

Interactive comment on “A new model for the global biogeochemical cycle of carbonyl sulfide – Part 1: Assessment of direct marine emissions with an oceanic general circulation and biogeochemistry model” by T. Launois et al.

Anonymous Referee #2

Received and published: 8 September 2014

GENERAL COMMENTS

This study presents a new estimate of direct marine emissions of carbonyl sulfide (OCS) calculated using parameterizations implemented in the NEMO-PISCES global ocean biogeochemistry model. The analysis aims to address the recently highlighted discrepancy in the global OCS budget between the estimates of identified sources and sinks to the atmosphere. Recent studies have suggested that ocean sources may need to be revised upwards to balance increases in the estimates of OCS uptake by

C6648

terrestrial vegetation and soils.

The analysis is generally well-founded, and appropriate sensitivity analyses have been conducted on some of the key parameters underlying the biogeochemical production and loss parameterizations, in order to provide a range of uncertainty for the ocean emissions. In this study, several of the OCS production and loss pathways implemented in the NEMO-PISCES model are based on previously derived parameterizations (e.g., as outlined by von Hobe et al. 2001, 2003). A new contribution of this work is the use of a global ocean biogeochemistry model to provide the organic matter cycling fluxes underlying the derivation of the OCS production and loss terms, in deriving a revised estimate of global ocean direct emissions of OCS.

This study is a useful addition to the field, and addresses an open question on the magnitude and nature of oceanic OCS emissions. I suggest the authors address the concerns outlined below before publication.

Specific areas of concern as outlined below in ‘Specific Comments’ include :

- (a) the lack of a more comprehensive and detailed evaluation of the modeled oceanic OCS concentrations (e.g., for their preferred ‘standard model run’ of section 4), as has been done in previous estimates of this type;
- (b) lack of clarity in some sections on model development, validation of the individual components, and discussion. These could be improved by a clearer discussion and more detail on the underlying assumptions and methods.

SPECIFIC COMMENTS

1) Evaluation of modelled OCS : A more systematic evaluation of modelled OCS concentrations against available ocean measurements would improve the manuscript. A significant concern I have is that the current validation of modelled OCS is minimal, and limited to a few sentences in the text comparing ocean model results to summary values from a few previous measurement campaigns. I recognize that ocean OCS

C6649

measurements are sparse, however for a model development exercise as presented here, it is important to present as comprehensive a validation as possible. It would have been good to have seen a more detailed evaluation using a larger database of the available ocean OCS measurements (in figure or table form, as was presented, for example, by previous studies such as von Hobe et al. 2003). This would be especially useful to assess the validity of the 'standard model run' of section 4, which incorporates their chosen 'best-guess' parameterizations. In the current manuscript version, for example, modeled OCS concentrations are shown in Figures 5 and 9, but no observed values are shown as points of comparison. Without such an evaluation of OCS against observations, it is difficult to assess the validity of the proposed total model for oceanic OCS that is presented here (i.e., the combined effect of parameterizations for production, hydrolysis and air-sea exchange components on modeled surface ocean OCS levels, and hence on ocean emissions).

2) Clearer more detailed discussion : Certain aspects of the description of model components and parameterizations in section 2 would also benefit from a clearer discussion and additional detail to clarify the methods used. For example, more detail is needed on the following :

a) The analysis underlying the normalization of the Apparent Quantum Yield of OCS production outlined in section 2.2.3, as this is a key quantity in the derivation of OCS production.

b) The implementation of the dark matter production pathway in conjunction with NEMO-PISCES; in particular, lines 1-12 of page 20687 (section 2.2.4) need more explanation.

c) Section 2 : A clearer identification is needed of (i) the specific parameterizations relating to modification of NEMO-PISCES, vs. (ii) independent parameterizations of OCS production and loss pathways. Reading through section 2, it is not always clear which parameterizations relate to NEMO-PISCES modules, and which are independent esti-

C6650

mates derived from other (e.g., remote sensing) data.

3) Indirect ocean sources of OCS : This study presents revised estimates of direct ocean emissions of OCS. The study of Kettle et al. 2002 also suggested there were large indirect fluxes of OCS associated with ocean emissions of CS₂ and DMS. These indirect sources were previously estimated to be significantly larger than the direct ocean source of OCS. Since the authors now present revised estimates of the ocean direct source, does their analysis suggest any new constraints on the indirect oceanic sources, within the overall limits of the global budget ? The authors should provide some discussion of this, if possible.

4) Figure of OCS production/loss pathways : Figure 1 should be improved to provide a clearer schematic of the linkages between upper ocean processes and OCS production and loss pathways. The legibility of the figure should also be improved (e.g., by being converted to a B/W schematic, rather than a grey-scale or color figure). Some of the figure's text, especially towards the bottom, is hard to read.

5) Introduction : p. 20679, Lines 11-20 : Please improve the discussion of the vegetation and soil uptake of OCS with a more detailed quantification of the fluxes involved, and more relevant references for the soil fluxes (e.g., see Van Diest and Kesselmeier, 2008, and references therein).

6) Introduction : p. 20679, Lines 23-25 : There are earlier global budgets of OCS than Kettle et al. 2002 (e.g., in Chin and Davis, 1993), therefore this cannot be the 'initial global budget'. Please reword.

TECHNICAL CORRECTIONS

Abstract

p. 20678 : Line 4 : Change to 'uptake'

Line 12 : 'using the UV absorption..' (add 'the')

C6651

Line 21 : change to 'uptake'

Introduction

Pg. 20679 : line 3 : Grammar issues, so reword sentence; e.g., '...it is a major contributor to the stratospheric...'

Pg 20680 : Line 4 : Missing reference : 'Kettle et al. 2002'?

Pg 20681 : Line 7 : Change to 'results'

Pg. 20683 : line 16 : Change to 'ranging'

Pg. 20683 : line 23 : Change to 'impacts'

Pg. 20683 : lines 24-25 : Grammar issues : change to '.. has been identified as one of the most influential factors...'

Pg 20684 : lines 5 and 7 : Use of the word 'primordial' is not clear. Do you mean 'fundamental' or 'necessary' ?

Pg. 20684 : line 23 : Change to 'The deduced...'

Pg. 20684 : line 24 : '..has been established by remote sensing...': Please provide more detail on the remote sensing sources.

Pg. 20687 : lines 1-3 : Please make clearer the sentence beginning 'Therefore the formulation...'

Pg. 20691 : lines 24 : Change to 'translated to a ..'

Pg. 20691 : lines 25 : Reword : change 'quick' to 'abrupt' or 'sharp'

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 20677, 2014.

C6652