I think the authors have addressed the most significant concerns and this version is suitable for publication with a couple minor additions. If they wish to use the Nightingale transfer coefficient model to estimate DMS fluxes for comparison with Lana et al., that's OK, but they should indicate that we now know the models with quadratic wind speed dependence are not representative of DMS transfer and will give significant overestimates for wind speeds above 10 m/s.

Food for thought: A revision to the Lana et al. 2011 global DMS model is in preparation. I am hoping authors of this revision will address some of the deficiencies in the DMS transfer estimate. But, direct observations from the region covered by this manuscript are still sparse and not representative of the entire ENSO cycle (according to the conclusions of this paper). Nutrient measurements from this region are likely much better represented over all seasons. I wonder if the authors feel the correlation with N:P could be used to help validate the reasonableness of the new global DMS climatology, or perhaps even find use as a predictive factor for surface seawater DMS?