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22. Sweat glands were obtained by skin punch biopsy from healthy volunteers who gave informed consent. Segments (1 to 2 mm in length) of reabsorptive ducts were microdissected from single sweat glands and microperfused as described (*13*). To reduce the duct preparation to a single membrane with Cl^- conductance only, all luminal perfusion solutions contained 140 mM K^+ (no Na^+) and 10 to 5 mM amiloride to remove any Na^+ conductance. Cl^-, gluconate, and ATP were substituted as required. The cytoplasmic bath solution contained 140 mM K^+ gluconate with 10^{-4} M cAMP and 5 mM ATP added as required. All solutions contained 1 mM $CaCl_2$, 1.2 mM $MgSO_4$, 2 mM K_2EGTA, and 3 mM K_2HPO_4 (pH 7.3). Measurements were made at 35° ± 2°C. Constant current pulses of 50 nA and 0.5-s duration were applied through one barrel of a double cannula perfusion pipette. Luminal perfusates were introduced, and luminal potentials were measured through the other barrel.
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25. We thank S.-X. Zheng and J. Liao for technical assistance. Supported by grants from the National Institutes of Health (DK43994 to R.R.K., HL42368 and DK45913 to J.J.W., and DK41329 to P.M.Q.), the Canadian and U.S. Cystic Fibrosis Foundations, the Medical Research Council (Canada), Cystic Fibrosis Research, Inc., and gifts from Patricia Bresee and Kay and Ronald Presnell. This work was done during the tenure of an Established Investigatorship of the American Heart Association (R.R.K.), a U.S. Cystic Fibrosis Foundation postdoctoral followship (K.L.G.), and a Medical Research Council Scientist award (J.W.H.).

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TECHNICAL COMMENTS

Testing for Bias in the Climate Record

We have tested two of the conclusions made by David J. Thomson in his article (*1*): That "anomaly series in climate research that have been deseasonalized by subtracting monthly averages need to be recomputed" and that "significant efforts must be made to understand the consequences of the annual temperature cycle following precession rather than the equinoxes, and of the capture effect." Unlike Thomson, we used daily data, which is necessary to detect possible bias resulting from precession in an instrumental climate record based on deseasonalized climate trends.

We tested Thomson's assertions with the use of 144 long-term stations (each station generally spanning about 75 to 85 years) within the contiguous United States. Most of these stations were derived from the U.S. Historical Climate Network (*2*). We also used the longest homogeneous daily series available, the Central England Temperature (CET) time series (*3*). Using these series, we estimated the change in monthly, seasonal, and annual trends, allowing for the change in perihelion dates. Over the span of the CET time series, 219 years, the change is about 5 days, and for the U.S. stations the change amounts to 2 days (perihelion shifts toward increasing Julian days). We compared monthly trends of temperature (calculated from deseasonalized anomalies of daily mean, maximum, and minimum temperatures based on Gregorian calendar months) with trends derived from deseasonalized values [where the days included in each month were offset by the change in the date of perihelion from the beginning of the record to the end of the record (*4*)]. Such a test bounds the maximum impact of precession because the differences will also result from seasonal effects and the random effects of weather as well as any effects due to precession of the orbit.

We found that incorporating perihelion calendar shifts in the network of U.S. stations had no effect on the annual mean temperature trends. However, monthly differences did appear (Fig. 1), but a question arose with regard to the significance of the monthly difference. How much is simply due to random chance (that is, the vagaries of weather over two days during the course of 75 to 85 years)? This may be reflected by the end of 1 month having a spell of weather significantly different from the beginning of the next month. Even over long records it is conceivable that such an effect may not

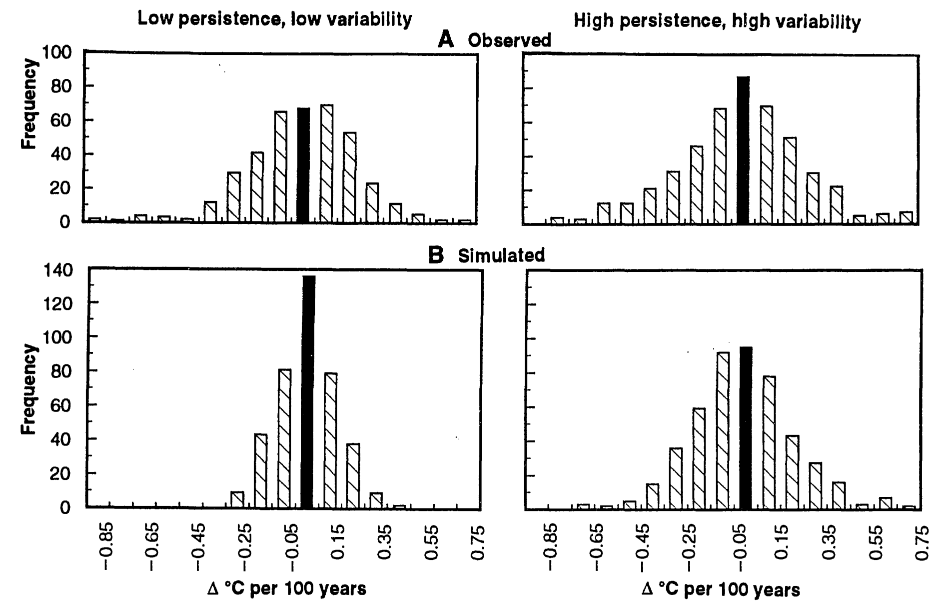

Fig. 1. Distribution of the observed differences (Δ°C per century) of monthly trends in mean daily minimum temperature between standard deseasonalized monthly anomalies and those calculated using a shifted calendar consistent with the Earth's precession of orbit (**A**) versus those simulated where only the effects of seasonality and day-to-day weather variability are active (**B**). Each set of differences is categorized by variance and day-to-day persistence of weather anomalies (both quantities are partitioned by the upper and lower one-half of the distribution of values for all months and all stations). Dark center, little or no difference; striped bars, positive or negative differences.

cancel. To calculate the distribution of such an influence, trends at each of the 144 U.S. stations were simulated with an annual cycle calculated from observed daily data (the first five harmonics captured most of the variance in the annual cycle) as well as the autocorrelation and variance of the standard monthly means. An auto-regressive model (5) of order 1 (AR-1 model) was fit to the residuals of the daily temperatures minus the value of the sum of the harmonics, and the variance of each month was calculated in the same manner. The data at each station were simulated with the use of the mean appropriate for the day of the year calculated from the harmonics and the perturbation about this mean from the AR-1 model. Trends were then calculated using the same calendar offset as calculated from precession of the Earth's orbit, as described above, and subtracted from the trends without the offset. The results were stratified by the magnitude of the persistence and variance term in the AR-1 model. A 2 × 2 continency table was developed with class limits based on the median value of persistence and variance for all stations and all months. A significant portion of the difference in trends was found to be a result of the vagaries of weather and had nothing to do with precession of the orbit (Fig. 1). We show the result for the minimum temperature (Fig. 1), but results are similar for the maximum and mean temperature.

The effect of perihelion should be most apparent in the longest daily data set available, the CET. After incorporating the perihelion dates into the CET data set, spring (March, April, and May) warmed relative to the standard trends by 0.42°C over the 219 years and autumn cooled by 0.46°C. Using standard monthly data, we found that winter warmed relative to summer over 1772 to 1990 by 1.1°C; allowing for perihelion reduced this difference to 1.0°C. For the last 100 years (1891–1990), trend differences for the CET series are within the ±0.25°C range, falling in the center of the U.S. distributions (Fig. 1).

Inspection of the differences in trends calculated with and without the shift in calendar dates indicated that a bias was introduced during the transition seasons. It averaged between 0.05° and 0.10°C per century, with a positive bias during spring and a negative one during the autumn. This was due to shifting the calendar to later in the season. During summer and winter the slight shift of 2 days showed little bias, and even a 5-day offset in the CET time series produced only small differences.

In sum, we find Thomson's concerns about the manner in which climatologists have calculated trends on monthly and annual time series to be of little consequence during the instrumental climate record.

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5 June 1995; accepted 28 August 1995

Are warming effects that result from man's activity detectable in the subtle phase changes in the seasonal temperature cycle found by Thomson (*1*)? Given this important question, we raise specific issues for discussion.

1) Misidentification of the anomalistic year signature in temperature time series. The precession constant, 50.256″ per year, is the rate at which the Earth's rotation axis precesses, and it can be computed as a difference in the rate of change of phase between oscillations with a period of the sidereal year (365.256360 days per year) and those with the period given by the tropical year (365.242194 days per year) (*2*). It is not the rate of change of the phase between oscillations with periods defined by the lengths of the tropical year and the anomalistic year (365.259644 days per year), as is apparently assumed in Thomson's analysis. Therefore, his finding of a phase rate difference of about 50″ per year does not identify the anomalistic year as a property of terrestrial temperature data.

We see no explanation of his rate estimates of 50″ per year, derived from the data shown in figures 1, 2, and 4 of his report unless Thomson can detect the sidereal year in his analysis. The sidereal year has no physical relevance in the seasonal temperature cycle. If the changes due to the anomalistic year and the cited capture effect really occur, he should find a phase rate difference of 61.9″ per year because this is the phase rate difference between oscillations with periods of the tropical and anomalistic years. His stated significance tests on the Central England time series (99.999%) indicate sufficient accuracy to detect the 61.9″ per year phase rate difference if it is present. A mechanism analogous to the capture effect cited by Thomson requires resonances of sufficiently narrow bandwidth to separate a frequency difference of 1/20000. Such resonances do not appear to exist in the climate engine.

The appearance of epochs with zero frequency difference [figures 2 and 3 of the article by Thomson (*1*)] is not unexpected, as we expect the temperature variation to be in phase with the tropical year at temperate latitudes. Near the equator, the solar driver has a phase rate twice that of the tropical year, superposed on the phase change due to the anomalistic year; but the amplitude of the anomalistic year component is weaker than that from the tropical year.

2) Decrease in amplitude of seasonal temperature variation. We confirm the decrease in the amplitude of the annual temperature cycles shown in figure 9 of (*1*) for both hemispheres. Thomson appears to assume, without discussion of the underlying processes and latitudinal dependencies, that an increase in solar irradiance necessarily leads to an increase in the amplitude of the seasonal temperature cycle. He then points out that the observed amplitude decrease argues against solar forcing because the trend of solar activity is increasing over the entire period. We expect the effects of increased solar irradiance or increased greenhouse gas concentrations to be quite complex due to the many feedback mechanisms in the climate system. For example, increased circulation due to global warming may well reduce the seasonal amplitude variation in higher latitudes. Detailed calculation of the effects of both solar and greenhouse forcing on the annual temperature cycle using the current general circulation models (GCMs) is needed to resolve the issue.

3) Variability in the phase curves from different locations. The discussion of figures 1, 2, 3, and 4 in Thomson's article (*1*) needs substantial revision to remove the anomalistic year references, but the data themselves show intermittence in the occurrence of even the tropical year frequency in surface-temperature records. However, the 12 locations around the world exhibit phase shifts with much larger amplitudes than expected. Thomson links these differences between individual stations to "local topography, heat storage, and albedo" in each case, but not to latitude. Without established physical relationships

between the observed phase-shift patterns and these effects, conclusions concerning solar and greenhouse forcing are premature. Again, the issue calls for calculations with current GCMs with sufficient spatial resolution to understand regional differences as functions of latitude.

4) Post-1930 epoch: Phase and CO_2 level correspondence. We find Thomson's discussion of figures 6 and 12 of his article (*1*) misleading, as he implies that the phase change of the annual temperature variation in both hemispheres is caused by the slow increase of the atmospheric CO_2 content over the last 130 years. Thomson does carefully point out in the captions that his suggested relationship comes from a regression of the logarithmic CO_2 variation with his computed phase rates. From a purely analytical point of view, the superposition of a slowly increasing temperature due to greenhouse warming by CO_2 with a sinusoidal signal representing the seasonal cycle does not give the phase shifts seen in either his figures 6 or 12. We simulated such a composite signal with a 1-year sinusoid with a 6 K amplitude superimposed on a hyperbolically growing temperature increase of 0.7 K—similar to that expected if the temperature increase is a result of greenhouse warming. Calculation of the variation of the phase with time shows a phase shift of less than 0.001″ per year. Without an internal mechanism linking temperature of CO_2, the cause of the unprecedented change in phase of the annual surface temperature since A.D. 1940 remains a mystery.

The Northern Hemisphere temperature data [figure 5 in the article (*1*)] show the abrupt change in slope of the phase variation at about A.D. 1940. This relatively abrupt change in phase of the seasonal cycle has no counterpart in the monotonically increasing CO_2 record. Interestingly, at this same time such a counterpart exists in the instantaneous frequency of the solar cycle as expressed by ^{10}Be radioisotope record (*3*). The absence of a physical mechanism linking sunspot activity and solar cycle phase shifts to surface temperature variations leaves the entire matter unclear. The origin of the intermittent 50″ per year shift found by Thomson remains unknown.

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3 August 1995; accepted 13 December 1995

Response: I thank Karl *et al.* and White *et al.* Their comments confirm many of my basic observations (*1*), so that disagreement seems limited to interpretation and terminology.

Karl *et al.* confirm my observation (*1*) that the temperature data is accurate enough to distinguish the effects of precession. I disagree, however, that the changes are of little consequence and do not require correction. Proving or disproving the existence of a greenhouse warming signal is a statistical detection problem, characterized by the probabilities of false alarms and of missing a real signal. Uncorrected bias terms, such as precession, increase the probability of both kinds of error. Setting acceptable limits for such terms depends on three factors: First, allowing such biases to degrade the detection error probability by a factor of 2 implies (*2*) that these biases must be cumulatively less than 5.2% of the noise variance. Second, when filtered to have a time resolution of 1 year, the Jones-Wigley global temperature series has a residual standard deviation (SD) less than 32.1 mK for the interval 1854 to 1920 (*3*). Taking 5.2% of this variance leave 7.3 mK. Third, there are at least 20 identifiable effects, plus an unknown number of effects yet to be discovered, that might influence climate data. Allowing an equal share of the variance for each of these implies that one should attempt to eliminate individual bias terms that exceed 1.6 mK in a global series at annual resolution.

The seasonal bias from the precessional shift of 1.4 days per century has a root-mean-square value of 0.087 AT mK, where A is the amplitude of the annual cycle in kelvins and T the length of the record in years. Thus, if precession were the only problem, as Karl *et al.* have shown, standard anomaly series would be acceptable where the amplitude of the annual cycle is low and the records are short.

A worse problem with anomaly series, not addressed by Karl *et al.*, is caused by rapid variation of the phase of the annual cycle during the reference period. To see the effects of phase changes during the reference period, consider a "half-artificial" temperature series

$$x(t) = A\cos[2\pi t + \theta(t)]$$

where $\theta(t)$ is the measured phase from Williston, North Dakota (typical of central North America) (Fig. 1), and A is the average amplitude, 16.71 K. In such a climate, the summer temperatures always reach exactly 16.71°C, the winter temperatures −16.71°C, the annual maxima and minima occur within a span of ±4 days over the duration of the record, and the extreme annual average temperature would be 0.039°C. There are no trends in minimum, maximum, or average temperature. The standard deseasonalizing procedure, however, produces an anomaly series with extremes of −1.63° and +1.56°C and an SD of 0.337 K. This squared-error is a factor of 44,000 larger than $(1.6\ \text{mK})^2$. This series also has apparent seasonal trends of +0.561 K per century in April and −0.622 K per century in October. The apparent annual trend is only −0.0188 K per century and the SD of the monthly trends is 0.44 K per century. As the apparent trend in temperature attributed to anthropogenic greenhouse gases is about 0.6 K per century, the seasonal bias in the anomaly series could be a significant cause of confusion or misattribution.

While such bias terms nearly vanish when averaged over a full year, they will contribute substantially near points where records have gaps, begin, or end, and will also be largely coherent over continental scale areas, so spatial averaging will be of limited help.

Thus, I see at least four problems with the standard deseasonalizing procedure: (i) There are components of temperature at both the tropical and anomalistic year; the process cannot handle both simultaneously and, as Karl *et al.* have shown, the data are

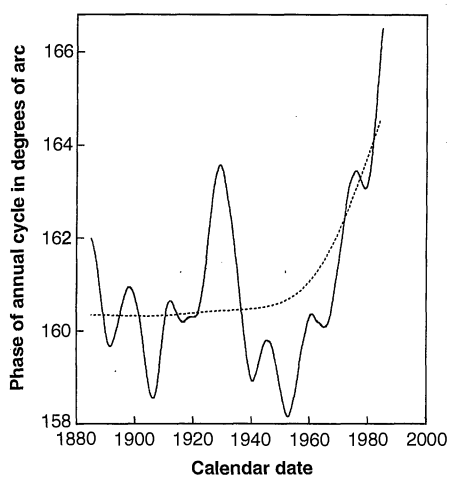

Fig. 1. Phase of the annual cycle at Williston, North Dakota, and a simple approximation using (Eq. 2) with different logarithmic dependences for both the direct and transport amplitudes. Influence of CO_2 suppresses the amplitude of the annual cycle on both terms, but the effect is larger on the direct radiation component than it is on the transport term; overall amplitude decreases and the phase changes rapidly in response to CO_2.

good enough to distinguish between them. (ii) The amplitude of the annual cycle varies significantly at many locations, so removing a constant amplitude climatology leaves another significant error term. (iii) The phase of the annual cycle has been more variable in the latter half of the century than previously, so the reference period is not typical of the earlier record. (iv) Because of the characteristic seasonal signature in the anomaly series, apparent correlations between stations will be altered, so reliability of the average series may not be properly assessed.

Each of these effects contributes much more than 1.6 mK, so I stand by my statement that "Anomaly series . . . need to be recomputed" (*1*, p. 66).

The persistence effects mentioned by Karl *et al.* are real and need to be better understood. I estimated spectra for several of their better quality daily U.S. temperature records and found that even the lower parts of the spectrum are at least 200 times larger than can be explained by quantization errors and the like. Autoregressive behavior explains the general shape of the spectrum, but the estimated spectra are so complicated that a more involved model of persistence is necessary. A detailed description of this analysis will be given elsewhere.

The second point raised in Karl *et al.* concerns my statement, "significant effort must be made to understand the annual cycle following precession rather than the equinoxes and of the capture effect." Although Milankovitch theory correctly explains most long-term climate observations, the effects of eccentricity are less satisfactory, and some revision is required (*4*). The phase trends could not be observed if the Earth were in a circular orbit, so their existence may be an important clue to the eccentricity puzzle. Despite the implications of the observed phase following perihelion, winter is unlikely to occur in July, even in 11,000 years. Thus, even though the trend has apparently continued for at least 300 years, at some point in the future it must change (*5*). The evidence favors an abrupt switch in the average atmospheric or oceanic circulation pattern. Phase plots from several European locations all show a phase discontinuity in about 1860, similar to that for Paris shown in figure 2 of my article (*1*). Thus, circulation patterns likely switched over much of Europe at that time.

The paleoclimate record also contains much evidence for abrupt discontinuities (*6*) that are not easily explained by Milankovitch theory; my intent was to suggest a possible mechanism and that slow changes in a climate parameter, such as greenhouse gas concentrations, may not necessarily be accompanied by slow changes in the climate. Indeed, the instrumental evidence suggests that an abrupt change happened in about 1920 and another in about 1970.

None of the assertions in my article (*1*) have caused as many questions as the ideas that the relatively slow variations in atmospheric CO_2 can cause rapid changes in the timing of the seasons. The relevant equations are given herein (*7*). The "capture effect" gives a good first approximation of the phase when two signals are present, but *both* the tropical and anomalistic years must be included in a complete analysis. It is the changing balance between these two components that accounts for the rapid recent change in phase.

White *et al.* raise the issue of the difference between the general precession constant, $\Psi = 50''.2$ per year, and the rate of change in the longitude of perihelion $\dot{\omega} \approx 61''.9$ per year. I established that the phase slope differs significantly from zero. Equation 3 (*7*) shows that knowing this fact is more important than having the exact value. My estimate of the SD of the slope in the Central England series of $6''.82$ per year shows that the difference between $61''.9$ per year and $51''.1$ per year is 1.6 SDs. Thus, with the available data, one cannot clearly distinguish between Ψ and $\dot{\omega}$. Moreover, the difference between Ψ and $\dot{\omega}$ only matters in the approximate theory heuristically described by the capture effect. Some complications with the Central England series are described in (*8*).

Next, there is reasonable evidence (*9*) that aspects of the climate system respond to solar energetic particles. The number of such particles interacting with the Earth will depend on the orientation of the ecliptic relative to the heliospheric current sheet and so could vary as the sidereal year. Although the changing balance of direct radiation and transport is more probable than an explanation based on the solar wind, the possibility should not be dismissed.

White *et al.* are incorrect in their assumption that narrow resonances are required for the capture effect. Close to the opposite is correct (*8*). All that is required is that two signals near the same frequency be present. If a resonator is present, both frequencies must be within its passband.

White *et al.* next discuss the declining amplitude of the annual cycle, which they confirm. In addition to latitude dependence, the amplitude signal is complicated by changes in instrumentation and by solar and albedo effects. The same is true at low frequencies, but the signal-to-noise ratio appears to be larger on the annual cycle than it is at low frequencies, so that analysis of the amplitude and phase may permit more reliable assessment of these effects than is currently being obtained from only low-frequency averages. When attempting to detect small, low-frequency signals, it is common practice in engineering and physics to "chop" the signal, amplify the chopped signal, then demodulate. Here, the annual cycle has, in effect, provided the chopper for us.

I agree that the climate system is complicated and has many feedback mechanisms. However, in attempting to distinguish between solar variability and greenhouse gases, I assumed (i) that *small* increases in solar irradiance would produce corresponding small increases in both the average temperature and in the amplitude of the annual cycle. This is exactly what is observed in the early parts of the record. Second, I assumed (ii) that the albedo feedback effects of greenhouse gases suppress the annual cycle in the way Manabe and Stouffer's GCM calculations and more recent calculations (*10*) predict. This appears to be what is observed in the post-1920 record. Because individual station records show decreases in amplitude compatible with the hemispheric averages and plate tectonics is a slow process, one cannot invoke latitude dependence as an explanation.

White *et al.* next question the variability of the phase curves with location. Because of the uncertainty in the phase slopes mentioned above and in (*7*), references to the anomalistic year should remain (*11*). The accuracy of the estimated slopes (which White *et al.* have reproduced) is sufficient

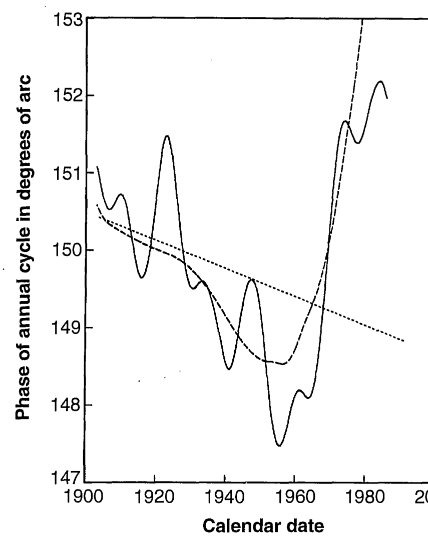

Fig. 2. Phase of the annual cycle at Sable Island, Nova Scotia. The dotted line has a slope of $-62''$ per year, and the dashed line is a least-squares fit to the phase with a constant, linear trend, and $\log CO_2$. Because the phase change is small, a Taylor's series approximation to Eq. 2 should be reasonable; however, it cannot separate the different dependencies on ε_d and ε_t. The estimated linear slope is $-55''$ per year. A slowly varying $\log CO_2$ curve accounts for most of the rapid change in phase.

to say that the phase slope is not zero; it is insufficient to discriminate between 51 and 62 arc-seconds per year and, as Eq. 3 (7) shows, there is no need to do so. Except for the gross subdivisions into Northern Hemisphere, Southern Hemisphere, and Tropics, I did not attempt to extract a latitude dependence; phase changes between records near the same latitude (for example, Central England and Warsaw) are at least as large as those between Central England and Milan. The plot of the different phase characteristics at Geneva and Basel [figure 2 in (1)] was made both to show that local topography, and so forth, could decisively switch the relative sizes of the direct and transport components, and also as a caution that simple regional averages could be "adding apples and oranges"; so, as always, data should be looked at carefully before averaging.

However, as latitude is implicitly included in the insolation formulas used to derive the direct radiation term given in Eq. 1 (7), these formulas show a mean latitude dependence that could be included in an augmented energy-balance model.

The simulation described by White *et al.* is not a simulation of the mechanism I proposed. The latter is *not* a sinusoid superimposed on a slow temperature increase. Rather, it has *two* sinusoids, one at the tropical year, the other at the anomalistic. In the Northern Hemisphere the two are approximately 180° out of phase, both are relatively large amplitude, and their vector sum gives the observed annual cycle. To see the effects of CO_2, approximate the amplitude of the direct component by replacing Eq. 1 (7) with

$$T_d(t) = D(1 + \varepsilon_d \log_2[CO_2(t)]) \cos(2\pi t + d)$$

and similarly with the transport component.

I have made different approximations to phase: for Williston (Fig. 1) and for Sable Island, Nova Scotia (Fig. 2). A nonlinear fit was made to the Williston amplitude and phase (Fig. 1), and the agreement is reasonable. However, I infer that, with this data, albedo feedback should be considered. Sable Island has been almost unchanged and nearly uninhabited for the duration of the record. It is a small island in an area where little sea ice develops, so albedo effects should be minimal. Here the fit to the phase with $\log CO_2$ is much better, and even the minimum between 1930 to 1960 is matched. The residual phase errors appear to be primarily of solar origin. In the fits that have been done so far, the sensitivity of the direct component ε_d has been larger than that of the transport component ε_t. The agreement (Fig. 2) argues against the remark by White *et al.* that changing concentrations of atmospheric CO_2 cannot cause abrupt changes in phase.

In sum, I do not agree with the implication that these questions can be completely resolved by more and better GCMs. Models rarely predict the consequences of effects that have not been built into them. For example, given that some major GCMs used a circular orbit for the Earth, how long might it have taken to discover the phase shifts (discussed above) in their output? Although continued development of GCMs is important, it is more important to ensure the continuity and reliability of the data collection process (12), and most important, to analyze the available data carefully.

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2. Reliable detection of a signal requires that the signal-to-noise ratio exceed a given threshold. The reliability of a detection can be improved in two ways: Increase the signal or decrease the noise. The signal is not under direct control, so understanding what is presently classified as "noise" should be a high priority. For similar communications theory detection problems, it is known that a 0.22-dB improvement in the signal-to-noise power ratio (SNR) typically results in a factor of 2 improvement in error rate. See, for example, figure 4.2.8. of J. G. Proakis [*Digital Communications* (McGraw-Hill, New York, ed. 3, 1989)]. An improvement of 0.22 dB in SNR is equivalent to a 5.2% reduction in noise power.
3. The SD of the monthly Jones-Wigley Northern Hemisphere anomaly series from 1854 to 1920 is 313 mK. In figure 9 of my article (1), I showed the Jones-Wigley hemispheric average temperature series low-pass filtered to a bandwidth of 0.5 cycles per year, corresponding to a time resolution of 1 year. A global series formed by averaging the Northern and Southern hemisphere series has an SD of 57.6 mK in the 1854 to 1920 part of the series. (So the increase in temperature is 11 SDs.) A fit to this global series with just a constant, $\log CO_2$, and the Foukal-Lean solar irradiance series [P. Foukal and J. Lean, *Astrophys J.* **328**, 347 (1988)] and feedback leads to a residual variance of $(32.1 \text{ mK})^2$.
4. J. Imbrie *et al.*, *Paleoceanography* **8**, 699 (1993).
5. Recent data implies that the phase of the Central England series may have already begun to change rapidly, but the effects of the Mount Pinatubo eruption make the significance of the apparent change uncertain.
6. G. C. Bond and R. Lotti, *Science* **267**, 1005 (1995).
7. The sum of constant amplitude components at the tropical and anomalistic years results in changing amplitude and phase. To obtain a first approximation, leave out the nonlinear feedback from seasonal changes in albedo and write the direct radiation component as

$$T_d(t) = D\cos(2\pi t + d) \quad (1)$$

where t is measured in tropical years and d is the phase of the direct radiation component. D and d are obtained from a Fourier series expansion of standard insolation formulas. For the Williston example used above, D is 47.1 K and $d = -171.35°$. The transport component can be obtained by subtracting the direct radiation from the observed temperature.

$$T_T(t) = B\cos[2\pi t + \phi(t)]$$

For the same example, $B \approx 33.45$ K and the average phase is $\bar{\phi} = 22.06°$. If one assumes that the transport time is approximately constant, my theory implies that transport should track perihelion so ϕ should, on average, decrease by 62″ per year. The observed temperature is

$$T(t) = T_d(t) + T_T(t) = A\cos[2\pi t + \theta(t)]$$

Trigonometry shows that

$$\theta(t) = \arctan\left[\frac{D\sin d + B\sin\phi(t)}{D\cos d + B\cos\phi(t)}\right] \quad (2)$$

Inspection of this equation shows that if $B \gg D$ one will have $\theta(t) \approx \phi(t)$ and, conversely, when $D \gg B$ one has $\theta(t) \approx d$. This is the essence of the capture effect. Experience shows that it gives a reasonable description when one signal is more than about 1 dB stronger than the other, that is, when the amplitude of the stronger signal is more than 12% larger than that of the weaker. More generally, the rate of change of phase, θ is

$$\dot{\theta} \approx -\dot{\phi}\left[\frac{B^2 + B\,D\cos(d-\phi)}{D^2 + B^2 + 2DB\cos(d-\phi)}\right] \quad (3)$$

With constant amplitude, the mean rate $\dot{\theta}$ is about 1.52 $\dot{\phi}$ or −94″ per year for the Williston example. Many combinations of B, D, and $d - \phi$ can cause the denominator of Eq. 3 to be small, resulting in rapid phase changes. Because local changes in albedo and topography can change the relative sizes of B and D, one expects to find considerable variation in θ at a fixed latitude.
8. My estimate of the SD of the Central England phase slope may be too low for, although I could not find direct evidence for the 416-year Suess period in the Central England phase, the presence of some of the shorter period ^{14}C terms suggests that it should not be ruled out. A 416-year component just below the detection threshold might raise the SD to near 10″ per year. The phase slopes of other long series with similar characteristics range from −31″ per year (Trondheim) to −88″ per year (New Haven). As a possible complication, the *daily* Central England data tabulation includes an entry for 29 February 1800 and, although it is unlikely that the pre-1800 monthly averages are off by a day, a 1-day error in 300 years gives an error of 12″ per year. It is also probable that the time of day of the observations has varied over the last 300 years. A 2-hour shift of observing time is equivalent to a slope error of 1 arc-second per year.
9. J. E. A. Stephenson and M. W. J. Scourfield, *Nature* **137** (1991); L. B. Callis *et al.*, *J. Geophys. Res.* **96**, 2921 (1991).
10. S. Marshall, R. J. Oglesby, J. W. Larson, B. Saltzman, *Geophys. Res. L.* **21**, 2487 (1994).
11. The celestial mechanics were condensed in note 11 of (1). See also notes 12 and 17 of (1). Some details on precession are available [D. J. Thomson, *Philos. Trans. R. Soc. London A.* **332**, 539 (1990)]. Where the difference between Ψ and $\dot{\omega}$ is important, the mean rates are insufficient and the DE 200 ephemeris [E. M. Standish, *Astron. Astrophys.* **233**, 252 (1990)] has been used.
12. Long series data sets are much more valuable for studies of this kind than are an equal number of observations from several short series. In this regard, the *decrease* in the number of available climate records since the 1960s is particularly alarming.

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