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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term trends of global marine primary and secondary aerosol production during the recent global warming hiatus (2000–2015)" *by* S.-K. Song et al.

Anonymous Referee #2

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General comments:

After closer reading of this manuscript, I am unable to recommend this manuscript for publication in ACP. The subject of the manuscript is a calculation of inferred/estimated trends in DMS and sea spray aerosol emissions over the years 2000-2015, as well as trends in the AOD contributed by these aerosol sources, which may have occurred as a consequence of variability in wind speed and sea surface temperature during this time period. However, the methods used have such serious deficiencies as to make the results uninterpretable.

I was surprised to find no discussion or evaluation of potential instrument biases, dis-

crepancies, retrieval uncertainties, or long-term drifts in satellite measurements here. Without this information, it is impossible to determine whether any trends in the wind speed or SST time series used here are likely to be reliable reflections of real-world trends.

In particular, in the analysis performed here, for both wind speed and sea surface temperature, two different satellites were used with breaks in the long-term time series (U10 from QuikSCAT for 2000-2007, and from ASCAT for 2008-2015; SST from NOAA AVHRR for 2000-2002 and from Aqua MODIS for 2003-2015). Are measurements between these different instruments consistent? Is there a potential that a discontinuity or bias could be introduced in the time series by using different instruments during different periods? Since the analysis focusses on time series, this information is critical to understanding the causes of the inferred trends, and ruling out potential instrument bias.

L. 122-123: The first reviewer has already pointed out some of the (well-known) challenges in estimating oceanic DMS concentrations. Some comparison with observed ocean DMS concentrations and fluxes would be useful in evaluating how well this approach produces results that resemble the real world.

The calculation of AOD associated with marine aerosol sources, as described in the manuscript, is also overly simplistic and will not produce interpretable results: - L. 144-158: It's not possible to realistically calculate the AOD from emitted aerosol without treatment of its transport, microphysical evolution, and removal in the atmosphere. The distribution of SSA concentrations in the atmosphere is controlled by removal processes (e.g. the distribution of precipitation) at least as much as by emission processes. - L. 159-169: It's not possible to generate reasonable distributions of DMS-derived sulfate aerosol in this way, with no consideration of atmospheric transport and chemistry (reaction with OH and subsequent reaction mechanisms), which occurs over time scales of several days to weeks. There is a large literature on the production of sulfate aerosol from DMS-derived SO2, which shows the critical role of transport and

chemistry processes in determining the contribution of DMS-derived SO2 to global aerosol concentrations. As just one example of this, in a paper that is referenced by the authors, Korhonen et al. (2008), Figure 8 clearly shows that the geographic distributions DMS emissions, and of the increase in cloud condensation nuclei number associated with DMS emissions, are nearly uncorrelated.

These are major flaws in the methodology that need to be addressed before any of the results will be interpretable.

In addition, I don't understand the underlying motivation for this study. The so-called "hiatus" period from 1998-2012 has been explained as most likely related to decadal-scale internal modes of climate variability, in particular, the Pacific Decadal Oscillation. It's unclear to me why this period should be of particular interest for marine aerosol emissions.

If the authors choose to revise this paper, they may also find the following specific comments useful: - L. 41-42: "However, the rate of warming (0.05 deg C per decade) over the last 15 years (1998–2012) was smaller than that (0.12 deg C per decade) calculated for 1951–2012 (IPCC, 2013)." This sentence needs to be contextualized to avoid potentially misleading readers. The 1951-2012 period represents 61 years of climate data, while the 1998-2012 period represents only 15 years; a fifteen-year period is well-known to be too short to reliably detect long-term climate trends in global mean surface air temperature, and I think this should be explicitly noted here. For example, research at the UK Met Office on understanding the slowdown in warming during this fifteen-year period, often termed a "hiatus" in global warming, found that in model simulations of a climate warming at a rate of 0.2 deg C per year, periods of cooling occur frequently. For example, a -0.10 deg/yr cooling period lasting fifteen years occurs more than 10% of the time (Roberts et al., Nature, 2015; https://www.nature.com/articles/nclimate2531). In light of such analyses, this "hiatus" period is more realistically viewed as a part of the expected natural variability in the climate system. - L. 43-44: This sentence should be updated: "According to a recent study (England et al., 2014), the global mean SAT

has remained flat since around 2001.". It is now 2018; the global mean surface air temperature has clearly risen between 2001 and 2018.

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