recommend publication in Biogeosciences following careful revision of the manuscript in the light of these comments.

Specific Comments:

Water column distribution, p. 715: Please include at least one example depth profile for "shallow, well-mixed stations". The present discussion and Figure 3 also don't clarify, where (surface or depth?) the highest HDA concentrations occurred. The depth profiles in Figure 3 HDA show surface concentrations close to 40 nM at all stations, suggestive of a rather homogeneous horizontal distribution. However, the authors' mean value for the mixed layer concentration in the central Baltic is 26 nM, i.e. significantly lower. I would appreciate if the authors could describe the surface distribution

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of HDA, comment on the range of HDA levels at the surface, and report the depth, at which the maximum of 179 nM had been found, in order to provide a clearer picture of HDA distribution in the Baltic. The authors use temperature difference to define mixed layer depth. I wonder if density criteria would be the better choice for the Baltic Sea, where strong salinity differences between surface layer and bottom waters are well documented. Inclusion of salinity or density profiles in Figure 3 might also help illustrating the distribution of saline North Sea water inflow across Baltic Sea basins. The authors comment that enhanced HDA levels were associated with high salinities, but unfortunately this statement is not supported by ancillary salinity data. Further, they state that "obviously there was no correlation of HDA with N2O or O2". This may be an oversimplification of their own findings, as depth profiles clearly document lowest HDA in anoxic, N2O depleted waters (Figure 3 c + d), and elevated HDA and N2O levels in waters with reduced oxygen concentrations (Figure 3 b + c). These results should be described/discussed in more detail.

Formation, p. 716 (HDA vs N nutrients): This section could build on and benefit from a more detailed discussion of HDA distribution in relation to water masses and nitrate/nitrite (see also comments above).

I have one more minor comment regarding the authors' statement on possible HDA inputs from the North Sea. Inflow of North Sea water is episodic, with the last major event dating back to January 2003 (see authors' introduction, p. 711) i.e. 13 months before the authors' research cruise. I am not aware of published turnover times for HDA, but given what we know about nitrogen cycling and nitrification, HDA turnover times should be much less than 13 months. Therefore, I would like to suggest that inflow from the North Sea can be excluded as an HDA source.

Interactive comment on Biogeosciences Discussions, 1, 709, 2004.