We thank the reviewer#1 for the very constructive and detailed review that helps to clarify and strengthen our argumentation. In the following, we address the raised points directly, with the *review in italics* and **our reply in bold** font.

*Overview:*
*The authors present new bottom-up measurements and analysis of COS and CS2 from 3 ocean cruises. The ocean source is a dominant source of uncertainty in global COS budgets so the authors should be commended for presenting new, high quality data. However, the central conclusion in the manuscript title and text is not supported by the measurements. Nevertheless, the measurements and analysis provide a very important contribution to understanding COS budgets and I suggest only simple, but critical, revisions to the title and text.*

**We will address the conclusions drawn in the specific comment below, but changed the title to "Direct oceanic emissions are unlikely to account for the missing source", because our observations, previous observations and the box model reproducing both reasonably well reveal a direct emission estimate that is a factor of 3-8 below the missing source estimate and thus very unlikely. We still deem indirect emissions as unlikely to account for the whole missing source, but acknowledge the uncertainty related to these emission estimates by changing the title.**

*Specific Comments:*

*The title and several statements in the text should be changed so that the conclusions become consistent with the data. In particular, the measurements are not a representative sample for extrapolating to the global source and thus conclusions on the global source should not be made. There are of course many other exciting conclusions that are possible. The measurements are not representative of the global source for following reason. Global satellite observations show global hot spot for the source in the Pacific Warm Pool for most of the year and in June/July/Aug more broadly across the tropical and mid-latitude Pacific. The cruise measurements from this study that are used in the global extrapolation do not cover this critical region. If the author's were looking to uncover information on the missing source they should target locations/times that top-down data points to for the missing source. However, the cruise data presented here are in times and locations were the top-down data suggest that the ocean source should be small or even a sink. I still think the global analysis is useful to include because it is already done and likely points to the problem with scaling up from non-representative data.*

**Obviously, the use of the word "extrapolation" in the context of relating our new observations to the global source estimate has been misleading. To estimate tropical and global oceanic OCS emissions, we use a model that is much simplified with respect to mixed layer dynamics and vertical mixing, but contains state-of-the-art parameterizations of the known processes governing OCS concentrations in seawater. To our knowledge, these process parameterizations have not been seriously challenged and have always yielded good results in studies comparing observed and simulated OCS for individual cruises from different parts of the oceans. In our paper, we present the first extensive study of this type focused on the tropical ocean, and find that here, too, the established model reproduces observed OCS quite well. The tropical observations are used to fine-tune the model, in particular corroborating the relationship of photochemical production on $a_{CDOM}$. The model is then used in a similar way as has been done by Kettle (2002) to estimate fluxes from all regions of the global ocean and integrate the results to yield a global flux estimate.**

**To clarify this approach and its benefits, we apply the following changes in a revised manuscript:**

1. **We add a more thorough description of the development and history of the box model and then point out how the new observations are used to fine-tune the model and to enlarge the range of biogeochemical regimes, for which the model is tested.**

**p. 6, l. 23ff: "A box model to simulate surface concentration of OCS is further developed from the latest version from von Hobe et al. (2003, termed vH2003), where concentrations along the cruistracks of 5 Atlantic cruises have been simulated and compared. The vH2003 model results from successful tests and validation to observations on several cruises to the Altantic Ocean covering all seasons (i.e. Flöck and Andreae (1996) in January 1994, Uher and Andreae (1997) in April/May 1992, Von Hobe et al. (1999) in June/July 1997, Kettle et al. (2001) in September/October 1998). By comparing photoproduction rate constants of the 5 cruises to CDOM absorption, von Hobe (2003) suggests a second order process for photoproduction with the photoproduction rate constant being dependent on the absorption of CDOM in seawater.**

**In our approach, we test vH2003 along the cruise track of two cruises, include a new way of determining the photoproduction rate constant (see below) and apply it with global climatological input (termed L2016). (Kettle, 2000; Kettle, 2002) applied a similar version of vH2003 globally, which included an optimized photoproduction constant from Atlantic transect cruise data, an optimized constant light-independant production and a linear regression to obtain CDOM from chlorophyll a. In comparison to K2000, we use (i) a new way of determining the photoproduction rate constant incorporating information from three ocean basins, (ii) the most recent parameterization of light-independent production available, and (iii) satellite observations for sea surface CDOM instead of an empiric relationship based on chlorophyll a.**

**Launois et al. (2015) implemented parameterizations for light-independant production, hydrolysis and air-sea exchange similar to vH2003 in the 3D global ocean model NEMO-PISCES. The main differences to the approach used here is the lack of accounting for mixing in L2016 (discussed in section 3.2.2., which will theoretically lead to higher simulated concentrations in our case) and the application of a photoproduction rate constant in our model that incorporates information from three open ocean basins in contrast to one from a study in the North Sea (Launois et al., 2015)."**

2. **In a new table (Tab. 4), we compare the physico-chemical conditions encountered during our OASIS and ASTRA-OMZ to those of the Pacific Warm Pool where the atmospheric inversion studies suggest the hotspot of oceanic OCS emissions, demonstrating that the conditions in the Indian Ocean and the Pacific Warm Pool are similar and the known processes should thus yield similar OCS concentrations and direct fluxes.**

| Parameter | OASIS | ASTRA-OMZ | Pacific Warm Pool |
|---|---|---|---|
| SST [°C] | 27.0±1.0 | 19.6±2.6 | 28.9±0.9 |
| SSS [g $kg^{-1}$] | 35.0±0.3 | 35.1±0.3 | 34.5±0.42 |
| wind speed [m $s^{-1}$] | 8.2±1.7 | 7.5±1.8 | 5.3±0.4 |
| $a_{350}$ [$m^{-1}$] | 0.039±0.02 | 0.146±0.02 | 0.050±0.08 |
| I [W $m^{-2}$] | 226.5±303.0 | 196.4±283.1 | 206.4±286.6* |
| SR [J $m^{-2}$] | 1.9 $10^7$±1.7 $10^6$ | 1.6 $10^7$±4.5 $10^6$ | 8.9 $10^6$±1.3 $10^6$ |
| pH [-] | 8.03±0.01 | -** | 8.07±0.01 |
| MLD [m] | 43.3±15.8 | 18.9±7.5 | 35.9±14.1 |

3. **In another table (Tab. 5), we compare the results of our global box model simulations to previously observed concentrations and fluxes.**

| References | Season | Region | Mean OCS±std. [pmol $L^{-1}$] | n | L2016 mean [pmol $L^{-1}$] |
|---|---|---|---|---|---|
| Mihalopoulos et al. (1992) | | open Indian Ocean 20°N-37°S | | | |
| | Mar/May 1986 | OCEAT II | 19.9±0.5* | 20 | 11.2±6.3 |
| | Jul 1987 | OCEAT III | 19.9±1.0* | 14 | 17.7±13.1 |
| Staubes and Georgii (1993) | Nov-Dec 1990 | Wedell Sea 40°-72°S,72°W-24°E | 109** | 126 | 66.6±49.8 |
| Ulshöfer et al. (1995) | | North Atlantic Ocean | | | |
| | Apr/May 1992 | 47°N 20°W | 14.9±6.9 | 118 | 42.8±11.3 |
| | Jan 1994 | 48-50°N, 10-17°W | 5.3±1.6 | 120 | 8.9±3.2 |
| | Sep 1994 | 48-50°N, 10-17°W | 19.0±8.3 | 23 5 | 33.4±3.5 |
| Flöck and Andreae (1996) | Jan 1994 | Northeast Atlantic Ocean 49°N, 12°W | 6.7 (4-11) | 120 | 9.6±3.7 |
| Ulshöfer and Andreae (1998) | Mar 1995 | West Atlantic 32°N, 64°W | 8.1±7.0 | 323 | 15.8 |
| von Hobe et al. (1999) | Jun/Jul 1997 | Northeast Atlantic Ocean 30-40°N, 8-15°W | 23.6±16.0 | 940 | 30.5±12.6 |
| Kettle et al. (2001) | Sep/Oct 1998 | Atlantic transect 50°N-60°S, 1°-64°W | 21.7±19.1 | 783 | 22.9±3.2 |
| von Hobe et al. (2001) | Aug 1999 | Sargasso Sea/BATS 32°N, 64°W | 8.6±2.8 | 518 | 8.1 |
| Xu et al. (2001) | Oct/Nov 1997 | Atlantic meridional transect 53°N-34°S,25°W-20°E | 14.8±11.4 | 306 | 11.8±12.7 |
| | May/Jun 1998 | Atlantic meridional transect 53°N-34°S,25°W-20°E | 18.1±16.1 | 440 | 27.8±47.9 |

**To discuss this table and compare to previous shipboard measurements, we added a new section "3.2.1. Comparison to previous shipbased measurements" p. 11, l. 18:**

**"The global simulation of OCS surface water concentrations generally reproduced the lower picomolar range of concentrations (Tab. 5), the seasonal pattern of higher concentrations during summer compared to winter (as e.g. in (Ulshöfer et al., 1995)) and the spatial pattern of higher concentrations in higher latitudes (e.g. Southern Ocean, (Staubes and Geogrii, 1993)). Given that monthly means of a model simulation driven by climatological data of the input parameters is compared to cruise measurements, the absolute mean error of 6.9 pmol $L^{-1}$ and the mean error of 3.7 pmol $L^{-1}$ indicate an overall good reproduction of observations (differences between observation and model output were weighted to number of observations in Tab. 5). It has to be noted that on average, the model overestimates OCS concentrations as indicated by the positive mean error, suggesting our emission estimate to be an upper limit to direct oceanic OCS emissions in most regions. Largest deviation from observations are found in the Southern Ocean (vgl. (Staubes and Geogrii, 1993) in Tab. 5), where the model underestimared observations by ~40%. While this can have several reasons, i.e. a possible violation of the underlying assumption of a constant OCS production in regions with deep mixed layers such as the Southern Ocean, or the missing satellite data for CDOM during polar nights, it is a clear indication of the need of more observations from high latitudes. However, this underestimation does not infer with our conclusion drawn for the tropical oceans, where the location of the missing source is derived from top-down approaches."**

**We hope that with these modifications and additions, it becomes clearer how our line of arguments leads to the conclusions drawn.**

*Robust conclusions for this study could instead focus on describing the ocean source for the times/locations of the three cruises shown in Figure 1. A qualitative comparison could also be made with previous top-down analysis. This seems to be good ground for an exciting conclusion of consistency between top-down and bottom-up estimates. In this case there appear to be some strong similarities between the bottom -up and top-down estimates. The TranPEGASO cruise covers a section of the Atlantic in Oct/Nov, showing a small source. This is qualitatively consistent with the MIPAS data along the same path in Sept/Oct/Nov. The Oasis cruise covers a small area in the central Indian Ocean in July/Aug showing a sink. This is roughly consistent with a MIPAS Jun/Jul/Aug map and a TES June map that show this region to be on the border between a source and sink.*

**The suggested qualitative comparison of data from a single cruise for OCS is difficult, because the satellite data and atmospheric inversions do not differentiate between direct and indirect emissions. We use our measurements to increase process understanding on a broader scale and use this to address the question of sources and sinks combining direct and indirect sources. As already stated in the text, it is impossible to conclude whether or not the ocean was a net source or sink for direct OCS from TransPEGASO, as only 2 measurements per day were available.**

*ASTRA-OMZ show a strong source in October for the Peru-Chile upwelling region. MIPAS Sept/Oct/Nov do not show this. However, MIPAS is an upper troposphere estimate so it is not expected to provide a close relationship to surface fluxes in regions without strong atmospheric convection such as the Peru-Chile upwelling region. TES provides a lower altitude sensitivity and could provide a better top-down on small regions of sources such as the Peru-Chile upwelling regions. While TES data have only been published for June, TES retrievals for other months are in preparation.*

**As the reviewer correctly mentions, TES would be a better comparison to our combined flux maps, but is unfortunately not yet available. Similarly to our comment above, indirect fluxes contribute significantly to the atmospheric budget and can currently not be differenciated by satellites.**

*Several revisions are needed in the introduction. Page 2 indicates that top-down studies were not consistent with the Kettle bottom-up estimates for sources and sinks. This should be corrected to say that the bottom-up and top-down info does not agree with Kettle. Kettle was a misinterpretation of the bottom-up information from plant studies which was first pointed out in the bottom-up study of Sandoval-Soto et al. and then later confirmed by multiple topdown studies (Campbell et al, Sunthralingam et al, Berry et al, etc.) and other bottom-up studies using chamber (Stimler et al) and canopy (Asaf et al, Maseyk et al) approaches.*

**We thank the reviewer for pointing us to these studies and added/adapted the following lines to the manuscript p. 2, l. 21ff "Nonetheless, current figures for tropospheric OCS sources and sinks carry large uncertainties (Kremser et al., 2016). While the budget has been previously considered closed (Kettle, 2002), a recent upward revision of the vegetation sink (Sandoval-Soto et al., 2005; Suntharalingam et al., 2008; Berry et al., 2013) led to a gap, i.e. a missing source, in the atmospheric budget of 230-800 Gg S per year (Suntharalingam et al., 2008; Berry et al., 2013; Kuai et al., 2015; Glatthor et al., 2015)(Tab. 1). , with the most recent estimates at the higher end of the range. This revision of vegetation uptake was suggested as to (i) take into account the different deposition velocities of $CO_2$ and OCS within the leaf and base it on GPP instead of net primary production (Sandoval-Soto et al., 2005) as well as (ii) to better reproduce observed seasonality of**

**OCS mixing ratios in several atmospheric models (Berry et al., 2013; Kuai et al., 2015; Glatthor et al., 2015).”**

*The top down evidence from the global scale should be better specified. First it should be clear that there are four independent lines of five independent lines of evidence that point to a tropical source: MIPAS satellite (Glatthor et al), TES satellite (Kuai et al), FTIR (Wang et al, ACP, 2016... this ref isn't in the manuscript but might be added), NOAA and HIPPO observations (Berry et al, Kuai et al, Suntharlingam et al).*

**We agree that we did not fully address all of the mentioned studies. However, only a latitudinal gradient on mixing ratios alone does not point to a tropical hotspot source (i.e. it could also be stronger high-latitude sinks) or an ocean source (i.e. other sources such as anthropogenic sources are possible). We wanted to highlight the magnitude of the missing source suggested by the inverse modelling studies in this section. The fact that highest atmospheric volume mixing ratios are found in the tropical atmosphere does not *per se* contradict our bottom-up emission estimate, as the oceanic emission is still confirmed as one of the dominant sources to the global budget in our study. We thus adjusted the following sentence, including the suggestions from the reviewer p. 2, l. 28: “Based on independent top-down approaches using the MIPAS (Glatthor et al., 2015) and TES (Kuai et al., 2015) satellite observations, FTIR measurements (Wang et al., 2016) as well as NOAA ground based time series stations and the HIPPO aircraft campaign (Berry et al., 2013; Kuai et al., 2015) together with inverse modelling, the missing source of OCS was suggested to originate from the (tropical) ocean.”**

*A critical point should be raised to alert the reader to alternative explanations for the top-down trends. In particular, the MIPAS remote sensing data is the upper troposphere (10km) and transport from Asia to the upper troposphere in the deep tropics (e.g. Ashfold et al ACP 2015).*

**We included this point in our manuscript by adding p.2, l. 32: “Other potential sources like e.g. advection from air masses from Asia have been discussed (Glatthor et al., 2015) but not tested.”.**

*Recent anthropogenic emission estimates for Asia are not yet sufficient to explain the missing source but they are based on very little bottom-up data from Asia (Campbell et al 2015). Other hypotheses could be mentioned as well such as a soil source which has been shown in a recent survey of global soils but not particularly large in the tropics (Whelan et al ACP 2016). Biomass burning is another but the most recent review of emission factors shows a relatively small source (Campbell et al 2015).*

**We added a sentence on the potential of biomass burning as the missing source p.14, l.10: “While biomass burning is known to emit OCS and is present close to the assumed source region, e.g. around Indonesia, the most recent review of emission factors result in a source too small to close the atmospheric budget (Campbell et al., 2015)”. Two other points are already described in our conclusion section on p. 14, line 12ff (other anthropogenic sources, now extended “However, Lee and Brimblecombe (2016) reevaluated the anthropogenic emissions of OCS and its precursors and provide a higher number than previously considered of 598 Gg S $yr^{-1}$. They attribute the largest direct OCS emissions to biomass and biofuel burning as well as pulp and paper factory, and the largest $CS_2$ emissions to the rayon industry. Hence, a hot spot of anthropogenic emissions in the Asian continent might be a potential candidate, together with atmospheric transport, to produce atmospheric mixing ratios as observed by the satellite.”) and p.14, l.18ff (soils).**

*The Van Hobe study was included but more could be done to explain what other cruise data is available. The introduction needs to explain how the cruise measurements and ocean box modeling fit within the context of previous cruise measurement and ocean box modeling. Were these cruises in seasons or locations that have others have not gone?*

**We agree that an overview on previous cruise data should be stressed more in this manuscript, which we do with the following addition apart from the new table 5. and box model description in section 2.4 described in our first comment above.**
**We add a more detailed description on previous OCS, CS2 and DMS measurements in the surface ocean in the introduction, p.3, l. 3ff: "OCS and its atmospheric precursors are naturally produced in the ocean. In the surface open ocean, OCS is present in the lower picomolar range <100 pmol $L^{-1}$, and has been measured on numerous cruises to the Atlantic (Ulshöfer et al., 1995; Ulshöfer et al., 1996; Ulshöfer and Andreae, 1998; Von Hobe et al., 1999) (including 3 latitudinal transects (Kettle et al., 2001; Xu et al., 2001), the Indian Ocean (Mihalopoulos et al., 1992), the Pacific Ocean (Weiss et al., 1995) and the Southern Ocean (Staubes and Geogrii, 1993). OCS is produced photochemically from chromophoric dissolved organic matter (CDOM) (Andreae and Ferek, 2002; Ferek and Andreae, 1984) and by a not fully understood light independent production pathway that depends on temperature and CDOM concentration (Flöck et al., 1997; Von Hobe et al., 2001) Dissolved OCS is efficiently hydrolyzed to $CO_2$ and $H_2S$ at a rate depending on pH and temperature (Elliott et al., 1989). $CS_2$ has been measured in the Pacific and Atlantic oceans in a range of 7.2-27.5 pmol $L^{-1}$ (Xie et al., 1998) and during two Atlantic transects (summer and winter) in a range of 4-40 pmol $L^{-1}$ (Xu et al., 2001). It is produced photochemically (Xie et al., 1998) and biologically (Xie and Moore, 1999), and no significant loss process other than air-sea gas exchange has been identified (Xie et al., 1998). DMS is present in the lower nanomolar range in the surface ocean and has been extensively studies in several campaigns, summarized in a climatology by Lana et al. (2011). DMS is biogenically produced and consumed in the surface ocean, as well as photo-oxidized and ventilated by air-sea exchange (Stefels et al., 2007)."**

*The introduction or discussion could also compare the modeling approach here to what has been done previously. In particular the recent paper by Launois et al.*

**We now discuss the comparison to Launois et al., 2015 on p. 7, l. 4ff: "Launois et al. (2015) to implemented parameterizations for light-independent production, hydrolysis and air-sea exchange similar to vH2003 in the 3D global ocean model NEMO-PISCES. The main differences to the approach used here is the lack of accounting for mixing in our model (discussed in section 3.2.2 (which will theoretically lead to higher simulated concentrations in our case) and the application of a photoproduction rate constant in our model that incorporates information from three open ocean basins in contrast to one from a study in the North Sea (Launois et al., 2015)."**
**A section on the development of the box model used in this study is now added on p. 6., l. 23ff.**

*Some comments may be helpful on alternative approaches for validating these flux estimates. Spatial gradients in atmospheric mixing ratios have been used recently (Berkelhammer et al below). Are other approaches also possible? (M. Berkelhammer, H.C. Steen-Larsen, A. Cosgrove, A. Peters, R. Johnson, M. Hayden and S.A. Montzka (Minor Revisions, July 2016) Radiation and atmospheric circulation controls on carbonyl sulfide concentrations in the marine boundary layer. Journal of Geophysical Research (available upon request).*

**Validation of the flux estimates would need different methods for different scales. Using atmospheric gradients could help to qualitatively validate sources and sinks, but since OCS is such a long-lived gas, the volume mixing ratio of OCS on larger scales is determined by many factors among which direct emissions are only one part (i.e. indirect emissions, conversion in the atmosphere, boundary layer height, trajectory history,…). The mentioned study shows that the ocean can be a source or have a zero net flux regionally, which qualitatively confirms our results, but of course cannot quantitatively validate global emission estimates.**
**Fluxes are physically defined by F=k x Δc, and computing them by measuring the concentration gradient is an established method that has been validated before (Johnson, 2010). Quantitatively validating the emission estimate on the local scale would mean using an independent way of measuring OCS emissions. This can be done by direct flux measurements, e.g. eddy covariance. As**

**OCS is such a long-lived gas, any validation on the global scale needs the global consideration of all sources and sinks, and atmospheric inversions can be used to establish a budget, but need the bottom-up validation of measurements themselves. Any additional data constraints from e.g. time series stations in the tropics and more measurements at sea, tested against the box model, would be beneficial in that respect.**

*Section 2.3 should describe how the box model relates to the measurements. This is done in the results section "Following an earlier study (von Hobe et al., 2003), we use our observations ..." but belongs in the methods. A few additional sentences of explanation may be helpful.*

**We shifted the mentioned part to the method section. Additionally, we clarified that the box model simulations of the two cruises were used as case studies to derive the photoproduction rate constant and validate against data from the tropical region, as a proof-of-concept for the global application of the model on p. 8, l. 15ff. "To extend the p-CDOM-relationship for other ocean basins, we use the two cruises OASIS and ASTRA-OMZ as case studies for parameter optimization of the photoproduction rate constant p. The photoproduction constant p in the case study simulations was fitted individually for periods of daylight >100 W $m^{-2}$ (Fig. 2, blue lines) with a Levenberg-Marquart optimization routine in MatLab version 2015a (8.5.0), by minimizing residuals between simulated and hourly averaged measurements. Different starting values were tested to reduce the risk of the fitted p being a local minimum."**

*Why was the parameter p chosen for fitting the model as opposed to the numerous other parameters. Were other parameters also examined? If not then perhaps this should be stated as an important next step for future work.*

**The parameter p was chosen for fitting since this is the one that is the most difficult to determine from measurements when a wavelength-integrated approach is chosen as is done in our model. We added on p.8 l.10ff: "The rate coefficients for hydrolysis, light-independent production and air-sea exchange are all reasonably well constrained and parameterizations have been derived from dedicated laboratory and field experiments (hydrolysis, air-sea exchange) or from nighttime OCS observations in several regions assuming steady-state (dark production, (Von Hobe et al., 2001)). On the contrary, the photoproduction rate constant p is not well constrained and no generally applicable parameterization exists. von Hobe (2003)have made a start of parameterizing p in terms of CDOM absorption, and found this to be dependent on the exact model setup used with respect to wavelength integration and mixed layer treatment."**

*Why was the von Hobe et al., 2003 study used but not other studies? What is the spatial and temporal extent of the Von Hobe data?*

**The model from von Hobe et al., 2003, is the most recent version of the box model which was further developed in our approach. We added a paragraph on the evolution of this model (beginning of section 2.4, p. 6), see also our comment 2, point (1) above.**

*"global radiation I was" not sure what "I" is*

**The "*I*" should have been in italics, as it is the symbol for global radiation in the equations, which we now corrected.**

*page 6, explain what you mean by "case study simulations"*

**We meant our two cruises as case studies for the global model, which we clarify by adding p. 8, l.15: "To extend the p-CDOM-relationship for other ocean basins, we use the two cruises OASIS and ASTRA-OMZ as case studies for parameter optimization of the photoproduction rate constant**

**p. The photoproduction constant p in the case study simulations was fitted individually for periods of daylight >100 W $m^{-2}$ (Fig. 2, blue lines) with a Levenberg-Marquart optimization routine in MatLab version 2015a (8.5.0), by minimizing residuals between simulated and hourly averaged measurements."**

*define "CTD profiles"*

**now defined "…CTD (conductivity, temperature, depth)…" p.8, l.1**

*The methods section should also include a summary of the time and location of the 3 cruises.*

**We added a new section 2.1 to summarize the study sites and cruises.**
**p.4, l. 5: "Several cruises were conducted to measure the trace gases OCS (OASIS, TransPEGASO, ASTRA-OMZ) and $CS_2$ (TransPEGASO, ASTRA-OMZ). Cruise tracks are depicted in Fig. 1. The OASIS cruise onboard RV SONNE I to the Indian Ocean started from Port Louis, Mauritius to Male, Maledives in July and August 2014, where mainly oligotrophic waters were encountered. TransPEGASO was an Atlantic transect starting in Gibraltar leading to Buenos Aires, Argentinia and Punto Arenas, Chile. It took place in October and November 2014 and covered a variety of biogeochemical regimes. ASTRA-OMZ onboard RV SONNE II started in Guayaquil, Ecuador and ended in Antofogasta, Chile, in October 2015. Although 2015 was an El Nino year, upwelling together with high biological production was still encountered during the cruise (Stramma et al., 2016)."**

*The section "2.1 Measurement set-up for trace gases" present a different method for each cruise. It would be helpful if this section also summarized the impact of having different methods on the different cruises in terms of different precision and other factors that may or may not influence the quality of these measurements.*

**The different methods are discussed in section 2.1, with detailed description of the different methods and precisions for all methods (i.e. OA-ICOS for OCS during OASIS/ASTRA-OMZ, GC-MS for OCS during TransPEGASO as well as for $CS_2$ during TransPEGASO and ASTRA-OMZ) including standards and calibration procedures, temporal resolution of the measurements, precision etc.**
**We added the following on p. 5, l. 29.: "The systems are calibrated against a standard each, but had not been directly intercompared. Still, our measurements are consistent with previous measurements using independent methods as discussed in section 3.2.1. and 3.3".**

*Table 3 missing km^-2 in TransPEGASO flux*

**Now corrected**

*"an non-negligible" to "a"*

**Now corrected**

*Some description is needed of the error associated with assuming a constant atmospheric mixing ratio on TransPEGASO. Seasonal and spatial variation in atmospheric mixing ratios can be on the order of 100 ppt.*

**We performed a sensitivity test for a scenario with 450 and 550 ppt, and added the following sentences to the manuscript p. 6, l. 6: "As air volume mixing ratios of OCS vary over the course of a year, we performed a sensitivity test for a scenrio of 450 and 550 ppt and found mean deviations of +7.8 and -7.8 % respectively."**

*"leaving the missing source still explained" should be "unexplained"?*

**Now corrected**

*Again this is an important contribution of new, high quality data and a well written manuscript. The authors present a compelling approach and with further data could provide a key to closing the global COS budget.*

**References:**


Andreae, M. O., and Ferek, R. J.: Photochemical production of carbonyl sulfide in seawater and its emission to the atmosphere, Glob. Biogeochem. Cycle, 6, 175-183, 2002.

Berry, J., Wolf, A., Campbell, J. E., Baker, I., Blake, N., Blake, D., Denning, A. S., Kawa, S. R., Montzka, S. A., Seibt, U., Stimler, K., Yakir, D., and Zhu, Z.: A coupled model of the global cycles of carbonyl sulfide and co2: A possible new window on the carbon cycle, Journal of Geophysical Research: Biogeosciences, 118, 842-852, 10.1002/jgrg.20068, 2013.

Campbell, J. E., Whelan, M. E., Seibt, U., Smith, S. J., Berry, J. A., and Hilton, T. W.: Atmospheric carbonyl sulfide sources from anthropogenic activity: Implications for carbon cycle constraints, Geophysical Research Letters, 42, 3004-3010, 10.1002/2015gl063445, 2015.

Elliott, S., Lu, E., and Rowland, F. S.: Rates and mechanisms for the hydrolysis of carbonyl sulfide in natural waters, Environmental Science & Technology, 23, 458-461, 10.1021/es00181a011, 1989.

Ferek, R. J., and Andreae, M. O.: Photochemical production of carbonyl sulphide in marine surface waters, Letters to Nature, 1984, 148-150, 1984.

Flöck, O. R., and Andreae, M. O.: Photochemical and non-photochemical formation and destruction of carbonyl sulfide and methyl mercaptan in ocean waters, Marine Chemistry, 54, 11-26, 1996.

Flöck, O. R., Andreae, M. O., and Dräger, M.: Environmentally relevant precursors of carbonyl sulfide in aquatic systems, Marine Chemistry, 59, 71-85, 10.1016/S0304-4203(97)00012-1, 1997.

Glatthor, N., Höpfner, M., Baker, I. T., Berry, J., Campbell, J. E., Kawa, S. R., Krysztofiak, G., Leyser, A., Sinnhuber, B. M., Stiller, G. P., Stinecipher, J., and Clarmann, T. v.: Tropical sources and sinks of carbonyl sulfide observed from space, Geophysical Research Letters, n/a-n/a, 10.1002/2015gl066293, 2015.

Johnson, M. T.: A numerical scheme to calculate temperature and salinity dependent air-water transfer velocities for any gas, Ocean Science, 6, 913-932, 10.5194/os-6-913-2010, 2010.

Kettle, A. J.: Extrapolations of the flux of dimethylsulfide, carbon monooxide, carbonyl sulfide and carbon disulfide from the oceans, PhD, Graduate Program in Chemistry, North York, Ontario, 2000.

Kettle, A. J., Rhee, T. S., von Hobe, M., Poulton, A., Aiken, J., and Andreae, M. O.: Assessing the flux of different volatile sulfur gases from the ocean to the atmosphere, Journal of Geophysical Research: Atmospheres, 106, 12193-12209, 10.1029/2000jd900630, 2001.

Kettle, A. J.: Global budget of atmospheric carbonyl sulfide: Temporal and spatial variations of the dominant sources and sinks, Journal of Geophysical Research, 107, 10.1029/2002jd002187, 2002.

Kremser, S., Thomason, L. W., von Hobe, M., Hermann, M., Deshler, T., Timmreck, C., Toohey, M., Stenke, A., Schwarz, J. P., Weigel, R., Fueglistaler, S., Prata, F. J., Vernier, J.-P., Schlager, H., Barnes, J. E., Antuña-Marrero, J.-C., Fairlie, D., Palm, M., Mahieu, E., Notholt, J., Rex, M., Bingen, C., Vanhellemont, F., Bourassa, A., Plane, J. M. C., Klocke, D., Carn, S. A., Clarisse, L., Trickl, T., Neely, R., James, A. D., Rieger, L., Wilson, J. C., and Meland, B.: Stratospheric aerosol—observations, processes, and impact on climate, Reviews of Geophysics, 54, n/a-n/a, 10.1002/2015rg000511, 2016.

Kuai, L., Worden, J. R., Campbell, J. E., Kulawik, S. S., Li, K.-F., Lee, M., Weidner, R. J., Montzka, S. A., Moore, F. L., Berry, J. A., Baker, I., Denning, A. S., Bian, H., Bowman, K. W., Liu, J., and Yung, Y. L.: Estimate of carbonyl sulfide tropical oceanic surface fluxes using aura tropospheric emission spectrometer observations, Journal of Geophysical Research: Atmospheres, 120, 11,012-011,023, 10.1002/2015jd023493, 2015.

Lana, A., Bell, T. G., Simo, R., Vallina, S. M., Ballabrera-Poy, J., Kettle, A. J., Dachs, J., Bopp, L., Saltzman, E. S., Stefels, J., Johnson, J. E., and Liss, P. S.: An updated climatology of surface dimethlysulfide concentrations and emission fluxes in the global ocean, Glob. Biogeochem. Cycle, 25, 10.1029/2010gb003850, 2011.

Launois, T., Belviso, S., Bopp, L., Fichot, C. G., and Peylin, P.: 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, Atmos. Chem. Phys., 15, 2295-2312, 10.5194/acp-15-2295-2015, 2015.

Lee, C.-L., and Brimblecombe, P.: Anthropogenic contributions to global carbonyl sulfide, carbon disulfide and organosulfides fluxes, Earth-Science Reviews, 160, 1-18, http://dx.doi.org/10.1016/j.earscirev.2016.06.005, 2016.

Mihalopoulos, N., Nguyen, B. C., Putaud, J. P., and Belviso, S.: The oceanic source of carbonyl sulfide (cos), Atmospheric Environment, 26A, 1383-1394, 1992.

Sandoval-Soto, L., Stanimirov, M., von Hobe, M., Schmitt, V., Valdes, J., Wild, A., and Kesselmeier, J.: Global uptake of carbonyl sulfide (cos) by terrestrial vegetation: Estimates corrected by deposition velocities normalized to the uptake of carbon dioxide (co2), Biogeosciences, 2, 125-132, 2005.

Staubes, R., and Geogrii, H.-W.: Biogenic sulfur compounds in seawater and the atmosphere of the antarctic region, Tellus, 45B, 127-137, 1993.

Stefels, J., Steinke, M., Turner, S., Malin, G., and Belviso, S.: Environmental constraints on the production and removal of the climatically active gas dimethylsulphide (dms) and implications for ecosystem modelling, Biogeochemistry, 83, 245-275, 10.1007/s10533-007-9091-5, 2007.

Stramma, L., Fischer, T., Grundle, D. S., Krahmann, G., Bange, H. W., and Marandino, C. A.: Observed el niño conditions in the eastern tropical pacific in october 2015, Ocean Sci., 12, 861-873, 10.5194/os-12-861-2016, 2016.

Suntharalingam, P., Kettle, A. J., Montzka, S. M., and Jacob, D. J.: Global 3-d model analysis of the seasonal cycle of atmospheric carbonyl sulfide: Implications for terrestrial vegetation uptake, Geophysical Research Letters, 35, L19801, 10.1029/2008gl034332, 2008.

Uher, G., and Andreae, M. O.: The diel cycle of carbonyl sulfide in marine surface waters: Field study results and a simple model, Atmospheric Geochemistry, 2, 313-344, 1997.

Ulshöfer, V. S., Uher, G., and Andreae, M. O.: Evidence for a winter sink of atmospheric carbonyl sulfide in the northeast atlantic ocean, Geophysical Research Letters, 22, 2601-2604, 1995.

Ulshöfer, V. S., Flöck, O. R., Uher, G., and Andreae, M. O.: Photochemical production and air-sea exchange of carbonyl sulfide in the eastern mediterranean sea, Marine Chemistry, 53, 25-39, 1996.

Ulshöfer, V. S., and Andreae, M. O.: Carbonyl sulfide (cos) in the surface ocean and the atmospheric cos budget, Atmospheric Geochemistry, 3, 283-303, 1998.

Von Hobe, M., Kettle, A. J., and Andreae, M. O.: Carbonyl sulphide in and over seawater: Summer data from the northeast atlantic ocean, Atmospheric Environment, 33, 3503-3514, 1999.

Von Hobe, M., Cutter, G. A., Kettle, A. J., and Andreae, M. O.: Dark production: A significant source of oceanic cos, Journal of Geophysical Research, 106, 31217, 10.1029/2000jc000567, 2001.

von Hobe, M.: Photochemical and physical modeling of carbonyl sulfide in the ocean, Journal of Geophysical Research, 108, 10.1029/2000jc000712, 2003.

Wang, Y., Deutscher, N. M., Palm, M., Warneke, T., Notholt, J., Baker, I., Berry, J., Suntharalingam, P., Jones, N., Mahieu, E., Lejeune, B., Hannigan, J., Conway, S., Mendonca, J., Strong, K., Campbell, J. E., Wolf, A., and Kremser, S.: Towards understanding the variability in biospheric co2 fluxes: Using ftir spectrometry and a chemical transport model to investigate the sources and sinks of carbonyl sulfide and its link to co2, Atmos. Chem. Phys., 16, 2123-2138, 10.5194/acp-16-2123-2016, 2016.

Weiss, P. S., Johnson, J. E., Gammon, R. H., and Bates, T. S.: Reevaluation of the open ocean source of carbonyl sulfide to the atmosphere, Journal of Geophysical Research, 100, 23083-23092, 1995.

Xie, H., Moore, R. M., and Miller, W. L.: Photochemical production of carbon disulphide in seawater, Journal of Geophysical Research: Oceans, 103, 5635-5644, 10.1029/97jc02885, 1998.

Xie, H., and Moore, R. M.: Carbon disulfide in the north atlantic and pacific oceans, Journal of Geophysical Research, 104, 5393, 10.1029/1998jc900074, 1999.

Xu, X., Bingemer, H. G., Georgii, H. W., Schmidt, U., and Bartell, U.: Measurements of carbonyl sulfide (cos) in surface seawater and marine air, and estimates of the air-sea flux from observations during two atlantic cruises, Journal of Geophysical Research, 106, 3491, 10.1029/2000jd900571, 2001.