We thank the referee#2 for taking the time to read the manuscript and offer helpful comments and suggestions. The referee's comments are repeated below with our response in bold.

Responses to those comments are listed below:

1. Atmospheric OCS has several sources and sinks, as mentioned in the paper. Untill now, the sources and sinks as well as the budget of atmospheric OCS are highly uncertain. The plant uptake of OCS is probably the most important factor driving the seasonal variation of OCS and directly related with CO2. Some studies (Xu et al., 2002; Sandoval-Soto et al., 2005; Montzka et al., 2008) indicated that this sink of OCS was significantly underestimated in previous studies like Kettle et al. (2002). Increasing this sink did reduce the differences between observed and simulated OCS concentrations. However, I think this paper relies too much on the adjustment of the vegetation sink of OCS. Only plant sink and ocean source were changed in different GEOS-Chem simulations (K2002x2, K2002x3). Other possiblities were excluded without convincing explanations. Some related studies are not referenced and considered in the discussions. High concentration and anthropogenic source of OCS were observed in some regions (e.g., Guo et al., 2010; Cheng et al., 2015). Is it possible that the anthropogenic source is underestimated? Can the large discrepancies between the observed and modeled OCS over the North Hemisphere (Fig. 3) be explained by such underestimation? The in-situ measurements (Weiss et al., 1995; Xu et al., 2001) suggested that the open ocean may only be a very small source or even a sink of OCS, particularly in the tropics. However, ocean emission in the tropical regions is increased to balance the global budget of OCS after increasing the OCS uptake by plant. Soil uptake of OCS was increased in the SiB simulation though a multi-seasonal study in a forest suggested that the soil sink of OCS accounts for only less than 1% of the OCS flux into the ecosystem (Xu et al., 2002; Steinbacher et al., 2004). I do not mean that the authors should make an extensive review. However, the published studies relevant to this work should be considered appropriately. After robust analysis you would be able to obtain a more reliable vegetation sink of OCS, which can then be used to constrain the GPP.

Response: **We thank the referee for the comments on OCS sources and sinks. We modified the introduction of the paper and included a more detailed review of the previous studies. For further clarification we modified the description of ocean fluxes rescaling in section 5.2.3:**

In this work, the ocean emissions were only modified at certain latitudes by a single regionally-specific factor. Because the role of ocean direct emission is under debate (Weiss et al., 1995; Xu et al., 2001; Berry et al., 2013; Launois et al., 2015a) and the variations of the direct and indirect ocean emissions are similar (kettle et al., 2002a), we take all ocean emissions as a whole when rescaling, similarly to the method in Suntharalingam et al. (2008).

2. A significant vertical gradient of OCS can be caused by seasonality of sinks and source (see Campbell et al., 2008). Is it possible to compare measured and modeled vertical profiles? If so, there might be some additional information to prove or disprove the changes in the sources and sinks.

Response: **The referee is correct that there is some profile information that could potentially be exploited if we were to be confident in the independence of the partial columns from the FTS retrievals. The degrees of freedom for signal (DOFS) gained in the retrievals is about 2.5 on average. That means we can derive 2.5 independent pieces of information, which is not sufficient to generate accurate profiles of OCS. In particular there is not enough information in the measurements to divide the troposphere into bins.**

3. P26036, L12, Whelan et al. (2013) is about emission of OCS from salt marsh vegetation. Salt marsh itself is also a source not a sink of OCS. Previous studies indicate that oxic soil is a sink of OCS. However, the strength of this sink is highly uncertain but may be very small (Xu et al., 2002; Steinbacher et al., 2004).

Response: **The nature of soil uptake of OCS is still largely unknown. The uptake rate varies with soil types and other physical parameters (Van Diest and Kesselmeier, 2008; Sun et al., 2015). Rather than speculating, we have maintained the uptake relationships from Berry et al (2013), with some changes in the parameters. There is a lot of research ongoing into soil uptake, and we will update the SiB code once we feel comfortable with a global relationship.**

4. P26031, L9-14, it would be better if data from same other sites can be used in this study. For example, there are also FTIR measurements of OCS and CO2 at Lauder, New Zealand (Griffith et al., 1998; https://tccon-wiki.caltech.edu/Sites/Lauder).

Response: **We agree that including more sites would be ideal. We have included Eureka and Mauna Loa in the revised manuscript. However, it is not yet possible to include Southern Hemispheric sites at this time. We have been working on a harmonized retrieval approach with other groups to ensure inter-site consistency, because the tropical and Southern Hemispheric sites are in wet conditions and the retrievals from the spectra are affected substantially by water. Mauna Loa is at high altitude, and therefore also less affected by water vapor. The retrieval strategy works well for the rather dry Northern sites we have chosen in the paper; however, we cannot yet be sure that the effects of water and how it is handled in the retrievals are consistent between dry and wet sites, and this may have impact on the relative seasonal cycles and latitudinal patterns. We prefer to err on the side of caution, and not include more additional measurements for which we might over-interpret the resulting trends and patterns. In addition, the Southern Hemispheric OCS seasonal cycles are more affected by ocean fluxes, which have large uncertainties. In this paper, we focus on the Northern Hemispheric land fluxes and only rescaled the ocean fluxes in a simple way to balance the global budget. This method can be used to get a reasonable latitudinal gradient, which was evaluated with the help of HIPPO data. After this rescaling, we can analyze the seasonal variation in the Northern Hemisphere mainly driven by biospheric fluxes.**

5. P26030, L19-22, "When interpreted by models, total column measurements are much less sensitive to assumptions on the boundary layer mixing, because every molecule in the atmospheric column is detected, independent of whether it is at the surface or in the upper troposphere". I feel this is a little

contraditory to “The FTIR OCS retrievals are sensitive at low altitude and can capture the variations due to the biospheric processes” (P26029, L24-25).

Response: **The first statement is to point out the difference between column measurements and surface in-situ measurements. Column measurements are less affected by the assumptions on the boundary layer mixing, which have large uncertainties in transport models. Therefore column measurements can provide additional information. The later sentence says that the retrievals are sensitive at low altitudes, which is compared to satellite retrievals, but does not mean they are not sensitive to high altitude. We can get OCS total columns and partial columns in the troposphere. We have clarified this in the revised manuscript.**

6. P26035, L24-28, some original studies should be cited here, e.g., Protoschill-Krebs and Kesselmeier (1992), Protoschill-Krebs et al. (1996), etc.

Response: **We have included these suggested references.**

7. P26037, L14-15, the CO2 maximum seems not to be in spring but in later winter.

Response: **The CO2 maximum is in February or March, so late winter or early spring. This has been changed in the manuscript.**

8. P26037, L24-25, such preference was also found in field experiments (Xu et al., 2002).

Response: **We included the suggested reference.**

9. P26039, L14-23 and Table 3, factors other than plant and ocean? Ocean is probably not that large source of OCS (Weiss et al., 1995; Xu et al., 2001).

Response: **We only rescaled the plant and ocean fluxes in this paper, while the other fluxes were kept the same to K2002. Recent studies suggest that the missing OCS sources are from the ocean (Berry et al., 2013; Launois et al., 2015a). In this paper, we take the direct and indirect ocean fluxes as a whole when rescaling.**

10. P26040, L7-10, evenif you had included the interannual variability in the simulations, you would not be able to judge the comparison between K2002x2 and K2002x3 for each year.

Response: **Ideally, if the measurements are continuous and less noisy, and the inter-annual variability is right in the simulations, it will be easier to judge the comparison between K2002x2 and K2002x3 by looking at individual years. In this figure, we agree that it’s not possible to judge that. We deleted “which makes it difficult to judge the comparison between K2002_2 and K2002_3 for each year”.**

11. P26041, L6-8, were these values arbitrarily chosen?

Response: **These values were chosen to increase the ocean sources to balance the global budget after changing the land sinks.**

12. P26041, L20-21, I think this statement is a little rash (see comments 1).

Response: **We have modified it in the revised manuscript.**

13. P26042, L10-11, can you prove this?

Response: **We used Figure 4 to show the difference between SiB and Kettle land fluxes, and proved that the latitudinal distribution of these two fluxes is different.**

14. P26042, L18, "The plant uptake of K2002"? K2002x2 or K2002x3?

Response: **The proportions are the same for K2002, K2002x2, and K2002x3, because we rescaled the plant uptake using a single factor at all the latitudes.**

15. P26043, L16, "in Fig.6" or in Fig. 5?

Response: **Corrected.**

16. P26045, L4-7, does this mean that we would not abtain a better estimate of GPP from OCS simulation than directly from the CO2 simulation?

Response: **The seasonal cycles and latitudinal distribution of CO2 are determined by NEP, which is the sum of GPP and Re. Models can get reasonable NEP, but wrong GPP (and Re). With the help of OCS, we can evaluate the estimated GPP in the model.**

17. P26045, L21, ". . .in SiB simulation". ". . .in SiB simulation of OCS"?

Response: **Corrected.**

18. P26046, L 16, a missing source is possible, but I do not think an overestimate of a sink is excluded without critical review.

Response: **From the comparison between HIPPO and the model simulation with K2002, which has very small OCS sinks, the simulated OCS concentrations in the Northern mid latitude are also lower than HIPPO measurements, therefore an overestimate of a sink is unlikely. The largest disagreement is not during the growing season, and the plant sink is relatively small in all the fluxes (Fig. 5), therefore could not result in big mismatch.**

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