July 10, 2019

Earth System Dynamics
Fubao Sun, Handling Editor

Dear Dr. Sun:

Please consider the revised manuscript, entitled “Dating Hiatuses: A Statistical Model of the Recent Slowdown in Global Warming -- and the Next One” (with Kyungsik Nam), for publication in Earth System Dynamics.

We have carefully and thoughtfully addressed the reviewers’ comments, which we found to be constructive and certainly helped us to improve the draft, in the response and in the manuscript. A response to each reviewer follows this cover letter, followed by latexdiff versions of both the manuscript and SOM.

My coauthor and I look forward to hearing from you in due course and are happy to make further revisions if useful.

Sincerely,

J. Isaac Miller
Associate Professor
Response to Anonymous Referee #1

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.

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 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 $\alpha_0$ and regression error $\eta_t$ 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 $\theta_4$ in equation (S.1) of the SOM, which we estimate to be nearly zero (Table S.1). Since we use $\theta_4$ to build the cyclical component, any bias in our estimate of $\theta_4$ is picked by the intercept $\alpha_0$ 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 $0.26^\circ C$ ($0.25, 0.28)^\circ C$. At its predicted nadir in 2061, temperatures are predicted have increased since 2023 by $0.11^\circ C$ ($0.11, 0.11)^\circ C$ under RCP6.0 or $0.50^\circ C$ ($0.49, 0.51)^\circ C$ under RCP8.5, meaning that they would have increased by $0.37^\circ C$ under RCP6.0 or $0.76^\circ C$ under RCP8.5 without the OMO. Based on the point estimates, we expect the variation of the OMO to 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).
Response to Anonymous Referee #2

Referee Comment: Global warming has long attracted the attention of the climate research community, but also socioeconomic fields, for its expected huge impacts on the Earth’s climate and our living environments. To date, we have not yet sufficiently understood the physical mechanisms accounting for the causality between warming and anthropogenic and natural processes. Although the existing studies using numerical models have provided important information for understanding global warming and climate change, the known inadequacy, uncertainties, and biases in models make us today still not clearly understand warming mechanisms by model alone. This study used a semiparametric statistical regression model, and proposed a new oceanic multidecadal oscillation index measuring the multibasin contribution to global mean temperature, to date and attribute the temperature hiatus from the perspective of physical processes and statistical features. The approaches and results are helpful to further our knowledge of the warming temperature oscillations and climate change. As is known, the biggest disagreement with hiatus comes from data uncertainty. So the usefulness of this study lies in the worthy addition to our study approaches and thinking perspectives for global warming. Regarding the projections, the credibility is not enough to support our policy-making, instead add risks thereof. So the suggestion is that authors limit the implications of this study in the range of study methodology and perspective, and include a caveat of uncertainty in the projection into the conclusion section.

Author Response: Thank you for your careful reading of the manuscript and suggestions for improvement. We have done our best to take into account uncertainty in the underlying data and in estimation of the model parameters, but projections are of course necessarily uncertain and we now include such a caveat. In the revision, we now emphasize a usefulness in the evaluation of policy: the natural cycle that we estimate can have a confounding influence on mitigation, so our results may be taken as a warning to those conducting quasi-experimental evaluations of such policies.

Author Actions: Clarified the use to inform policy throughout. Added to the conclusion: “We emphasize, however, the inherent uncertainty in such an exercise, even taking into account our allowance for uncertainty in the data and estimates.” Added to the conclusions: “Our forecasts are conditional on hypothetical concentration pathways. We cannot and do not suggest that policy should be based on our results. Rather, we seek to inform scientists and policymakers of the possibility of a warming hiatus due to a natural cycle. Such a cycle may be expected to have a confounding effect on policy evaluation, because a natural downturn may be mistaken for the effectiveness of mitigation. Quasi-experimental statistical evaluation of such policies must take into account this effect to avoid mistaking a failed policy for a successful one.”
Dating Hiatuses: A Statistical Model of the Recent Slowdown in Global Warming – and the Next One

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\textbf{Abstract.} Much has been written about the so-called hiatus or pause in global warming, also known as the stasis period, the start of which is typically dated to 1998. HadCRUT4 global mean temperatures slightly decreased over 1998-2013, though a simple statistical model predicts that they should have grown by $0.016^\circ$C/yr, in proportion to the increases in concentrations of well-mixed greenhouse gases and ozone. We employ a statistical approach to assess the contributions of model forcings and natural variability to the hiatus. We find our point estimates suggest that none of the model forcings explain more than a fifth of the missing heat and that the El Niño Southern Oscillation (ENSO) explains at least half and possibly more than four fifths no more than a quarter of the missing heat. Two thirds and possibly up to 84\% is explained by the unusually high temperature of 1998. Looking forward, the simple model also fails to explain the large increases since then ($0.087^\circ$C/year over 2013-2016). This period coincides with another El Niño, but the ENSO fails to satisfactorily account for the increase. We propose instead a semiparametric cointegrating statistical model that augments an energy balance model with a novel multibasin measure of the oceans’ multidecadal temperatures cycles. The model partially explains the recent slowdown and explains nearly all of the subsequent warming. The natural cycle suggests the possibility of a much longer hiatus over roughly 2023-2061, with rather important policy implications potentially important implications for policy evaluation.

1 Introduction

There is a well-established physical and statistical link between temperatures and anthropogenic and natural climate forcings. A simple linear cointegrating regression of the HadCRUT4 global mean temperature anomaly (GMT) onto the radiative forcings given by Hansen \textit{et al.} (2017) correlates 88\% of the variation in mean temperature with variations in these forcings. Constraining all but volcanic forcings to have a common coefficient in the regression explains 84\%.

Over the period of 1998-2013, the second regression, estimated using a canonical cointegrating regression, predicts an increase of $0.241^\circ$C or $0.016^\circ$C/yr on average, in proportion to the increase in well-mixed greenhouse gases (WMGHGs) and ozone over this period. Instead, observed GMT slightly decreased by $0.024^\circ$C or $0.002^\circ$C/yr on average, earning this period the nicknames of the “stasis period,” or “hiatus” or “pause” in global warming. The difference, measured in this way as $0.265^\circ$C or $0.018^\circ$C/yr, is the so-called “missing heat” of the hiatus, which is quite substantial in the context of the aggregate temperature increase since the pre-industrial era of $0.85^\circ$C (IPCC, 2013), is quite substantial. In contrast, since 2013 (through 2016), temperatures have increased by $0.087^\circ$C/yr, much faster than this simple statistical model predicts.
Our notion of hiatus is roughly consistent with that of Meehl et al. (2011), Kosaka and Xie (2013), and Drijfhout et al. (2014), who reference the apparent hiatus in global warming with respect to heat flux from greenhouse gases or model forcings more generally. Instead, some authors refer to the hiatus with respect to temperature changes or a trend over an earlier period (Schmidt et al., 2014; Karl et al., 2015; Yao et al., 2016; and Medhaug et al., 2017), while some authors refer to the hiatus without any explicit baseline.

Linking missing heat to contemporaneous model forcings is physically appealing, and our empirical evidence suggests that our measure of missing heat comes from a cointegrating regression,1 so the approach is statistically appealing, too. Note that a slowdown or hiatus in global warming as we have defined it does not require a similar slowdown in forcings. On the contrary, such a hiatus is defined in spite of continuing increases in WMGHG concentrations.

What caused this hiatus? Various studies attribute it to one or more of (a) natural variability of the ocean cycles, particularly the Atlantic Multidecadal Oscillation (AMO), the Pacific Decadal Oscillation, and the El Niño Southern Oscillation (ENSO) (Kosaka and Xie, 2013; Steinman et al., 2015; Yao et al., 2016); (b) cooling from stratospheric aerosols released by volcanic activity (Vernier et al., 2011; Neely et al., 2013); (c) variability of the solar cycle (Huber and Knutti, 2014); (d) a change in the oceans’ heat uptake and a weakening of the thermohaline circulation, particularly the Atlantic Meridional Overturning Circulation (Meehl et al., 2011; Drijfhout et al., 2014; Chen and Tung, 2014, 2016); (e) increased anthropogenic emissions of sulfur dioxide from bringing online a large number of coal-burning power plants in China (Kaufmann et al., 2011); and (f) coverage bias or poor data more generally (Cowtan and Way, 2014; Karl et al., 2015). Schmidt et al. (2014), Pretis et al. (2015), and Medhaug et al. (2017) emphasize the need to account for multiple explanations for the hiatus.

We propose a new method to measure the oceans’ aggregate multidecadal cycle, which we call the Oceanic Multidecadal Oscillation (OMO), recognizing the possibility of heterogeneous long-run effects of anthropogenic forcings on ocean basins and allowing for a multibasin contribution to global mean temperatures, in the spirit of Drijfhout et al. (2014) and Wyatt and Curry (2014). The method allows an improvement over the linear detrending method of Enfield et al. (2001) or a single ocean approach such as the AMO signal estimated by Trenberth and Shea (2006). Not only do we estimate the mean OMO for the globe, but we estimate a global distribution representing the contribution of the OMO to spatially disaggregated sea surface anomalies.

We utilize a semiparametric cointegration statistical approach (Park et al., 2010), with well-known widely used and publicly available data sets to estimate an energy balance model (EBM) similar to the widely used well-known model of North (1975) and North and Cahalan (1981). The estimated OMO enters the model nonparametrically, as does the quasi-periodic southern oscillation index (SOI), a common proxy for the ENSO. However, information criteria select a linear specification for the latter.

We find that the solar cycle and multidecadal ocean cycle have warmed rather than cooled over the period 1998-2013, so they cannot account for the missing heat. Volcanoes, tropospheric aerosols & surface albedo, and ENSO account for about

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1 The regression of temperature anomalies on volcanic forcings, the sum of the remaining forcings, and an intercept yields a covariance stationary residual series. Augmented Dickey-Fuller tests with lag lengths up to four reject the null of no cointegration. The GPH estimator suggests the possibility that the residual series is stationary with long memory, such that the memory parameter estimated between 0.44 and 0.49, which also supports (fractional) cointegration.
1%, 19%, and 23% respectively of the missing heat of the hiatus. An even simpler explanation – that the hiatus is defined by starting in an unusually warm year, even taking into account El Niño – explains more than half—about two thirds of the missing heat, a result that echoes previous authors (Medhaug et al., 2017, e.g.). A model that takes into account all but the residual just mentioned explains about 43% of the hiatus.

Roberts et al. (2015) speculate that the hiatus could last through the end of the decade, and Chen and Tung (2014) and Knutson et al. (2016) make stronger statements about its continuation. If so, then the unusually warm years of 2015-2016 are outliers and global temperatures can be expected to stabilize or cool in the next decade. On the contrary, our proposed model explains nearly all of the more recent record warm years, overshooting the record high anomaly of 0.773°C in 2016 by only 0.021°C. This result provides conclusive statistical evidence that the hiatus is over. In other words, we date the end of the recent hiatus prior to 2015.

Can we expect a future hiatus or slowdown? If so, when? We find that the two most influential non-seasonal drivers of global aggregate temperature change are the long-run contribution of WMGHGs and the fairly predictable OMO with a period of 76 years, consistent with the 65-80 year period estimated for the AMO in the literature (Knight et al., 2005; Trenberth and Shea, 2006; Keenlyside et al., 2008; Gulev et al., 2013; Wyatt and Curry, 2014). Although the OMO cannot explain the recent hiatus, it can explain past multidecadal cooling or hiatus periods, such as the decades following the temperature spikes in about 1877 and 1943.

If we condition the model on future forcings with growth rates similar to RCP8.5, we can expect temperatures to increase with a possible slowdown but without any future hiatus. However, if we more realistically condition on future forcings with the same average annual rate as that of the past 76 years, similarly to RCP6.0, we expect a multidecadal hiatus over approximately 2023-2061. Note that our finding of a warm period separating the previous slowdown episode with from the next one is exactly consistent with the recent projection of a warming period warming from 2018-2022 by Sévellec and Drijfhout (2018) using a different model and method.

2 Empirical Results

We utilize forcings data over 1850-2016 from Hansen et al. (2017).2 We create two radiative forcing series: the sum of forcings from well-mixed greenhouse gases (primarily CO₂, CH₄, N₂O, and CFCs), ozone, tropospheric aerosols & surface albedo, and solar irradiance, denoted by \( h_1 \), and that from volcanoes, denoted by \( h_2 \). Shindell (2014) suggests the possibility that forcings due to aerosols and ozone may have effects that are different from those of WMGHGs. By aggregating all non-volcanic forcings into \( h_1 \), we are instead following Estrada et al. (2013), Pretis (2015), inter alia. A Wald test shows no statistically significant difference (\( p \) value of 0.31) between models with and without the restriction imposed.3

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3The test is executed as \( qF \), where \( q = 3 \) is the number of restrictions tested and \( F \) is the F-test of these restrictions based on Cochrane-Orcutt transformed regressions to accommodate an AR(1) error consistent with the bootstrapping strategy discussed below.
Some authors, such as Estrada et al. (2013), ignore volcanoes in statistical estimation of EBMs. Leaving out volcanoes is statistically justified by the apparent uncorrelatedness of this series with the other forcings. Relegating that series to the error term may affect statistical uncertainty, but should not bias the estimates of the effect of \( h_1 \). Because one of our goals is to assess the impact of volcanic activity on the slowdown, we include volcanoes. However, we allow for a separate coefficient \( h_2 \), in order to accommodate the suggestion of Lindzen and Giannitsis (1998) of a smaller sensitivity parameter for physical models that include volcanic forcing.

We use HadCRUT4 and HadSST3 temperature anomaly data, measured relative to 1961-1990, from Morice et al. (2012) and Kennedy et al. (2011a,b) respectively.\(^4\) In order to estimate the distribution of the OMO, monthly HadSST3 data observed over 5° latitude by 5° longitude are pooled into years over 1850-2016 (167 years of data). The HadCRUT4 data set combines HadSST3 for sea and CRUTEM4 for land, so the temperatures from HadCRUT4 and HadSST3 are comparable. However, using only HadSST3 for the distribution ensures that grid boxes containing both land and ocean stations will contain only ocean measurements in the distribution.

The SOM (Supplementary Online Material) contains a detailed description of the methodology used to estimate the distribution of the OMO. Specifically, \( f(r) \) is the probability density function of the OMO over the globe’s oceans estimated with a support \([r^-, r^+]\). Estimation of the OMO omits both long-run temporal temperature trends to avoid cointegration with \( h_t \) and \( \delta \)–idiosyncratic noise to avoid over-fitting very short-run fluctuations in GMT by using sea-surface temperatures.

We base our statistical model on an EBM given by

\[
T^a = h^* \alpha + \int_{r^-}^{r^+} b(r) f(r) dr + c(S) + \eta,
\]

where \( T^a = \frac{1}{\Delta x} \sum_{x} T(x) \) is the global mean temperature anomaly (GMT), \( h^* = (1, h_1, h_2)' \) is global forcing, \( S \) is the Southern Oscillation Index (SOI, Ropelewski and Jones, 1987) used to proxy ENSO quasi-periodic cycles.\(^5\) \( \alpha = (\alpha_0, \alpha_1, \alpha_2)' \) is a coefficient vector, \( \eta \) is an error term.

A detailed derivation of the EBM from a more familiar EBM similar to those of North (1975), North and Cahalan (1981), North et al. (1981), inter alia is provided in the SOM. A primary intuition for the derivation is that \( \text{we the nonlinear functions } b(r) \text{ and } c(S) \) allow the oceans’ heat uptake to vary over multidecadal and interannual oscillations, and \( \text{the nonlinear functions } b(r) \text{ and } c(S) \) allow for these variations.

In order to estimate the EBM in (1) nonparametrically in \( b(r) \) and \( c(S) \), we attach time subscripts and write

\[
T_{at}^a = h^* \alpha + x_t' \gamma + w_t' \delta + \eta_t,
\]

where \( x_t = (x_{1t}, ..., x_{mT})' = \int_{r^-}^{r^+} b_{1:mT}(r) f_t(r) dr \) with \( b_{1:mT}(r) = (b_1, ..., b_{mT})' \) and \( w_t = (w_{1t}, ..., w_{mS})' = c_{1:mS}(S_t) \) with \( c_{1:mS}(S_t) = (c_1(S_t), ..., c_{mS}(S_t))' \), finite-order series approximation to \( \int_{r^-}^{r^+} b(r) f_t(r) dr \) and \( c(S_t) \), with \( m_S \) and \( m_T \) –vector

\(^4\) Ensemble median of HadCRUT4.4.5.0.0 (annual unsmoothed globally averaged) and HadSST3.1.1.0 (monthly globally disaggregated) downloaded from www.metoffice.gov.uk/hadobs on July 18, 2017 and April 4, 2017 respectively.

-vectors of coefficients given by $\gamma$ and $\delta$ respectively. The error term $\eta_t$ contains both (serially correlated) stochastic forcings, along the lines of North et al. (1981), and any approximation errors from the series expansions.

2.1 The 1998-2013 Episode

The missing heat of the recent hiatus is defined above by the difference between the actual GMT in 2013 and the temperature predicted from increases in WMGHG and Ozone (G+Z, hereafter) alone using the restricted model with $\gamma, \delta = 0$ and starting in 1998. The GMT in 1998 was $0.536^\circ$C. Fixing 1998 as the starting year and based on an increase of climate forcings from G+Z of 0.561 W/m$^2$ over 1998-2013, the model predicts a GMT of $0.536 + 0.561 \times 0.430 \approx 0.777^\circ$C in 2013, with a confidence interval of $(0.750, 0.804)^\circ$C.\(^6\) In contrast, the observed GMT is $0.512^\circ$C in 2013, so that the difference, $0.777 - 0.512 \approx 0.265^\circ$C, represents the missing heat. The 1998-2013 episode is illustrated by the missing heat in Figure 1.

One way to try to explain the missing heat is to “turn on” some of the other forcings in the model. To that end, we estimate the model in (2) with both $\gamma, \delta \neq 0$ (unrestricted) and $\gamma, \delta = 0$ (restricted). Least squares is expected to be consistent, but we use the canonical cointegrating regression approach of Park et al. (2010), in order to estimate the coefficients asymptotically normally and the standard errors consistently for cointegrated temperatures and forcings. As explained in the Supplementary Online Material (SOM), this procedure also corrects for uncertainty in the forcings data. A number of previous studies have provided physical and statistical evidence in favor of a cointegrating relationship: Kaufmann et al. (2006a, 2006b, 2010, 2013), Pretis (2015), inter alia. As explained in the SOM, this procedure also corrects for uncertainty in the forcings data.

Adding only volcanoes to G+Z decreases forcings by 0.036 W/m$^2$. Predicted GMT decreases by only 0.003\(^\circ\)C (0.001, 0.005)\(^\circ\)C or about 1.1% (0.3, 1.9)% of (the point estimate of) the missing heat. In the data, 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 attribute an increase in stratospheric aerosols to emissions. 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 has been relatively small.

Similarly, adding only tropospheric aerosols & surface albedo to G+Z decreases forcings by 0.118 W/m$^2$. Predicted GMT decreases by 0.051\(^\circ\)C (0.005, 0.097)\(^\circ\)C, or 19.1% (1.7, 36.5)% of the missing heat. That these two forcings, both attributable to aerosols, anthropogenic aerosol emissions appear to explain some of the missing heat is consistent with the findings of Storelvmo et al. (2016). These results suggest that In spite of the uncertainty in measuring forcings from anthropogenic aerosol emissions, our results suggest that these may play a larger role than natural volcanic emissions, which is more consistent with the findings of Kaufmann et al. (2011) than those of Vernier et al. (2011). In any case, our results suggest that neither of these two explanations explain very much of the missing heat.

\(^6\)The intervals throughout the paper are given with 90% confidence, in keeping with those for the forcings given by the IPCC (Myhre et al., 2013). The intervals reflect not only statistical uncertainty from the regression error but also uncertainty in the underlying data. Details of the construction of these intervals are given in the SOM.
Figure 1. A Visual Anatomy of the 1998-2013 Episode. The hiatus is defined by the missing heat in 2013 relative to that predicted by increases in WMGHG and ozone forcings since 1998. Plots crossing the missing heat help to explain it (pictured: tropospheric aerosols & surface albedo, ENSO, and the comprehensive model), while those passing above the missing heat exacerbate it (pictured: solar irradiance).

Solar irradiance increases forcings by 0.037 W/m², so that the predicted GMT increases by 0.016°C (0.012, 0.019)°C, exacerbating the missing heat by 6.0% (4.7, 7.3)%.

There is a decline from 2002-2006, but the net effect of solar over 1998-2013 is to increase temperature – not to decrease it. To the extent that solar contributed to the slowdown by decreasing temperatures, the results suggest that solar alone cannot be sufficient. This finding is not inconsistent with that of Schmidt et al. (2014), who examine solar in conjunction with other forcings as an explanation.

The preceding explanations are model forcings, and none of them satisfactorily account for the slowdown either alone or in concert. As previous authors have pointed out, natural variability may play a role, and we now turn to measures of two such types: the OMO and the ENSO.

In order to examine multidecadal oscillations from the OMO as a possible explanation, we let $\gamma \neq 0$, but keep $\delta = 0$. The regressor vector $x_t$ is correlated with the other forcings, and we want to capture the partial effect of the OMO while retaining the total effect of G+Z. In order to do so, we employ the fitted residuals from regressing $x_t$ onto the other forcings as a regressor
in the model, rather than using \( x_t \) itself. The two approaches – using \( x_t \) or its fitted residuals – yield equivalent model fits, but using the fitted residuals fixes the coefficient vector \( \alpha \).

These residuals increased by 0.162 and 0.161 W/m\(^2\) over 1998-2013, exacerbating. The oscillation exacerbates the missing heat by 0.110°C (0.078, 0.143)\(^\circ\) or 41.6% (29.3, 53.9)%. By itself, the fitted OMO worsens the puzzle in the sense that the predicted temperature in 2013 increases to 0.777 + 0.110 = 0.887\(^\circ\)C. The reason for the increase is that the OMO appears to be increasing rather than decreasing during this period. This result contrasts sharply with that of Yao et al. (2016), who attribute the hiatus to a much shorter oceanic cycle of 60 years.

An analogous exercise with the SOI, letting The ENSO is quasiperiodic with a period of about 5-6 years. However, 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, coinciding with the beginning and end of the 1998-2013 episode. Letting \( \delta \neq 0 \) but keeping \( \gamma = 0 \), yields an increase in the normalized and orthogonalized SOI of 1.162 and thus a decrease of 0.062°C (0.011, 0.113)\(^\circ\)C, so that the ENSO explains 23.4% (4.3, 42.5)\% of the missing heat.

All of the explanations so far ignore to some extent that the starting year matters, as has been pointed out by previous authors (Medhaug et al., 2017, e.g.). Not only was 1998 an El Niño year, it was an anomalously warm one. Suppose that the temperature anomaly in 1998 had been equal to that in 1997, 0.389°C. The same exercise of defining the hiatus using growth rates predicted by G+Z results in a 2013 temperature anomaly of 0.630°C, a decrease of 0.777 – 0.630 = 0.147°C, which explains 55.5\% of the missing heat. In other words, half of the puzzle is explained simply by the construction of the puzzle.

The counterfactual of setting the 1998 temperature to that of 1997 seems effective in explaining the slowdown, but it is extremely ad hoc. A similar result is obtained more formally by fitting the model forcings but no natural variation – i.e., by estimating the model in (2) with \( \gamma, \delta = 0 \). This model predicts the temperature in 1998 to be 0.400°C – close to that in 1997 – and increasing to 0.604°C by 2013. In other words, a simple model, including all forcings but without the OMO or the ENSO, explains 0.173°C (0.124, 0.223)\(^\circ\)C, or 65.4\% (46.8, 84.0)\% – more than half – of the missing heat. Looking at the most comprehensive model with \( \delta, \gamma \neq 0 \) gives a qualitatively similar result, explaining 42.6\% (32.0, 52.4)\%(25.1, 60.2)\%.

This finding suggests that the unusually warm year of 1998 – a residual in the model – accounts for most of the apparent slowdown between 1998-2013. It is consistent with the finding of Kosaka and Xie (2013), in the sense that the El Niño year is necessarily followed by La Niña cooling. However, with the results of Pretis et al. (2015) and those using the SOI above in mind, we certainly cannot attribute the slowdown to the ENSO uniquely.

Yet there is a new problem given by the recent high GMT’s GMTs of 0.760°C in 2015 and 0.773°C in 2016. The restricted model undershoots these temperatures by more than 0.1°C. Are these again simply outliers, as 1998 is? Perhaps the first explanation that comes to mind was? A natural explanation is the ENSO, because 2015 and 2016 were El Niño years. The model with \( \delta \neq 0 \) and \( \gamma = 0 \) – i.e., with SOI but no AMO – undershoots 2015 by 0.113°C and 2016 by 0.057°C. In other words, accounting for ENSO does about as poorly as not accounting for ENSO in predicting the temperature in 2015, but improves the prediction for 2016.
Finally, consider the proposed comprehensive model with \( \gamma \neq 0 \). The model undershoots 2015 by only 0.044°C, while overshooting 2016 by only 0.021°C (see Figure 1). We interpret these numbers to mean that the recent high temperatures of 2015 and 2016 are attributable more to the smooth, multidecadal, and somewhat predictable OMO than to the higher-frequency quasi-periodic ENSO. As a result, we can say that 2015 and 2016 were not outliers, and that increases in global mean temperatures may be expected to continue as the OMO continues to put upward pressure on temperatures. Put more simply, the hiatus that appeared to begin in 1998 ended in 2013.

### 2.2 The 2023-2061 Episode

Wyatt and Curry (2014) emphasize that, although evidence supports a secularly varying oscillation like the one that we estimate, future external forcings may alter the amplitude and period of the cycle. Linear detrending may overemphasize this possibility by giving a stochastically trending series with secular oscillations the appearance of a secularly trending series with quasi-periodic or stochastic oscillations. If the long-run trend is indeed anthropogenic, the former is more appropriate than the latter.

We fit a sine function and predict it to 2100, as shown in Figure 2. After crossing zero before 2005, the sine function continues to increase for roughly \( 76/4 = 19 \) years until about 2023, and it then decreases for about 38 years until roughly 2061. Figure 2 shows sine functions reflecting a lower and upper 90% confidence interval for the estimated period. This interval is not a prediction interval for a future year, so the plots do not straddle that of the point forecasts. Nor is it constructed from standard errors, which do not reflect correlation of the estimates of the period and phase shift.

Rather, we rely on an AR(1) bootstrap strategy in the spirit of Poppick et al. (2017), which is described in the SOM. The 90% bootstrap confidence interval of the estimated period of 76 years is 73 to 80 years. We date the next peak in the sign function as 2023 – likely falling in the interval 2021-2028 – and the next minimum as 2061 – likely falling in the interval 2057-2068.

A downturn in the temperatures due to the ocean cycle implies a slowdown but not necessarily a hiatus in global warming, because the upward trend in forcings may more than offset the downturn. The model in (2) may be used to forecast temperature anomalies conditional on changes in one or more forcings. In our forecasts, we condition on volcanic activity remaining at its 2016 level and the SOI remaining at its temporal mean over 1850-2016. We do not forecast the ENSO, because it 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.

We consider two possible scenarios for non-volcanic forcings, which are closely related to RCP8.5 and RCP6.0. The data in 2016 have already deviated from the RCPs, so we simply match the cumulative growth rates of all forcings CO\(_2\) equivalents of all anthropogenic forcings under the two scenarios starting in 2016. RCP8.5 is considered by many to be “business-as-usual,” and the average annual growth implied by RCP8.5 is 0.054 W/m\(^2\)/yr. Forcings would have to grow at a sustained rate much faster than the recent growth of 0.047 W/m\(^2\)/yr since 2013 – i.e., since the end of the hiatus and beginning of the recent El Niño period. On the other hand, RCP6.0 has an average annual growth of 0.022 W/m\(^2\)/yr, similar to 0.025 W/m\(^2\)/yr over the last 76 years – one complete period of the OMO – in order to filter out any multidecadal cyclicality in the forcings themselves.

Two points bear discussion. First, we are ignoring the recalcitrant component of warming (Held et al., 2010), nor are we using a dynamic model to try to capture short-run dynamics. As a result, our model is set up to make conditional forecasts of
roughly 5-90 years from the end of the sample. Second, forecasts are conditional on the scenarios mentioned above, but we make no attempt to forecast individual forcings, such as solar or WMGHGs.

Figure 3 shows the sample paths of the conditional forecasts under the two scenarios. Under RCP8.5, anthropogenic forcings increase so much that downturns in the OMO cycle are never again powerful enough to force a hiatus in global warming. The global temperature anomaly increases by about $0.023^{+0.022}_{-0.022}$°C/yr ($0.021, 0.026)_R^{(0.020, 0.025)}$°C/yr on average to $2.732^{+0.652}_{-2.652}$°C over the base period by 2100.

Nevertheless a slowdown is predicted until about 2061, after which point temperatures growth is predicted to accelerate to a much faster rate over multiple decades than that of the historical record. Of course, our forecasts are conditional on ENSO being unrealistically flat. A hiatus could again result from a well-timed super El Niño year, such as 1998, even under RCP8.5. We believe that the loss in forecast accuracy from conditioning our temperature forecasts on those of the SOI would exceed the benefit from doing so.
Figure 3. Conditional Forecasts of Temperature Anomalies, 2022-2100. Until 2016, dots represent actual temperature anomaly data and the solid line represents the temperature anomaly predicted by the model. Scenario labeled RCP8.5 uses RCP8.5 growth rates for anthropogenic forcings from a starting point of 2016, and similar for RCP6.0. 90% confidence bands generated using methodology described in the SOM.

Under RCP6.0, the temperature increases by about $0.01^{+0.09}_{-0.008, 0.012} \, ^\circ C/yr \in [0.007, 0.011] \, ^\circ C/yr$ on average. By 2100, temperature anomalies increase to $1.576^\circ C (1.408, 1.745) \in [1.329, 1.664] \, ^\circ C$, which is $1.882^\circ C (1.713, 2.050) \in [1.801, 1.969] \, ^\circ C$ above pre-industrial temperatures, because the base period is approximately 0.305^\circ C above pre-industrial temperatures. While this may still be below 2^\circ C, it exceeds the Paris Accord Agreement goal of 1.5^\circ C.

We see a substantial ebb and flow of the effect of the OMO cycle on temperatures under RCP6.0. Between 2023 and 2061, the dates identified of the next maximum and minimum of the OMO, temperature is predicted to grow by only 0.0001^\circ C/yr – i.e., virtually no growth. In contrast to the average annual growth of anthropogenic forcings of 0.022 W/m^2/yr under RCP6.0, this projection clearly suggests a future hiatus period that is much longer than the 1998-2013 episode. However, a crude rule-of-thumb forecast suggests the possibility of a super El Niño in approximately 2034, which could break up the hiatus predicted by the OMO.
The variation in temperatures from the OMO is estimated to be 0.26°C (0.25, 0.28)°C. At its predicted nadir in 2061, temperatures are predicted to 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 mask the underlying warming trend by 34% under RCP8.5 and 70% under RCP6.0.

3  Summary and Implications for Policy Evaluation

It is no exaggeration to say that the 1998-2013 apparent hiatus in the otherwise evident trend of warming global mean temperatures has generated controversy. From a scientific point of view, a number of researchers have put forth differing explanations backed up by plausible physical models joined with sound statistical methods. Because of the critical importance of climate change to human systems – economic, political, etc. – the controversy has spilled over into the arena of public and political debate, where the lack of warming is viewed as empirical validation by those skeptical of global warming. Lack of consensus about the cause only adds to such doubt.

In this paper, we disentangle some of the causes of the 1998-2013 hiatus and subsequent run-up in temperatures using a modern statistical technique, a semiparametric cointegrating regression, based on an energy balance model. 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. Other potential causes that we investigate had considerably less impact or else an accelerating rather than confounding impact on rising temperatures. Our statistical model not only explains much of the hiatus but also explains the rapid warming since 2013. We find that this warming marks the end of the hiatus, in contrast to some findings in the literature (Chen and Tung, 2014, Knutson et al., 2016, e.g.) but consistently with that of Sévellec and Drijfhout (2018).

Further, fitting the mean of the distribution of detrended ocean temperature anomalies (an oceanic multidecadal oscillation) to a periodic function enables us to make forecasts of the global mean temperature conditional on forcing scenarios. If these forcings grow at the same rate as they have for the past 76 years, the estimated period of the OMO, we can expect a longer hiatus in global warming from about 2023 to about 2061, roughly 3-4 decades. The controversy of the recent 15-year hiatus is a precursor to that which may result from a much longer one. Kaufmann et al. (2017) recently showed a correlation between climate skepticism and locally cooler (or less warm) temperatures in the US. If the lack of warming indeed drives doubt, three decades of no warming is indeed a long period to fuel the fires of skepticism. Nevertheless, on the current trajectory, we can expect the decades following the next hiatus to push well past the 1.5°C goal of the Paris Accord Agreement and even past 2°C.

In order to inform policymakers, it's important to note that even though our model makes use of spatially disaggregated sea surface temperatures, our results have nothing to say directly about regional differences in temperature oscillations. As emphasized by Kaufmann et al. (2017) and many other authors, the effects are spatially heterogeneous. Based on estimated warming trends displayed in
Figure S.1 of the SOM, we speculate that the effect of a future hiatus will be more noticeable in the vicinity of the Pacific and Indian Oceans than in the vicinity of the Atlantic Ocean, because the latter has more strongly increasing trends.

It may be useful to assign a probability to the possibility of a future multidecadal hiatus. Such a forecast would require more information and entail more uncertainty than the conditional forecasts above, because a probability distribution would be needed for future forcings. Rather than try to forecast forcings, one could base such a forecast on, say, expert opinion of the likelihood of forcing scenarios. Suppose, for example, that a policymaker believes that forcings will increase at an average rate of \( w \) per year, where \( w \) is a random variable symmetrically distributed around RCP6.0. Figure 3 suggests that, roughly speaking, scenarios with weaker growth will result in a future hiatus, while those with stronger growth will not. Ignoring the uncertainty associated with the conditional forecasts, a policymaker with such a prior could make a prediction that a multidecadal hiatus will occur with a probability of roughly 50%. We emphasize, however, the inherent uncertainty in such an exercise, even taking into account our allowance for uncertainty in the data and estimates.

Our forecasts are conditional on hypothetical concentration pathways. We cannot and do not suggest that policy should be based on our results. Rather, we seek to inform scientists and policymakers of the possibility of a warming hiatus due to a natural cycle. Such a cycle may be expected to have a confounding effect on policy evaluation, because a natural downturn may be mistaken for the effectiveness of mitigation. Quasi-experimental statistical evaluation of such policies must take into account this effect to avoid mistaking a failed policy for a successful one.

Code and data availability. Data, code, and appendices are available as Supplementary Online Material.

Competing interests. The authors declare no competing interests.

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References


Supplementary Online Materials for
“Dating Hiatuses: A Statistical Model of the Recent Slowdown in
Global Warming – and the Next One”

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S.1 Updating the Forcing Series

We update the forcing series of Hansen et al. (2017) to 2016 as follows. We regress the first three series, CO₂, CH₄, N₂O, (in W/m²) onto the natural log of the series given by NOAA¹ in ppm for carbon dioxide and in ppb for methane and nitrous oxide. We then predict the 2016 forcings using the natural log of the 2016 NOAA data.

In contrast, forcing from CFCs is changing very slowly, so we set 2016 to be the average of 2004-2015. We take the same moving average approach to estimate the 2016 forcings for ozone, tropospheric aerosols & surface albedo, and volcanoes as for CFCs. Volcanic activity over this period was not trivial, as noted by Vernier et al. (2011) and Neely et al. (2013), but neither are there any major eruptions on the order of Mount Pinatubo in 1991. Finally, solar data in 2016 is updated by imposing the 2015-16 percent change from NASA.²

S.2 Estimating the Oceanic Multidecadal Oscillation

Ocean cycles in mean temperature data – and the AMO in particular – have been estimated a number of ways in the literature. A key problem in estimating the cycle is removing the long-run trend due to global climate change. A common method for this purpose is linear detrending of GMT (Enfield et al., 2001; Wyatt and Curry, 2014), although linear detrending has been criticized for this purpose by the IPCC (Bindoff et al., 2013). Approaches using stochastic trends include those of Trenberth and Shea (2006), who use temperatures in other oceans to detrend the Atlantic, and Lenton et al. (2017), who use global mean temperature to detrend regions in the Atlantic and Pacific.

Another problem that we must avoid is over-fitting the statistical model in equation (2) in the paper. As an example of over-fitting, consider 2016, which was an unusually warm El Niño year. A variable constructed by simply detrending sea surface temperatures would have a particularly high value for 2016. Regressing GMT onto detrended GMT or sea surface temperatures

¹Downloaded from www.columbia.edu/~mhs119/GHGs on May 15, 2017.
²Downloaded from solarscience.msfc.nasa.gov on May 15, 2017.
Figure S.1. Heterogeneous Oceanic Temperature Trends. Fitted anomalies from regressing average temperature anomalies of five ocean regions onto a constant and WMGHGs.

would show a superficially good fit, in the sense that the model could not distinguish between secular cyclical variability and idiosyncratic noise.

Our approach handles these two problems by filtering out both long-run and short-run information from the time series of temperature anomaly distributions. We decompose the temperature anomaly $T_a$ (see Section C) into a long-run trend component $T^\tau$, a stationary multidecadally oscillating component $T^s$, and a noise component $T^n$, so that $T_a = T^\tau + T^s + T^n$. Similar to the AMO, we refer to $T^s$ as the Oceanic Multidecadal Oscillation (OMO).

In order to estimate $T^s$, we first divide up the HadSST3 data into oceans: North Atlantic, South Atlantic, North Pacific, South Pacific, Indian, defined according to NOAA. We then calculate the mean for each ocean at each year and then detrend those means. Rather than using a linear time trend or using the trends from other oceans, we regress these means into a constant and WMGHGs, reflecting models in the literature, like that in our equation (2), which assume a long-run relationship between temperature and WMGHGs. Doing so for each ocean separately allows for heterogeneous local climate sensitivity – i.e., for the sea surface temperatures in different oceans to be influenced by WMGHG differently over the long term.

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Figure S.2. **Unsmoothed and Smoothed Anomaly Distributions Over Time.** Top: Unsmoothed anomaly distribution from heterogeneously detrended sea surface temperature anomalies. Bottom: Smoothed anomaly distribution from fitting the unsmoothed distribution. **Number of Observations per Year from HadSST Data.** Number of observation available to a sine function used to represent the OMO estimate annual probability density functions.

Figure S.1 shows the predicted temperature trends from these regressions. Note that the Pacific and Indian Oceans share a common warming trend, while those of the Atlantic are quite a bit different. The Atlantic is warming faster than the other oceans, with the South Atlantic catching up to the Indian and Pacific from a colder starting point and the North Atlantic becoming increasingly warmer.

In order to detrend the distribution of sea surface temperatures, we simply subtract the estimated trend for each ocean from the series of temperatures in each of the 5° by 5° boxes in that ocean. The maximum number of observations in the HadSST3 data is $36 \times 72 = 2,592$ per month and $2,592 \times 12 = 31,104$ per year. Figure S.2 shows the actual number available for each year of our sample. A maximum of 17,391 is attained in 1979. 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.

A standard nonparametric density estimation technique (Gaussian kernel with Silverman bandwidth) is used to estimate the density $f_t^{s+n}(r)$ of heterogeneously detrended sea surface temperature anomalies. Detrending removes $T^+T^-$, so that $f_t^{s+n}(r)$
Figure S.3. **Unsmoothed and Smoothed Anomaly Distributions Over Time.** Top: Unsmoothed anomaly distribution from heterogeneously detrended sea surface temperature anomalies. Bottom: Smoothed anomaly distribution from fitting the unsmoothed distribution to a sine function, used to represent the OMO.
<table>
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<tr>
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<th>Trenberth-Shea</th>
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<tr>
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<td>-0.21 1.67</td>
</tr>
<tr>
<td>$\theta_4$</td>
<td>-0.03 0.08</td>
<td>-0.03 0.10</td>
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Table S.1. Periodic Function Estimation Results. Results from fitting the WMGHG/heterogeneously detrended OMO, linearly/homogeneously detrended OMO, and Trenberth-Shea (2006) AMO to the nonlinear regression in (S.1).

reflects the density of $T_s + T_n T_s + T_n$. We omit 0.5% of the outliers in each tail, which we believe is an adequate threshold to ameliorate well-known boundary problems from kernel density estimation without substantively altering the moments of the distribution. Figure S.3 (top panel) shows the density $f^{t+n}(r)$ of the stationary temperature distribution for each year.

Next, we smooth $f^{t+n}(r)$ by removing short-run noise $T_n$. To do so, we first calculate the spatial mean $\int r f^{t+n}(r) dr$, which could be referred to as the heterogeneously detrended oceanic mean temperature. We fit the result to a single sine function, estimating

$$\int r f^{t+n}_t(r) dr = \theta_1 \sin(\theta_2 (t/T) + \theta_3) + \theta_4 + e_t,$$  

(S.1)

using nonlinear least squares. We abbreviate the endpoints of the integral simply by $-$ for $r-$ and $+$ for $r+$ throughout the SOM.

Nonlinear least squares estimates a periodic function with an amplitude of $0.130^\circ$C, a vertical shift of $-0.000^\circ$C (nearly zero), a period given by $2\pi/13.75 \times T \approx 76$ years, and a phase shift given by $0.21/13.75 \times T \approx 3$ years (Table S.1). This period is roughly consistent with the Wyatt-Curry “stadium wave” with a half-period cooling regime of 31-38 years from about 1940 to about 1975. The years over the sample in which the OMO has a neutral effect – neither cooling nor warming – are (approximately) 1852, 1928, and 2004.

As a comparison, Table S.1 and Figure S.4 compare the OMO and periodic function estimated in this manner with an OMO and periodic function estimated using linear detrending and with the AMO signal of Trenberth and Shea (2006)$^4$ and similarly estimated periodic function. The linear detrending method estimates a shorter period of 72 years, while the Trenberth-Shea AMO signal has a longer period of 78 years. Although the AMO signal has a longer period, the phase shift is negative, so that the next peak occurs just after the end of the sample. In contrast, the linear detrending method shows a peak in about 2011 – in stark contrast to the recent high temperatures in 2015 and 2016.

Now, in order to estimate the distribution of $T_s - T_n$ from that of $T_s + T_n T_s + T_n$, we construct a distribution that is changing only in mean over time. To that end, we first create a measure of the average distribution $f^n(r)$ of “de-cycled”

\footnote{Downloaded from www.cgd.ucar.edu/cas/catalog/climind/AMO.html on July 17, 2017.}
Figure S.4. Mean and Fitted Oscillations. Mean and fitted oscillations using a single linear trend, using heterogeneous WMGHG detrending, and using data from Trenberth and Shea (2006).

anomalies $\mathcal{T}_n-T_{\text{m}}$ with trend and periodic function removed. The density $f_{t}^{s+n}(r)$ is already detrended, but in order to remove the multidecadal cycle, we change the support by subtracting the heterogeneously detrended oceanic mean temperature $\mathcal{T}_n-\hat{\theta}_1 \sin(\hat{\theta}_2 (t/T_n) + \hat{\theta}_3) - \hat{\theta}_4$ estimated from equation (S.1) from each temperature anomaly $T_n$ in each year. For example, the density function estimated for a temperature anomaly of $1^\circ$C in year $t$ becomes the “de-cycled” density function estimate at $(1-\mathcal{T}_n)^\circ(1-T_{\text{m}})^\circ$C and, most importantly, the density at $\mathcal{T}_n^\circ(T_{\text{m}})^\circ$C becomes $0^\circ$C. We then average the densities at each temperature anomaly $r$ across the sample, 1850-2016, obtaining a “de-cycled” density $f^n(r)$.

Finally, we create a series of estimated densities $f_t(r)$, smoothed versions of $f_t^{s+n}(r)$. In order to do so, we reverse the procedure described above by adding the heterogeneously detrended oceanic mean temperature $\mathcal{T}_n-T_{\text{m}}$ to $f^n(r)$ in order change the support back to $r$. We then remove outlying anomalies outside the original support. In this way, we estimate the smoothed density $f_t(r)$ of the OMO, displayed in the bottom panel of Figure S.3. The methodology clearly extracts a density that (a) appears to be stationary, devoid of a long-run stochastic or deterministic trend from warming, (b) appears to be smooth, devoid of idiosyncratic noise, and (c) appears to capture the multidecadal cycle.

Disaggregated historical temperature data are measured with bias and uncertainty. By design, our method for smoothing the cyclical component eliminates 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 $\theta_1$ in equation (S.1), which we estimate to be nearly zero (Table S.1). Since we use $\theta_1$ to build the cyclical component, any bias in our estimate of $\theta_1$ is picked up by the intercept $\alpha_0$ 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, modeling it would require a more involved strategy, which we leave for future research.

5 S.3 Energy Balance Model

We define location $\ell \in G = L \cup O$ where the set $G$ is all locations on the globe and $L$ and $O$ are sets of land and ocean locations. Location $\ell$ may be given as a latitude-longitude pair, in which case the integrals over $\ell$ below become double integrals over latitude and longitude. Letting the index $j = G, L, O$, $n_j = \int_j d\mu_\ell$ with counting measure $\mu$ denotes the number of locations in each set.

Adapting the energy balance model (EBM) of North (1975) and North and Cahalan (1981) to accommodate external forcing, we may write

$$C(\ell) dT_t(\ell) = QS(\ell) \alpha(\ell) - (A + BT_t(\ell)) + D(\ell, T_t(\ell)) + h_t(\ell) + \varepsilon_t(\ell), \quad (S.2)$$

where $C$ is heat capacity, $T_t$ is temperature at time $t$, $Q$ is the solar constant, $S$ is solar irradiance, $a$ is co-albedo, $A + BT_t$ is emitted energy, $D$ is a linear-in-temperature approximation to the heat diffusion term in their model, $h_t$ is radiative forcing, and $\varepsilon_t$ is stochastic forcing. The stochastic forcing term is assumed by North et al. (1981) to be idiosyncratic, but we do not require this assumption.\(^5\)

Historical temperature data sets typically express temperature in terms of anomalies from a base period in order to ameliorate well-known measurement errors. Accordingly, we decompose temperature as temperature during a base period in order to ameliorate well-known measurement errors. Accordingly, we decompose temperature during a base period $b$ plus the temperature anomaly, $T_t = T_b + T_t^a$. The two forcing components may also be decomposed into base plus anomaly, expressed as $h_t = h_b + h_t^a$ and $\varepsilon_t = \varepsilon_b + \varepsilon_t^a$. Adapting the EBM in (S.2) to accommodate temperature anomalies allows

$$(C + B)T_t^a - CT_{t-1}^a = D(\ell, T_t^a) + h_t^a + \varepsilon_t^a, \quad (S.3)$$

by subtracting $C(\ell) dT_b(\ell) = 0$ from both sides, discretizing the derivative to a unit increment, and suppressing the location argument for now.

Because $C, B > 0$, $\pi = C/(C + B) < 1$, so that the autoregressive component may be inverted. Doing so yields

$$T_t^a = (C + B)^{-1} \sum_{i=0}^{\infty} \pi^i [D(\ell, T_{t-i}^a) + h_{t-i}^a + \varepsilon_{t-i}^a] \simeq \frac{1}{B} [D(\ell, T_t^a) + h_t^a + \varepsilon_t^a],$$

where the approximation results from a Beveridge-Nelson-type decomposition (see Phillips and Solo, 1992). The approximation is more valid when the data are cointegrated or cotrending, in which case the neglected terms have a lower asymptotic order.

\(^5\)North and Cahalan (1981) assume co-albedo to be a function of temperature as well as latitude, but more recent studies show that co-albedo is effectively constant in temperature at a given latitude (Stephens et al., 2015; Stevens and Schwartz, 2012).

\(^6\)Alexeev et al. (2005) note the effectiveness of modeling the diffusion coefficient $D$ as a function of temperature in order to capture polar amplification, which is not our aim.
What does the stochastic forcing term $\varepsilon^s_t$ represent? Aside from noisy measurement of the data, it also accounts for otherwise missing components of temperature changes. Most notably missing are natural variability, such as changes in the ocean heat uptake, and other natural cycles, such as ENSO. As proxies, we employ the OMO, given by $T^s_t(\ell)$, and the SOI often used as a proxy for ENSO, given by $S_t \ell$. Timmermann et al. (1999) note the possibility that external forcings may correlate with more frequent and/or severe ENSO cycles. By including the SOI in the model, we are implicitly assuming that the correlation is reflected in the SOI.

An alternative way to capture natural variability might be to allow for separate meridional ocean transport, along the lines of Rose and Marshall (2009), which could likely be accomplished along the lines of Pretis (2015) using a model that cointegrates surface temperature with deep ocean heat content. However, keeping in mind that our aim is to model hiatus periods that may be sparsely distributed over the historical record, the short time span over which ocean heat content is measured precludes this approach.

In such a model, surface temperatures, deep ocean heat content, and forcings share a single stochastic trend, so that the marginal value of the deep ocean heat content relative to forcings is natural variability. Hence, omitting deep ocean heat content does not cause a spurious regression, but rather relegates this stationary variability to the error term. Our proxies, the OMO and SOI, allow us to explicitly model the primary multidecadal and interannual sources of this variability.

Because the EBM does not explicitly include these indicators, it is natural to model them nonparametrically. To this end, we specify the model as

$$T_a = \alpha_0 + \alpha_1 h_t(\ell) \left[ h_t(\ell) + D(\ell, T^s_t(\ell)) \right] + \alpha_1 D(\ell, T_a(\ell)) + b(T^s_t(\ell)) + c(S_t) + \eta_t(\ell),$$

(S.4)

where

$$b(T^s_t(\ell)) = \begin{cases} \sum_{i=1}^{m_T} \gamma^T_i b_t(T^s_t(\ell)) & \text{for } \ell \in O \\ 0 & \text{for } \ell \in L \end{cases}$$

$$c(S_t) = \sum_{i=1}^{m_S} \gamma^S_i c_t(S_t)$$

are two generic series expansions intended to capture possibly nonlinear effects of these indicators. The OMO captures variability over the ocean, so this component is set to zero over land. The SOI is a single indicator. The last term $\eta_t(\ell)$ contains the original stochastic forcings and their lags and an allowance for finite-order approximation error of the two expansions. We may think of this term representing residual forcings, and it almost certainly exhibits temporal correlation.

Now we aggregate across locations to obtain a global model. To this end, let $O(T_s) = \{ \ell \in O : T_s(\ell) = T_s \}$ $O(T^s) = \{ \ell \in O : T^s(\ell) = T^s \}$ be a subset of $O$ over which the $T_s(\ell) T^s(\ell)$ has the same numerical value, let $n_{O(T_s)} = \int_{O(T_s)} d\mu_t$ denote the number of locations in $O(T_s)$ that have the value $T_s$, and let $f$ be the probability density function of $T_s(\ell)$ $T^s(\ell)$ in $O$ with support $[r^-, r^+]$. Note that

$$\int_{-}^{+} b(r) f(r) ds = \int_{-}^{+} \left[ \sum_{n=0}^{O(T_s)} n_{O(T_s)} (O(T^s))^{-1} \int_{O(T_s)} O(T^s) b(T^s_t(\ell)) d\mu_t \right] f(T^s_t(\ell)) dT^s_t = n_{O}^{-1} \int_{O} b(T^s_t(\ell)) d\mu_t.$$
which means we can aggregate all of the functions \( b \) across ocean locations or we can aggregate all of the functions \( b \) with the same observed argument and then aggregate them again with weights given by the frequency of each argument. Defining 
\[
T^n = n^{-1} \int_G T^n(\ell) d\mu, T^a = n^{-1} \int_G T^a(\ell) d\mu, h = n^{-1} \int_G h(\ell) d\mu, \eta = n^{-1} \int_G \eta(\ell) d\mu,
\]
integrating across locations, and noting that the diffusion term \( D(\ell, T^n(\ell)) D(\ell, T^a(\ell)) \) is constrained to integrate to zero by the first law yields the EBM in equation (1) in the paper.

### S.4 Estimating the Energy Balance Model

We approximate the functions \( b \) and \( c \) nonparametrically using a series of polynomial and trigonometric functions known as the flexible Fourier functional form, which Park et al. (2010) analyze using a semiparametric cointegrating regression much like ours. This form may be written as

\[
b^y_j(v) = v^j \text{ for } j = 1, \ldots, p_1
\]

\[
= \cos 2\pi k v \text{ for } j = p_1 + 2k - 1 \text{ and } k = 1, \ldots, q_1
\]

\[
= \sin 2\pi k v \text{ for } j = p_1 + 2k \text{ and } k = 1, \ldots, q_1
\]

and analogously for \( c^y_j(v) \), for \( v \in [0, 1] \). Using this notation, \( m_T = p_1 + 2q_1 \) and \( m_S = p_2 + 2q_2 \). It is important that these functions are defined over the unit interval, so let \( b_j(r) = (r^+ - r^-) b^y_j ((r - r^-)/(r^+ - r^-)) \) and \( c_j(S) = (S^+ - S^-) c^y_j ((S - S^-)/(S^+ - S^-)) \), where \( S^+ \) and \( S^- \) are the maximum and minimum observed SOI. Thus,

\[
\int b_j(r) f_t(r) dr = \int (r^+ - r^-) b^y_j ((r - r^-)/(r^+ - r^-)) f_t(r) dr
\]

holds, making estimation convenient by simply multiplying \( b^y_j \) by the range of values in the domain of the OMO.

The optimal orders \((p_1, q_1, p_2, q_2) = (2, 0, 1, 0)\) i.e., \( m_T = 2 \) and \( m_S = 1 \) are jointly determined by Schwarz-Bayesian and Hannan-Quinn information criteria evaluated using least squares with \( p_1, p_2 \) up to 3 and \( q_1, q_2 \) up to 2. With \( m_S = 1 \), SOI enters linearly and the regressor is thus simply \((S_t - S^-).\)

The estimation results are given in Table S.2.

### S.5 Estimation of Uncertainties

#### S.5.1 Uncertainties from Estimating the OMO

Estimating the OMO relies on a statistical approximation, and we employ a parametric bootstrap strategy similar to that of Poppick et al. (2017) to account for uncertainty in estimation. Specifically, after fitting the periodic function in (S.1), we fit the residuals to an AR(1) and redraw from the residuals of the fitted AR(1). We re-create the regressand using a re-created AR(1) error with fitted autoregressive parameter (0.49). We then re-estimate all the parameters of the nonlinear regression. We

---

\(^7\)All models are expected to be cointegrated, because we simply add stationary series to the model with \((p_1, q_1, p_2, q_2) = 0\), which we found to be stationary (see footnote 1 in the paper). More formally, we run residual-based augmented Dickey-Fuller tests which strongly rejected no cointegration up to four lags. In addition, variable addition tests proposed by Park et al. (2010) with added variables \( t^2 \) and \( t^3 \) fail to reject the null of cointegration.
Table S.2. EBM Estimation Results. Results from estimating the model in (2) in the paper using least squares (OLS) and asymptotically normal canonical cointegrating regression estimates (CCR) (Park et al., 2010) with $p_1 = 0, p_2 = 0, 1$, and $q_1, q_2 = 0$ in (S.4).

 conduct 999 bootstrap replications in this manner, and the sample paths plotted in Figure 2 in the paper reflect the periodic functions with 0.05 and 0.95 quantiles of $\theta_2$ in (S.1), which determines the period.

Our bootstrap differs from a typical bootstrap in that we do not re-estimate only the period and restrict the other parameters to their fitted values. Re-estimating all of the parameters allows uncertainty about the period to be correlated with uncertainty about the phase shift in particular, because uncertainty in both parameters affects our dating of the function’s optima.

S.5.2 Approximations to Uncertainties in Forcings

Myhre et al. (2013) estimate forcings (in W/m²) with 90% confidence for 2010 as follows: from WMGHGs: 2.83 (2.54 to 3.12); from ozone: 0.07 (0.02 to 0.12); from tropospheric aerosols & surface albedo: $-0.35$ ($-0.85$ to 0.15); and from solar irradiance: $0.05-0.05$ (0.00 to 0.10). Volcanic forcings are estimated to be $-0.06 (-0.08$ to $-0.04$) over 1999-2002 and $-0.11 (-0.15$ to $-0.08$) over 2008-2011. Myhre et al. (2013) state that uncertainty about forcing from WMGHGs is about $\pm 10\%$, and indeed this roughly corresponds to the numerical values given for 2010. The numerical values given for volcanic forcings over the two periods correspond to $\pm 33\%$ within rounding error. If we make a similar assumption regarding the other forcings, then uncertainties about forcings from ozone, tropospheric aerosols & surface albedo, and solar are $\pm 70\%$, $\pm 143\%$, and $\pm 100\%$ respectively in 2010.

We assume that these percentages are roughly the same each year, as they are for WMGHGs and appear to be for volcanoes. In this way, we generate intervals for each forcing in each year to be consistent with the given data, in the spirit of Poppick et
al. (2017) but without a bootstrap. This procedure inherently but realistically allows heteroskedasticity, because the uncertainty grows as the forcing’s value grows. However, it does not allow for heteroskedasticity due to more precise measurements over time, e.g., as noted by Myhre et al. (2013) for solar. We further assume that the uncertainties in the forcings are purely idiosyncratic in the sense that they are neither time-dependent nor mutually dependent.

Approximating the distributions of these uncertainties by a Gaussian distribution centered at the reported value, we can estimate the variance by dividing the difference in the quantiles by $2 \times 1.645$ and squaring the result, which reverses the formula $\pm 1.645 \sqrt{\text{var}(v_t)}$ to calculate 90% intervals from the variance of a mean-zero Gaussian random variable $v_t$. Doing so generates a $5 \times 5$ variance/covariance matrix for each time period, with the estimated variance of each of the five forcings along the main diagonal and zeros elsewhere.

The average of the diagonals, reflecting the variances of the uncertainty for each forcing (WMGHGs, ozone, aerosols, solar, and volcanic respectively), is estimated to be $(0.007, 0.001, 0.284, 0.002, 0.012)'$ over 1850-2016 and $(0.029, 0.003, 0.969, 0.004, 0.000)'$ over 1999-2013, the recent fifteen-year hiatus period. As expected, forcings from aerosols are estimated to be the most uncertain. The uncertainties over the hiatus period are generally larger than those over the whole sample, reflecting the larger magnitudes of the forcings near the end of the sample. In contrast, volcanic forcings are nearly zero during the hiatus period, reflecting the absence of a volcanic eruption with a major impact on global climate.

S.5.3 Contribution of Regressor Uncertainty to Coefficient Estimators

Uncertainty in the regressors may be treated as “classical measurement error” in the parlance of the econometrics literature, which is known to cause bias in the coefficient estimates. Because we observe forcings with error, we may denote our observation of forcings by $h_t = h_t^0 + v_t$, where $h_t^0 = (h_{1t}^0, h_{2t}^0)'$, with $h_{1t}^0$ and $h_{2t}^0$ denoting respectively the sum of non-volcanic and volcanic forcings and volcanic forcings if the forcings could be observed without uncertainty. Similarly, $v_t = (v_{1t}, v_{2t})'$ such that $v_{1t}$ and $v_{2t}$ are respectively the sum of uncertainties about non-volcanic forcings, estimated as described above, and uncertainty about volcanic forcing. $v_t$ has a mean of zero and its components have variances given by $\sigma^2_{v_{1t}, v_{2t}}$ and $\sigma^2_{v_{1t}, v_{2t}}$. The former is the sum of the variances of the non-volcanic forcings, as the covariances are assumed to be zero.

In a cointegrating model like ours, bias in the long-run relationship is not hard to fix. In fact, although it was not designed to do so, the feasible CCR-Canonical Cointegrating Regression (CCR) methodology of Park et al. (2010) already takes into account this bias. Using a closely related model, Miller (2010, Theorem 2) shows the CCR estimator to be consistent, asymptotically normal, and asymptotically unbiased, with a variance that takes into account the measurement uncertainty.\(^8\)

S.5.4 Contribution of Uncertainty to Forecasts

Explaining the contributions of the uncertainty to the missing heat of the 1998-2013 episode requires a measure of in-sample fit of $T_t^a$ for some arbitrary time period $t = 0$, given by $\hat{T}_t^a = h_t^0 \hat{\alpha} + x_t \hat{\gamma} + w_t \hat{\delta}$. For simplicity, denote the right-hand side

\(^8\)We do not model uncertainty in measuring temperatures, which is expected to be smaller than uncertainties in the forcings. Also, we do not model the effect of uncertainty in the volcanic forcings on the coefficient estimate, because the uncertainty is much smaller than the uncertainty for the other forcings – nearly zero – over the 1998-2013 period.
by \( z'_t \), with \( z_t = (h_t', x_t', w_t') = (1, h_{1t}, h_{2t}, x_t', w_t') \) and \( \pi = (\alpha', \gamma', \delta)' = (\alpha_0, \alpha_1, \alpha_2, \gamma', \delta)' \) and let \( z^0_t = (1, h^0_{1t}, h^0_{2t}, x'_t, w_t') \) and \( \varpi_t = (0, v_{1t}, v_{2t}, 0, 0) \), such that \( z_t = z^0_t + \varpi_t \). The variance of the uncertainty in \( \hat{T}_0^a \) is given by

\[
\text{var}(\hat{T}_0^a | z^0_0) = z^0_0' \text{var}(\hat{\pi} | z^0_0) z^0_0 + \text{var}(\varpi'_0 \hat{\pi} | z^0_0) + 2 z^0_0' \text{cov}(\hat{\pi}, \varpi'_0 \hat{\pi} | z^0_0) + \text{var}(\varpi'_0 \hat{\pi} | z^0_0) - \text{var}(\hat{\alpha}_1 | z^0_0) \bar{\sigma}^2_{1v} + \text{var}(\hat{\alpha}_2 | z^0_0) \bar{\sigma}^2_{2v})
\]

using this notation.

If we could observe \( z^0_0 \), a 90% uncertainty interval for \( \hat{T}_0^a \) would be

\[
z^0_0 \hat{\pi} \pm 1.645 \sqrt{z^0_0' \text{var}(\hat{\pi} | z^0_0) z^0_0 + \text{var}(\hat{\alpha}_1 | z^0_0) \bar{\sigma}^2_{1v}}
\]

from the first term. Instead, rewrite the variance as

\[
\text{var}(\hat{T}_0^a | z^0_0) = z^0_0' \text{var}(\hat{\pi} | z^0_0) z^0_0 + \text{var}(\hat{\alpha}_1 | z^0_0) \bar{\sigma}^2_{1v} + \text{var}(\hat{\alpha}_2 | z^0_0) \bar{\sigma}^2_{2v}
\]

\[
+ [2 z^0_0' \text{cov}(\hat{\pi}, \varpi'_0 \hat{\pi} | z^0_0)] + [\text{var}(\varpi'_0 \hat{\pi} | z^0_0) - \text{var}(\hat{\alpha}_1 | z^0_0) \bar{\sigma}^2_{1v} + \text{var}(\hat{\alpha}_2 | z^0_0) \bar{\sigma}^2_{2v})]
\]

where \( \bar{\sigma}^2_{1v} \) and \( \bar{\sigma}^2_{2v} \) are temporal averages that estimate the variances \( \sigma^2_{v1,t} \) and \( \sigma^2_{v2,t} \) at \( t = 0 \). Specifically, we use the averages over 1999-2013 given above, so that \( \bar{\sigma}^2_{2v} \) is effectively zero.

Ignoring the two terms in brackets and setting \( \bar{\sigma}^2_{2v} = 0 \), a 90% uncertainty interval for \( \hat{T}_0^a \) given by

\[
z'_0 \hat{\pi} \pm 1.645 \sqrt{z'_0' \text{var}(\hat{\pi} | z^0_0) z^0_0 + \text{var}(\hat{\alpha}_1 | z^0_0) \bar{\sigma}^2_{1v}}
\]

takes into account uncertainty in the non-volcanic forcings. Because the uncertainty in the regressors is correlated with the uncertainty in the estimator, the bracketed terms are not zero, but we expect that they will be small.

As with predictions from any linear model, the variance of the out-of-sample conditional forecasts is augmented by the estimated variance of the error term \( \eta_t \). In that case, we drop \( \text{var}(\hat{\alpha}_1 | z^0_0) \bar{\sigma}^2_{1v} \), because we are conditioning on specific data and there are no measurement errors, and because we use least squares rather than CCR, in order to minimize mean squared forecast error.
References


