**Review: Dating Hiatuses: A statistical model of the recent slowdown in global warming – and the next one (esd-2018-81)**

This paper uses econometric techniques to investigate the causes for a hiatus in warming and forecast whether another hiatus will occur in the future. The main results indicate that three factors drove the hiatus, the unusually warm year of 1998, the ENSO itself, and increases in tropospheric aerosols. With regard to the future, the authors conclude that the rate at which forcings rise will influence whether there is a hiatus in the future. These results (and to some degree the econometric results that are used to generate them) will interest the readers of *Earth System Dynamics.* As such, the manuscript should be considered for publication. The current form of the manuscript is close to being ready for publication. As described below, I have two substantive concerns and some minor concerns about the presentation/interpretation of results.

*Substantive Comments*

My first substantive concern focuses on the Oceanic Multidecadal Oscillation (OMO) and the empirical methods used to identify its effects. The OMO phenomenon needs to be described in more detail. Specifically, the authors need to define the OMO explicitly, briefly review the physical mechanisms thought to drive it, and how the OMO affects surface temperature.

The authors also need to investigate the degree to which their results are sensitive to the methods used to estimate its effect on temperature. On page 8, line 1, the authors state that the OMO is estimated using a sine function. The authors should explain why a sine function is used. Figure 1 suggests that the sine function is used to fit a time series for stochastically detrended GMT. But the authors do not explicitly define the data used to fit the sine function nor do the authors describe how they stochastically detrend global mean temperature. Because many readers are not econometricians, the authors need to show the equation(s) that they use to stochastically detrend global mean temperature.

Finally, the authors need to investigate the degree to which this approach affects their results. My cursory review of the literature indicates that one set of authors calculate an index for the Atlantic Multidecadal Oscillation index from linearly detrended North Atlantic sea surface temperature anomalies while others identify the OMO signal using empirical orthogonal functions. How would the results reported in this manuscript change if they used one of these methods to estimate the effects of the OMO, instead of using the stochastically detrended global mean temperature?

My second substantive concern focuses on the time series used to represent the cooling effects of tropospheric sulfates. In Figure 1, the authors identify the ability of various forcings to account for the missing heat, as represented by degrees Celsius anomaly from the base period. This is a very straightforward and understandable way to approach the problem. My issue here concerns the forcings used to simulate the model, which are the time series used to simulate the GISS model. In general, the forcings used to simulate the GISS model are highly stylized. They are largely linear with little variation in growth rates over time. This is especially true for reflective tropospheric aerosols. This linearity is different from the time series for anthropogenic sulfur emissions that are assembled by Steven J. Smith (and others) at the Pacific Northwest National Laboratory. These data are updated such that it is compatible with the sample period used by the authors

(https://www.geosci-model-dev.net/11/369/2018/gmd-11-369-2018.pdf). I suggest that the authors investigate the degree to which their results are sensitive to the forcing used by redoing their analysis with the time series from the paper by Hoesly *et al*., (2017).

*Minor Comments*

**Page 3**: "explain past multidecadal cooling or hiatus periods, such as the decades following the temperature spikes in about 1877 and 1943." On page 271 Kaufmann et al (2006) (cited by the authors) write "The radiative forcing of anthropogenic sulfur emissions increases at about the same rate as greenhouse gases between 1944 and 1976. As a result, there is relatively little net increase/decrease in total radiative forcing and therefore, global surface temperature. The timing of these temperature effects is consistent with results obtained from model simulations (Andronova and Schlesinger, 2000; Tett et al., 1999)." The authors should reconcile their statement about the hiatus with the explanation based on a slight decrease in total forcing.

**Page 5** Lines 1-3 describe the method used to covert a change in W/m2 to temperature ($0.536 + 0.561 \times 0.431 \approx 0.777^{o}$C). The authors should explain where 0.430 'comes from.'

**Page 11** "Our main findings for this period suggest that the three main factors driving the hiatus were (a) the unusually warm year of 1998, even conditional on the ENSO, (b) the ENSO itself, and (c) the increase in tropospheric aerosols during that period, though the latter is measured with a high degree of uncertainty" This is a very important component of the authors results, but these results are not really clear in the abstract. The authors should edit this abstract to make these results clearer. Also the abstract should highlight the result that the occurrence of a future hiatus depends on in part on the rate at which forcing grows.

**Lieteratiure cited**

Hoesly, R. M., Smith, S. J., Feng, L., Klimont, Z., Janssens-Maenhout, G., Pitkanen, T., Seibert, J. J., Vu, L., Andres, R. J., Bolt, R. M., Bond, T. C., Dawidowski, L., Kholod, N., Kurokawa, J.-I., Li, M., Liu, L., Lu, Z., Moura, M. C. P., O'Rourke, P. R., and Zhang, Q.: Historical (1750–2014) anthropogenic emissions of reactive gases and aerosols from the Community Emission Data System (CEDS), Geosci. Model Dev. Discuss., doi:10.5194/gmd-2017-43, in review, 2017.