ly, combination gas-liquid chromatography-mass spectrometry, where its identification was made. We have considered cholesterol complexed with retinoic acid. Such a complex would explain the findings of Wolfe *et al.* (*1*) as well as our own. However, no such complex has been demonstrated to our satisfaction.

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Nelson and Halley suggest that our data are more compatible with a retinoyl-cholesterol complex as the autofluorescent component in the neuronal storage material of Batten disease. We have found that although most of the phospholipids and cholesterol of CLB's are removed by multiple extractions with chloroform and methanol (2 : 1 by volume), the fluorescent residue, when subjected to basic or acidic methanolysis, still yielded small amounts of cholesterol and fatty acids together with large amounts of a fluorescent material that contained not only a compound which we reported as methyl retinoate but also more polar products which are in the course of investigation. All of these fluorescent components were clearly separated from cholesterol by thin-layer chromatography in benzene. It would thus appear that cholesterol in small amounts is indeed bound tightly to the retinoyl-peptide complex in CLB's, which explains the appearance of cholesterol in direct-inlet mass spectra of lipid-free CLB's. The cholesterol, however, was a minor component which eluted at 40°C. At higher temperatures (50° to 100°C) the major components eluted gave mass spectra with apparent molecular ions of *m/e* 482 and 576, together with prominent peaks at *m/e* 255 and below as reported in (*1*). Ions of *m/e* above 255 characteristic of cholesterol were not found in these mass spectra. Since cholesterol was eluted under electron impact, it is highly unlikely that it is covalently bound.

We disagree with the contention that the proton magnetic resonance spectra of the lipid-free CLB's dissolved in d_6-dimethyl sulfoxide or treated with a strong base are "not inconsistent" with the published spectra of cholesterol. The bulk of the absorptions in cholesterol are in the region 0.67 to 1.2 parts per million (*2*), unlike our reported spectra. That our spectra do not correspond exactly to reported spectra of the free A vitamins is not surprising since we were dealing with both the intact retinoyl-peptide complex and a base-treated material. More detailed chemical evidence for our initial report will be published elsewhere (*3*).

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Variations in the Earth's Orbit: Pacemaker of the Ice Ages?

Hays *et al.* (*1*) have "concluded that changes in the earth's orbital geometry are the fundamental cause of the succession of Quaternary ice ages." They reached this conclusion by statistical analysis of three time series (obtained by combining data from two deep-sea sediment cores) without attempting to identify or evaluate "the mechanisms through which climate is modified by changes in the global pattern of incoming radiation. . . ." Such a procedure places stringent

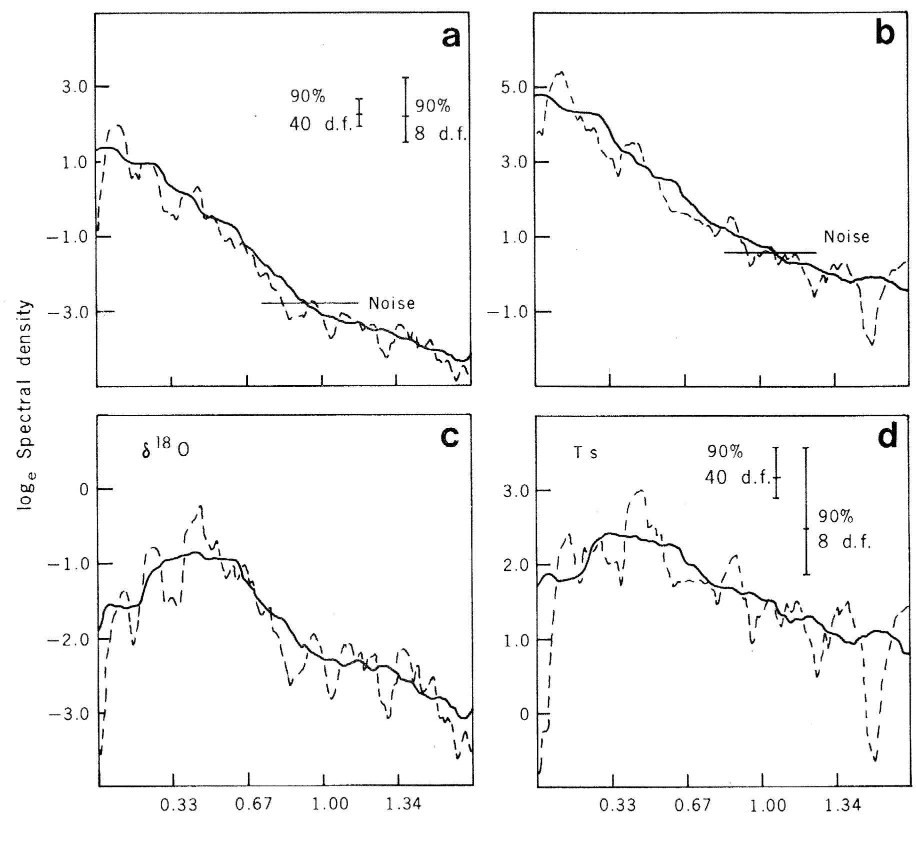

Fig. 1. Spectra of climatic variations in the combined (PATCH) record from two subantarctic deep-sea cores based on the TUNE-UP age model (*1*). (a and b) High-resolution spectra (dashed lines) and low-resolution spectra (solid lines) expressed as the natural logarithm of the variance per unit frequency as a function of frequency. (c and d) The same spectra after prewhitening with a first difference filter. Confidence intervals are the same for (a) and (b) and for (c) and (d).

demands on the quality of the statistical techniques and their interpretation. We feel that, while the calculations presented are suggestive, they are far from conclusive.

The notion that the existence of peaks in the spectra of the geological series is indicative of forcing at those frequencies is a substantial oversimplification of the concept of a time-invariant linear system. Many familiar systems, such as a mechanical system with a spring and a dashpot, exhibit peaked frequency response functions and hence may show spectral peaks in the output even when there are none in the input (*2*). Such peaks are a consequence of the dynamics of the system, not just the spectral character of the input. A far more convincing approach would be to assume astronomical forcing as proposed by Vernekar (*3*), compute the transfer function, and attempt to provide a physical model which accounts for the observed peaks.

Realizing that statistically significant peaks in the spectra of the geological series might convey useful information, we consider those calculations. It is well known that the technique of Fourier-transforming the autocovariance function may lead to questionable results (normalized standard errors, ϵ, greater than 0.3) if the maximum number of lags exceeds about one-tenth of the record length (*4*). The number of lags (m) was 50 for a record of length (n) 163 for the high-resolution calculation of figure 6 in (*1*), so $\epsilon = (50/163)^{1/2} = 0.55$.

Considering figure 6 (*1*) in more detail, we find the use of the "one-sided confidence interval" to be curious. It must presumably be justified by a priori knowledge that the spectral density at a given frequency will exceed the background. (The observed peaks do not fall exactly on the predicted frequencies.) In fact, the whole technique presupposes an ergodic, stationary random process, so one is obliged to quote the more conventional two-sided confidence interval (C.I.), which is not only larger overall but has a lower lower bound. Plotting this C.I. for the high-resolution spectrum and a two-sided C.I. for the low-resolution spectrum indicates that, for the peak to be significant at probability $P = \alpha$, the upper bound of the low-resolution $(1 - \alpha) \times 100$ C.I. and the lower bound of the high-resolution $(1 - \alpha) \times 100$ C.I. must not intersect. None of the peaks plotted satisfy this criterion at $\alpha = 0.1$. Indeed, only one peak (in the prewhitened $\delta^{18}O$ spectrum) would satisfy the authors' original criterion. If it is argued that this test is too stringent and that the peaks in the nonprewhitened spectra appear to be significant, it must be recalled that for peaked, sloping spectra the number of degrees of freedom is greatly overestimated by $2n/m$ (*4*), so that the C.I.'s plotted are actually too small.

Finally, attempting to deduce the forcing frequencies from the prewhitened spectra is itself questionable because the effect of prewhitening is to multiply the spectrum by $1 - 0.998 \cos (2\pi f \Delta t)$, where f is the frequency and Δt is the sampling interval—that is, to include another frequency-dependent multiplicative factor in the (unknown) frequency response function. Although this is an acceptable technique, it is clear that the location of peaks will not be the same as in the unprewhitened spectrum.

To gain a better understanding of the potential significance of time series analysis of the geological record, we have recomputed the summer temperature and oxygen isotope composition ($\delta^{18}O$) spectra (*5*) using fast Fourier transform techniques (*6*), attempting to match the equivalent resolution bandwidth used in (*1*). As can be seen in Fig. 1, there are no statistically significant peaks (at $P = .1$) in the spectra, although there is a suggestion of variance concentration at 96,000, 43,000, and 21,000 years. For any reasonable transfer function the phase of the response should vary in a simple fashion near the peak of the response, either remaining constant far from a resonance or varying linearly at a resonance. In fact, close to the main peaks observed in Fig. 1, phases can be found in all four quadrants of the circle and phase appears to be varying randomly as a function of frequency. Thus, either the required phase stability is missing or the signal-to-noise ratio is so high as to mask it (in which case only a small fraction of the variance is due to the presumed forcing). We therefore conclude that while these results may be tantalizing, they fall far short of confirming the Milankovitch hypothesis; there may be an astronomical effect, but it is evidently small.

The time-domain analysis is also suggestive but the phase relations should be studied quantitatively by cross-spectral analysis. Lacking a specific model for the spectral continuum, the low-resolution spectra were used for comparison to test the statistical significance of the peaks. While this method is acceptable, it should be noted that other theories of climatic variation (*7*) predict the observed "red" spectrum. Regardless of how the background is determined, it is misleading to cite the total variance from abscissa to peak as being due to the proposed forcing. Only that above the background is of significance. Thus, there has been an overestimate of the variance accounted for at each peak by a factor of 3 to 5. Cross-spectral analysis would have the additional advantage of producing the best model (in a linear least-squares sense) relating the astronomical input to the geological record. It might even account for the variance in the 96,000-year band without an appeal to nonlinearity. In any event, the coherence and phase information would provide a quantitative measure of the amount of variance accounted for by the astronomical forcing. The model will still require a sound physical basis before the fundamental hypothesis can be accepted. Perhaps when more long cores with well-established chronologies are available to permit ensemble averaging, a proper application of these techniques will be helpful in understanding the role of the variations in the earth's orbit in influencing climatic change. At present, however, the data do not support the conclusion of cause and effect relationships.

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Evans and Freeland have criticized our article both conceptually and technically. The conceptual disagreements, and some of the technical ones as well, stem from what are apparently fundamentally different strategies for studying a natural system which is complex and poorly understood. Our strategy is to make quantitative predictions based on the simplest and most general assumption—that the system is linear and time-invariant—and then to test both the orbital hypothesis and that assumption by searching for the predicted frequencies in three climatic spectra. Since frequencies close to those predicted are found in three different spectra, we are willing to conclude that orbital variations do play a significant role in triggering the ice-age succession—and we see no need to postulate frequency-response peaks which coincide by chance with the astronomical frequencies. Furthermore, to allow for the inevitable irregularities in the geological time scale, we pay particular attention to the coincidence between the ratio of two predicted frequencies and the ratio of two frequencies in the climatic record. We also avoid using cross-spectral methods because, as illustrated by the calculations of Evans and Freeland, these are particularly sensitive to time-scale irregularities.

In our opinion, the strategy of Evans and Freeland has some of the disadvantages of a statistical fishing expedition in that it places too much faith in fiducial tests, makes too many demands on the data, and assumes that we know a great deal more than we do about the dynamics of climate.

Our critics suggest as an alternative and "far more convincing" approach that orbital forcing be assumed, a physical model formulated, and transfer functions computed. We welcome such alternative approaches. However, the most ambitious attempt of this kind known to us (*1*) illustrates both the virtues and the drawbacks of this procedure. On the one hand, the development of physically based transfer functions is a scientifically rewarding venture. On the other, the arbitrariness and complexity of the mathematical formulations make them poor vehicles for testing the validity of the astronomical theory. For this purpose, we prefer the frequency-domain approach, and point to the study of ocean tides by way of example. Here, the evidence which led originally to the equilibrium theory was primarily in the frequency domain. Only much later was it possible to model the response of the ocean dynamically.

The technical criticisms of Evans and Freeland center on the question of statistical significance. Before responding in detail, it is appropriate to point out that we have a rather limited regard for the role that confidence intervals actually play in solving fundamental scientific problems. Wherever information is lacking on the underlying structure of a physical process, we "hesitate to make quantitative statistical statements, which would be based on seemingly objective methods, hiding perhaps the weak points in the argument and giving the research worker the illusory feeling of security" (*2*). For the problem in hand, all confidence intervals should be taken merely as a guide to what the general magnitude of the actual noise level might be. As with Mendelian genetics and magnetic reversals, questions about the orbital control of climate will be answered through the repetition of results rather than the outcome of a fiducial test.

Our critics are concerned that the number of lags used exceed one-tenth of the record length. The number of lags was not chosen arbitrarily, but taken as the minimum needed to resolve all three predicted periods (41,000, 23,000, and 19,000 years). As the confidence intervals given reflect the number of lags, nothing is lost and a great deal gained thereby. Since we published our article, Berger (*3*) has verified that periods near the 23,000 and 19,000 years are dominant in precession curves calculated by the methods of celestial mechanics. As a check on the significance of our results, we followed the window-closing procedure recommended by Jenkins and Watts (*4*), and found that the position of the main spectral peaks does not change as the number of lags is gradually increased. This procedure gives a far more convincing demonstration of significance than the application of a 10 percent lag criterion, or any similar, essentially arbitrary rule of thumb. Unfortunately, space constraints prevented publishing these results in our article.

Evans and Freeland point out that we did not explain our use of one-sided confidence intervals. We used them for two reasons. First, we consider them to be the appropriate guide to use in testing a hypothesis which predicts where power should be concentrated, not where it should be absent. Second, we made Monte Carlo simulations to test the accuracy of the formula used for confidence intervals, and found that in a population of 500 spectral estimates the calculated limits matched the results for spectral peaks, but not for troughs.

In our turn, we are puzzled by Evans and Freeland's comments about our prewhitening procedure, which is a standard method of correcting distortions introduced into critical parts of a badly behaved unprewhitened spectrum by the lag window.

To make certain that our results were not artifacts of procedure, our data were processed independently at three different institutions with different programs and mathematical techniques. In addition to the results reported, T. E. Landers applied the maximum entropy method at the Lincoln Laboratory, Massachusetts Institute of Technology, and N. Pisias used a variety of lag windows with a different autocorrelation program at the University of Rhode Island. All results were essentially the same. We see no special virtue in the raw Fourier transform method advocated by Evans and Freeland. In fact, although such a procedure is simpler conceptually and may well be appropriate for time series met with in oceanography, which are long relative to the periods studied, we do not recommend it for the shorter, noisier records of paleoclimatology, where poor sampling properties (*5*) place it at a disadvantage in comparison with the autocorrelation approach. This disadvantage is well illustrated by the calculation of Evans and Freeland (see their figure 1).

Finally, we used the word "pacemaker" in our title to convey the idea that orbital variations control the timing but not the amplitudes of the main ice-age succession. Our concluding statement that "changes in the earth's orbital geometry are the fundamental cause of the succession of Quaternary ice ages" is meant to suggest the same idea. Perhaps we should have emphasized more strongly that our evidence only applies to the last half-million years, and that the mechanisms through which orbital forcing operates, particularly those due to changes in eccentricity, remain a mystery. And we agree with Evans and Freeland that a considerable fraction of the observed variance may result from responses to orbital forcing more complex than we considered.

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