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Interactive comment on "Dating Hiatuses: A Statistical Model of the Recent Slowdown in Global Warming – and the Next One" *by* J. Isaac Miller and Kyungsik Nam

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Referee Comment: This paper focuses on the detection and attribution of the temperature hiatus over the last decade, as a hot issue for the climate change studies. Based on the semiparametric cointegrating regression approach, the authors give one explanation of the temperature hiatus by considering many physical causes, which is useful for improving the understanding. However, there are two questions I concerned. One is the influence of data quality on the results, and the other is the influence of the temperature hiatus on the whole temperature variability in the future.

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Author Response: Thank you for your careful reading of our manuscript and suggestions for improvement. We carefully considered the issues you raised and revised the manuscript accordingly. Our responses to each point raised ensue.

Referee Comment: Some specific comments are given as follows: (1) The volcanoes of course influence the temperature variability, with a contribution of 1% explained by the authors. However, as is known, the results from regressive approaches have big uncertainty, and so the 1% contribution is real or just bias?

Author Response: We spent much time thinking about uncertainty when we conducted our analysis. The 1% for volcanoes is just a point estimate, and our interval estimate of $(0.3, 1.9)\%$ suggests that the real contribution could be higher or closer to zero. This range comes from not only the "usual" uncertainty of the regression error, but also uncertainty in the underlying data, which we tried to accommodate in a reasonable but admittedly *ad hoc* manner based results in the IPCC chapter of Myrhe *et al.* (2013), explained in our SOM. We use the commonly employed data of Hansen *et al*. (2017), in which forcing from stratospheric aerosols is attributed to volcanic activity, while forcing from tropospheric aerosols is attributed to anthropogenic sulfur dioxide emissions. Vernier *et al.* (2011) refute previous studies that attributed an increase in stratospheric aerosols to emissions, which gives us some confidence that we are interpreting these measurements appropriately. While those authors do not discuss the effect of volcanic activity on the hiatus directly, their Figure 5 suggests that the stratospheric aerosol levels from Mt. Pinatubo subsided until about 1997, and the increase since then – to which we attribute $(0.3, 1.9)\%$ of the recent hiatus – has been relatively small.

Author Action: **Amended** footnote 6 to explain that uncertainty results from both statistical error and data. **Added** some of the explanation above to page 6 (lines 4-8) of the revised manuscript.

Referee Comment: (2) The quality and quantity of observations prior to the satellite era are questionable. How much actual observation data is included prior to 1970s in the monthly HadSST3 used in this study? Please provide that information for the credibility of the results, especially for the results in Figure 2. Besides, if the results have biases only using the HadCRU data? as this data set has biases at monthly scales.

Author Response: We certainly agree. Keeping in mind that the HadSST3 data already aggregate observations within five degrees latitude and longitude in order to alleviate some of the observational uncertainty, the maximum number of observations possible is $36 \times 72 = 2,592$ per month and $2,592 \times 12 = 31,104$ per year. We include a figure here (please see below) and in the revised version of the SOM that shows the actual number of observations per year. Observations are based on readings from buoys and ships. A maximum of $17,391$ is attained in 1979 and the number is slightly lower but steady since then, as the referee points out. Prior to the 1970's the number of observations generally increases over time, but with noticeable dips during major international disruptions, such as World War II, World War I, and the American Civil War. Even at its lowest in 1866, just after the American Civil War, the number of observations $(2,414)$ still exceeds a thousand.

We use temperature data on both sides of the model. On the left-hand side, we use the global mean temperature anomaly from HadCRUT4, which also includes land. Static bias and idiosyncratic/short-run error from uncertainty in the measurement of the data are picked up by the intercept α_0 and regression error η_t respectively in equation (2) of the paper. However, a change in quality and quantity of temperature measurements over time may cause heteroskedasticity of unknown form in the regression error. Our coefficient estimates would be less precise as a result, but should still be statistically consistent.

On the right-hand side, we use disaggregated HadSST3 data. By design, our method for smoothing the cyclical component should eliminate any idiosyncratic/short-run uncertainty in these data. If there is a static bias throughout the time span, it is picked up by a non-zero estimate of θ_4 in equation (S.1) of the SOM, which we estimate to be nearly zero (Table S.1). Since we use θ_4 to build the cyclical component, any bias in our estimate of θ_4 is picked by the intercept α_0 in equations (1) and (2), but should not affect the other coefficients estimates used to make our inferences. However, if the bias changes over time in a non-idiosyncratic way, it would be a more complicated problem to explicitly model, and we leave it for future consideration.

Author Action: **Included** new figure and **added** exposition similar to that above regarding the number of observations on page 3 (lines 6-10) of the SOM**. Added** exposition similar to that above regarding bias on page 6 (line 12) through page 7 (line 3) of the SOM.

Referee Comment: (3) I cannot understand why using the ENSO to explain the temperature Hiatus at decadal scales, because it mainly exhibits oscillatory variations at interannual scales. Further, the authors also investigate the temperature Hiatus in the future based on the OMO, but not the ENSO. How to coordinate the influence of the OMO and ENSO on the temperature variability?

Author Response: As the referee points out, the ENSO oscillates at an interannual scale. It is quasiperiodic with a period of about 5-6 years. Roughly every three El Niño episodes are so-called “super El Niños” with much higher amplitudes than the intervening episodes. In other words, the ENSO also oscillates at a decadal scale, roughly 15-18 years. The last two peaks of the longer oscillation were in 1997-98 and 2015-16, marking the beginning and the end – we claim – of the recent hiatus. Essentially, our model suggests that cooling after 1997-98 offset and temporarily masked warming from anthropogenic and other causes until 2015-16.

Looking forward, the referee is correct that we did not predict the ENSO and therefore conditioned our temperature forecasts on scenarios with no ENSO. The ENSO is not periodic enough that long-run forecasts of the ENSO would be very accurate and its estimated effect on temperature is not as large as that from the OMO. Put differently, we believe that the loss in forecast accuracy from conditioning our temperature forecasts on those of the ENSO would exceed the benefit from doing so. However, as a very crude forecast, we may expect a super El Niño again in about 2034, which could break up the hiatus predicted by the OMO.

Author Action: **Added** exposition similar to that above on the decadal scale to page 6 (lines 32-35). **Added** exposition similar to that above on prediction to page 9 (lines 1-2, 20-22, and 30-32).

Referee Comment: (4) The authors discuss the temperature Hiatus in the future using the Sin extrapolation of OMO. As mentioned above, is there any uncertainty for the practice? Moreover, the contribution of the temperature Hiatus (that is, the oscillatory variations of temperature) to the whole temperature variability (especially more significant increase) in the future should be clarified, as it is more important for the policy-making, as discussed in Conclusions. Besides, the regional difference about the results can be simply discussed.

Author Response: Every forecast comes with uncertainty. We have taken into account uncertainty in the historical data and uncertainty in the parameter estimates in generating our interval forecasts for the start and end of the next hiatus. We explain our procedure in the SOM.

We estimate the variation in temperature from the OMO to be $0.26°$C $(0.25, 0.28)°$C. At its predicted nadir in 2061, temperatures are predicted have increased since 2023 by $0.11°$C $(0.11, 0.11)°$C under RCP6.0 or $0.50°$C $(0.49, 0.51)°$C under RCP8.5, meaning that they would have increased by $0.37°$C under RCP6.0 or $0.76°$C under RCP8.5 without the OMO. Based on the point estimates, we expect the variation of the OMO to

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mask the underlying warming trend by 34% under RCP8.5 and 70% under RCP6.0. It is this 70% that we expect to result in an apparent hiatus over this period.

Even though our model makes use of spatially disaggregated sea surface temperatures, our results have nothing to say directly about regional differences in the effects of the oscillations. The referee is certainly correct that the regional differences are important for policymakers, so it seems appropriate to speculate on these effects, which we do in the revision.

Author Action: **Added** exposition similar to that above on variations to page 9 (line 33) through page 10 (line 2). **Added** exposition speculating on regional differences to page 11 (lines 19-24).

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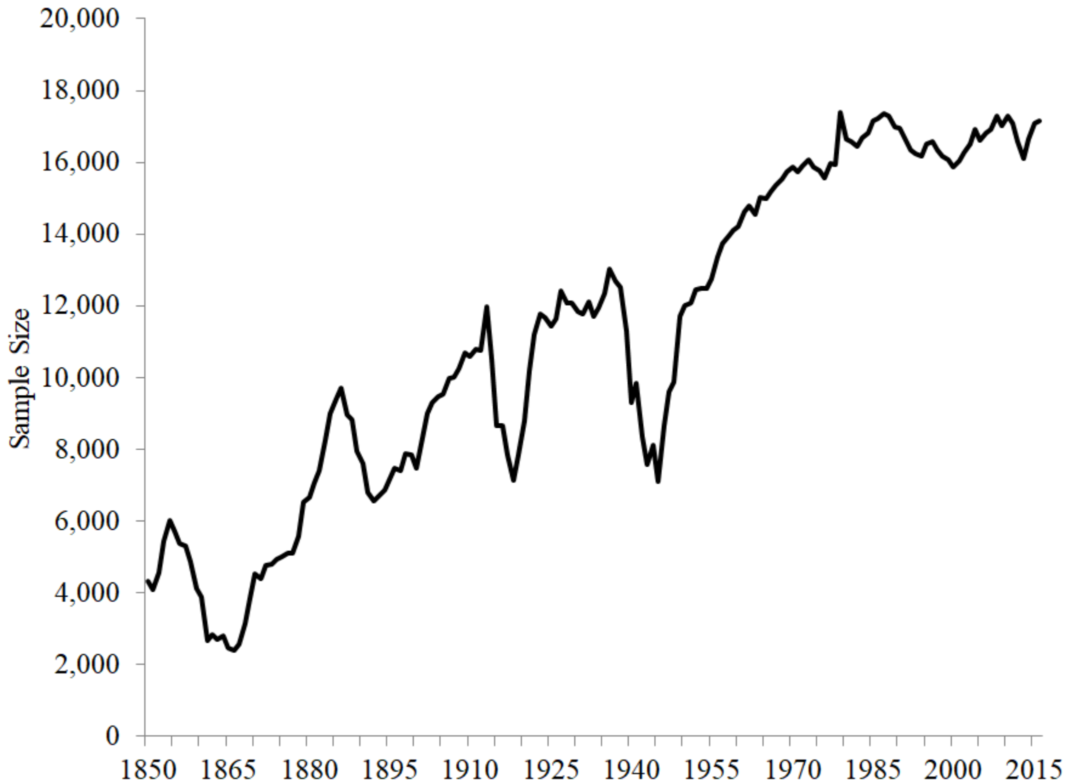

Fig. 1.

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