Dear Byron Blomquist:

Thank you for your constructive and helpful comments which led us to reconsider our findings and the way they are presented. Below are our detailed line-by-line responses.

**General comments:**

**R1**: Line 17, 25-28 and elsewhere: The conclusion that seawater DMS was 'relatively low' seems too vague and imprecise. Better to just state the mean/range/variance of the observations and compare this to previous measurements. I wouldn't stress a comparison with Lana et al. 2011 too much (PMEL database is better). As you have mentioned, there isn't much data from the "Humboldt Current Coastal' province, as defined in Lana et al., and their seasonal extrapolation for this province from limited data is just a best guess. This is a region where we expect seawater DMS concentration to vary quite a bit, spatially and temporally (on seasonal, yearly and perhaps decadal timescales). Combined with previous measurements, these data provide a better picture of seawater DMS in this region than the Lana et al. gridded product.

Authors: We thank the referee for his suggestion, and we have modified the corresponding parts of the text, in order to present our results in a precise manner.

**R2**: Since this study doesn't represent extremes in the ENSO cycle it would be interesting to know how well previous studies have sampled ENSO variability. Can the authors discuss their study and previous measurements from the ENSO perspective a bit more? Are we still far from a representative sample of DMS variability during ENSO extremes? Should this be a focus of future studies?

Authors: We thank the referee for his questions on the ENSO topic. Initially, this work was designed to explore the effect of ENSO on the biogenic production of DMS. In addition to our DMS data, we also extracted all previous DMS data collected from the Eastern Tropical South Pacific (ETSP) from the PMEL database. However, we could not draw a definitive conclusion on whether ENSO would affect DMS concentrations, which was mainly attributed to the fact that no cruise took place in the middle of ENSO events. For example, the upwelling waters were still observed, at least during part of the cruises in 1982 and 2015, when two extreme EI Niño events were occurring. This indicates that they were going through developing EI Niño events, and as a result, they are not adequately representative of fully developed EI Niño events. For other previous DMS data, they were generally collected in open ocean areas in the ETSP, and they were less influenced by ENSO events (Bates and Quinn, 1997). Therefore, our understanding is still hindered by the limited DMS data set, especially in the Peruvian coastal regions, and that is also why we shifted from the original idea to the current version with only briefly describing the story of ENSO and DMS. Also, in addition to the ENSO events, the seasonal cycles of DMS along the Peruvian upwelling make the comparison between previous measurements and our measurements more complex. Overall, to justify the influence of ENSO on DMS and to gain a more comprehensive understanding, we think more data are required, especially those collected from fully developed EI Niño events. It is clear that the link between ENSO and DMS should be a focus of future studies as this will expand our knowledge, for example, on how DMS fluxes change during/after the EI Niño/La Niña events, and if they have a

significant impact on the regional/global climate.

**R3**: line 19 and elsewhere: the terminology' flux density' is a bit odd and not typically used in the air-sea flux community. It's potentially confused with usage like 'spectral density' for power spectra, etc. Better to just say 'flux' or 'fluxes'.

Authors: We have replaced all 'flux density' and 'flux densities' with 'flux' and 'fluxes', respectively.

**R4**: lines 129-131: Although it doesn't make much difference to the conclusions of your study, we should really discourage the use of transfer models like Nightingale 2000 for DMS flux estimates. Numerous direct studies of DMS air sea flux have been conducted over the past decade or more and the relationship with wind speed (or friction velocity) is closer to linear, especially over the wind speed range of your study. Transfer models based on highly insoluble tracers don't represent DMS transfer well (see Fig 3 in Bell et al. 2013). Better to use a model that's actually validated with direct DMS flux observations. The COARE gas transfer model has been used (e.g. Bell et. al. 2013, Blomquist et al. 2017) but for this paper a simple linear relationship is probably fine: e.g. Huebert et al. 2010, Fig 3 or Blomquist et al. 2017 Fig 5.

Authors: We thank the referee for this advice. We acknowledge that the DMS gas transfer coefficient exhibited more of a linear (as referee suggested) instead of a quadratic (e.g., Nightingale 2000) dependence with the wind speed. However, to allow a direct comparison with Lana's climatology, we decided to use the same equation which was adopted in Lana et al. (2011) to reduce the uncertainty in the comparison. In the future studies, we will surely avoid using transfer models like Nightingale 2000 for estimating DMS fluxes, but for this manuscript we will continue with N2000 as the overall conclusions are not changed. Please note, we did refer to the direct DMS covariance flux work of Marandino et al. (2009) and Yang et al. (2011), who also found a linear k-U dependence with measurements in the study region.

**R5**: I'm confused by the reference to 'terrestrial DMS sources'. What are these potential sources? I'm not aware of any, especially in the arid coastal climate of Peru and Chile. A more likely source of high atmospheric DMS variability would be hotspots close to shore, which might be implicated if trajectories in the marine boundary layer follow the coastline for some distance.

Authors: The referee is right, there are no references reporting atmospheric DMS directly emitted over the terrestrial regions of Peru and Chile. However, there are some references reporting that terrestrial sites could be a source of atmospheric DMS in South American, such as coastal marshes (Crutzen et al., 2000) and tropical soils (Jardine et al., 2015). Some references also reported terrestrial sources in other regions, such as biomass burning in Australia (Meinardi, 2003) and the Pearl River Delta in China (Chan et al., 2006). We have added these references to the corresponding text.

**Technical comments:**

line 15: don't need a comma after 'present'.

Authors: Done.