Atmos. Chem. Phys. Discuss., 12, C11116–C11118, 2012
www.atmos-chem-phys-discuss.net/12/C11116/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dimethylsulphide (DMS) emissions from the West Pacific Ocean: a potential marine source for the stratospheric sulphur layer" *by* C. A. Marandino et al.

Anonymous Referee #1

Received and published: 28 December 2012

The paper addresses the question of the possible contribution of biogenic DMS emission to the formation of stratospheric aerosols as suggested by Crutzen in 1976. From simultaneous measurements of oceanic and atmospheric DMS during a research vessel cruise from Japan to Australia in October 2009, the authors are showing that DMS emissions are reinforced up to 200 ppt during storms high wind episodes. The same DMS concentration reported occasionally by the HIPPO2 aircraft at 8 km altitude is very consistent with a fast uplift from the PBL by meso-scale convective systems (although poorly captured by the ERA-Interim global model). The vertical and horizontal transport of the DMS rich layer is further explored using the FLEXPART Lagrangian model with ERA-Interim winds, suggesting that large amounts of sulphur could be transported

up to 17 km altitude in this region and thus that DMS might be an important source of sulphur for the stratospheric aerosols.

General Comments The analysis of ship and aircraft DMS measurements and the demonstration of the reinforced emissions in high wind conditions is a nice piece of work which by no doubt will deserve publication. But the conclusion that oceanic DMS emissions could be an important source of stratospheric aerosol is far less convincing. There are several reasons for that: a) The 17 km altitude over the West Pacific during the boreal winter is in the upper troposphere and not in the lower stratosphere. It is below the cold point tropopause. b) the ERA-Interim vertical wind in the Tropical Troposphere Layer (TTL) derived from the horizontal wind divergence is a very poor measure of vertical velocity. It leads to a strong overestimation of the DMS flux reaching 17 km. As shown for example by Corti et al (GRL 2005), Yang et al. JGR (2008) or Fueglistaler (2009), the vertical transport above the neutral buoyancy layer (NBL) around 14 km altitude is due (at least above oceanic areas) to the radiative heating of the air-masses, a very slow process of 0.4 mm/s (1 km/month) which do not permit very short lived species like DMS to reach the stratosphere. c) As shown by Vernier et al. (GRL 2011) the stratospheric aerosol increase since 2003 reported by Hofmann (2009) is not resulting from anthropogenic sulphuric pollution from China as suggested by Hofmann but to a series of relatively small volcanic eruptions. d) In the absence of volcanic eruption between 1996-2003, the background aerosol concentration is reducing considerably (Vernier et al. 2011 Fig. 1) showing that DMS and anthropogenic sources of sulphur are very small compared to volcanoes, and finally, e) There is no signature of aerosol increase during the DMS boreal winter in the tropics but exactly the opposite: a cleansing of the aerosols by injection of clean air in the lower stratosphere (mainly above continental regions) (Vernier et al. ACP 2011). In summary, it cannot be concluded from FLEXPART/ERA-Interim simulations that DMS could penetrate the stratosphere and that sulphur from DMS could represent a significant contribution to stratospheric aerosols. However, providing the conclusions are revised, the analysis of the DMS ship and aircraft measurements showing that high DMS levels can be reached in the mid-troposphere near convective systems is very valuable and deserves publication. I fully understand that the authors might be disappointed by the above comments. But the conclusion that DMS cannot reach the stratosphere will be also very valuable and useful. This already happened with another oceanic VSLS, Iodine oxide IO, which was suggested by S. Solomon et al. (JGR 1994) to be responsible for ozone depletion in the lower tropical stratosphere, but shown later to be of insignificant concentration there (Pundt et al. JAC, 1998).

Recommendations My recommendation is to publish the paper but after deep revision of the discussion and the conclusions by taking into account the above comments. If meso-scale model simulations, which could reproduce the DMS entrainment up to the altitude of the HIPPO2 aircraft, are not available which is fully understandable, I would recommend to add a discussion on the limitation of the FLEXPART/ERA-Interim simulations to reproduce the convective lifting of PBL air-masses in the mid-troposphere, particularly above the neutral buoyancy level, and add some comments on the very slow ascent velocity above the zero radiative heating level. Otherwise, the paper is nicely written and technically fully acceptable.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 30543, 2012.