of such a structure is responsible for the first discontinuity on the simulated Hugoniot (*1*). With respect to the following analysis, it is not essential what exactly occurred—recrystallization or melting. Structures in both cases can be indistinguishable (*5*, *6*). The important point is that this is definitely not a solid-solid transition, and there is a discontinuity on the Hugoniot.

4) In my report (*1*), I state that calculations have been checked by doubling the size of the cross section, and no significant changes of results were observed. Because the dependence of the results on the number of particles (in this case, on the number of unit cells in the x and y directions) is asymptotic (the results do not change after exceeding some sufficiently large number), it is clear that if I did not see any changes of the results when doubling the size from five to ten unit cells, then the results would not change with further increase in size. The Lindemann (*7*) criteria for melting is a first approximation, and for Ar it is a good one (*8*). Mean square displacement of atoms [this is within the Lindemann criteria; see (*9*) for details] depends on the number of unit cells in a computational cell of cubic shape (Fig. 1). Ten cells are more than enough for obtaining reliable results. Dubrovinsky's reference (reference 12 in the comment) to the data, without providing details, is not convincing because properties of small particles could be different from bulk properties for several reasons (for example, the structure of adsorbate is that of the adsorbent).

5) The calculated melting temperature from the Hugoniot (that is, one of the points on the liquid Hugoniot) is in agreement with experiments and calculations, and Dubrovinsky is not correct on this point. Ross (*10*, table 5) gives $T = 4235$ K at $P = 22.53$ GPa as a point on the liquid branch of the Ar Hugoniot. This is the highest pressure within the range of my calculations. Figure 2A in my report gives $T = 3700$ K at the same P. Therefore, the difference is about 500 to 600 K, which is definitely less than "at least 2000 K" and can be accounted for by different potentials and methods used in the calculations.

6) Argon and iron are, of course, different substances. In my report, I reasoned that if two discontinuities on the Ar Hugoniot (*1*) do not require an assumption about solid-solid transition, it is not necessary to presume that the two discontinuities on the Fe Hugoniot (*11*) are the result of a solid-solid transition. Two discontinuities can exist without a solid-solid structural transition of the material, in accord with the theory of shock waves in solids. In addition, my simulations (*1*) showed the approximate size of discontinuities one could expect in Ar, which was comparable with the size of discontinuities in Fe (*11*). The corresponding figures showing discontinuities in curves of velocities of rarefaction waves in Ar (*1*) and in Fe (*11*) are similar (*12*). This comparison supports my original conclusion (*1*).

Anatoly B. Belonoshko
Institute of Earth Sciences,
Uppsala University,
Uppsala S-75236, Sweden

REFERENCES AND NOTES

1. A. B. Belonoshko, *Science* **275**, 955 (1997).
2. S. M. Stishov, I. N. Makarenko, V. A. Ivanov, V. I. Fedosimov, *JETP Lett.* **11**, 13 (1970).
3. The potential (*1*) was applied to study in detail melting in Ar (*8*). This is the potential that was applied by Ross *et al.* (*13*). Three potentials (*10*, *13*) differ in one parameter only (stiffness), and the particular choice of the parameter was justified by the particular range of densities. Contrary to the statement by Dubrovinsky, the new potential has been applied both in (*1*) and in (*8*).
4. C. E. Morris, *Shock Waves* **1**, 213 (1991).
5. K. S. Vecchio, U. Andrade, M. A. Meyers, L. W. Meyer, in *Shock Compression of Condensed Matter*, S. C. Schmidt, R. D. Dick, J. W. Forbes, D. G. Tasker, Eds. (Elsevier, New York, 1991), p. 527.
6. R. L. Selinger, R. M. Lynden-Bell, W. M. Gelbart, *J. Chem. Phys.* **98**, 9808 (1993); A. B. Belonoshko and L. S. Dubrovinsky, *Am. Miner.* **82**, 441 (1997).
7. F. A. Lindemann, *Phys. Z.* **11**, 609 (1910).
8. A. B. Belonoshko, *High Pressure Res.* **10**, 583 (1992).
9. B. Winkler and M. T. Dove, *Phys. Chem. Min.* **18**, 407 (1992).
10. M. Ross, *Phys. Rev. A* **8**, 1466 (1973).
11. J. M. Brown and R. G. McQueen, *J. Geophys. Res.* **91**, 7485 (1986).
12. In my report (*1*), I state that it is not *necessary* to have solid-solid transition to get double discontinuity in the case of Fe. If I said that it is *sufficient* to have double discontinuity in the case of Ar (to dismiss the possibility of the solid-solid transition in Fe), then Dubrovinsky's comment would be relevant.
13. M. Ross, H. K. Mao, P. M. Bell, J. A. Xu, *J. Phys. Chem.* **85**, 1028 (1986).
14. A. B. Belonoshko and L. S. Dubrovinsky, *Am. Miner.* **81**, 303 (1996).
15. I am thankful to B. Holian (at LANL) and his colleagues for discussions about my report.

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Intertropical Latitudes and Precessional and Half-Precessional Cycles

A. McIntyre and B. Molfino, in their report about the forcing of Atlantic equatorial and subpolar millennial cycles by precession, suggest that climatic changes in high polar latitudes (which are related to Heinrich iceberg surges) may be caused by events that occur in low latitudes (*1*). This suggestion is based partly on a quasi cycle of about 8400 calendar years that they found in the relative abundance of a tropical marine algae and that they relate to precession. Our comment is about their attempt to give this period an astronomical origin. (*1*, p. 1869).

1) In the intertropical zone, the sun passes directly overhead twice in a year at each latitude, but this does not imply that "[o]ver one precessional cycle, this produces two intervals during which perihelion is coincident with the solstice in Northern Hemisphere summer," as stated by McIntyre and Molfino (*1*, p. 1869). By definition of the precessional cycle, perihelion can coincide only once with the Northern Hemisphere summer solstice during one cycle. What produces half a precession cycle in the tropics can only be explained if we accept the proposition that, in the tropics, the climate is responding principally to the largest maximum of insolation, independently of the date it occurs during the year. At the equator, for example, the sun passes overhead at both equinoxes. The evolution of the daily insolation at the equator at the spring and autumn equinoxes can be graphed (Fig. 1). This insolation is given by [see formula 30 in (*2*)]

$$W^{\text{spring}}_{\text{autumn}}\ \text{equinox} = \frac{S}{\pi}\left(\frac{1 \pm e\cos\tilde{\omega}}{1 - e^2}\right)^2 \qquad (1)$$

where W is the insolation; S is the absolute solar constant, estimated at a distance equal to the semimajor axis of the Earth orbit around the sun; e the eccentricity; and $\tilde{\omega}$ the so-called longitude of the perihelion ($\tilde{\omega} = 0$ when the spring equinox occurs at the perihelion; $\tilde{\omega}$ is currently equal to 282°).

These insolations do not depend at all on obliquity. Their spectrum is dominated by precession [about 23- and 19-kyr (thousand-year) periods], but displays also, with much less power, half-precessional periods (11.5 and 9.5 kyr), eccentricity periods, and combination tones. To a good approximation, equation (1) can be written

$$W^{\text{spring}}_{\text{autumn}} \cong \frac{S}{\pi}\left(1 \pm 2e\cos\tilde{\omega} + \frac{e^2}{2}\cos 2\tilde{\omega} + \frac{5e^2}{2}\right) \qquad (2)$$

Equation 2 shows also that, with an excellent approximation,

$$W^{\text{spring}} \cong \frac{S}{\pi}(1 + e\cos\tilde{\omega}) \qquad (3)$$

$$W_{\text{autumn}} \cong \frac{S}{\pi}(1 - e\cos\tilde{\omega}) \qquad (4)$$

and therefore that the insolations at spring and autumn equinoxes are out of phase by half a precession cycle. Selecting for each date of the past, the largest of the two values, leads to a curve (Fig. 2) that is dominated by half-precessional cycles.

This result conceptually remains valid for intertropical latitudes $\phi : -\varepsilon \leq \phi \leq \varepsilon$, although it is then slightly more complicated because of the additional role played by obliquity outside the equator.

2) We agree that the primary components in the tropics are the precessional periods. But this holds also for high polar latitudes (except for those very close to where the polar night occurs [see figures 10B and 14B in (*2*)].

Moreover, the periods of the two largest amplitude terms in the trigonometrical expansion of climatic precession are 23,716 and 22,428 years (*3*), which average (weighted or not) is about 23,100, not 22,000 years, as considered by McIntyre and Molfino (*1*) with a first harmonic of 11,550 years, not 11,000 years. 22,000 years is much more related to the average period of the precessional parameter itself than to its spectral components. This period is not stable in time and, over the last 60 kyr, we observe its progressive shortening (Fig. 3) : the minima of the climatic precession occur at −61, −33, −12, and +9 kyr, respectively, which leads to periods that are 28, 21, and 21 kyr long, respectively; if we consider the interdistances between maxima (which occur at −47, −22, −1, and +18 kyr), we arrive at 25, 21, and 19 kyr.

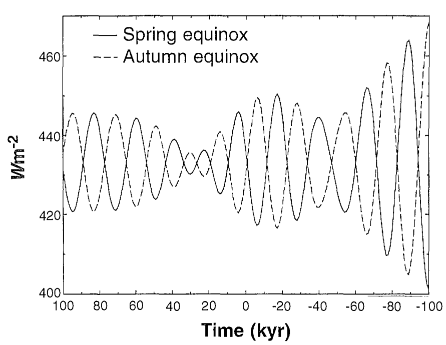


Fig. 1. Long-term variation of the daily insolation at the spring and autumn equinoxes at the equator, 100 kyr BP to 100 kyr AP (*3*).

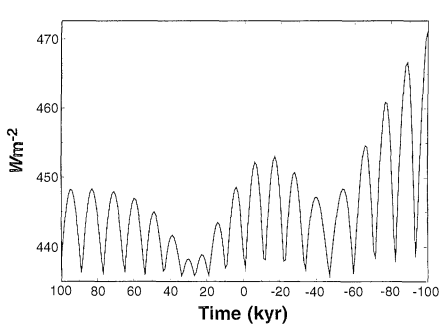


Fig. 2. Long-term variation, 100 kyr BP to 100 kyr AP, of the largest of the two values of the daily insolation at the spring and autumn equinoxes at the equator (*3*); see also (*8*).

One reason for this result is the rather particular behaviour of eccentricity as quoted by the authors [see also (*4*)]. The minima of e occur at −158, −45, and +27 kyr, leading to a length of the period equal to 113 and 72 kyr; the maxima occur at −115, −14, and 87 kyr, leading to two periods of 101 kyr (Fig. 3). Asymmetry, damping, and associated shortening of the eccentricity cycle are therefore important because they influence significantly the length of the precessional cycle. If 22 kyr is important in the finding of McIntyre and Molfino (*1*), we would suggest that it corresponds to an average value of the precessional cycle over the last 45 kyr.

This result is not without having consequences on the spectral components of precession themselves. The eccentricity cycle and all the spectral components of eccentricity are obtained as combinations of the precession frequencies. For example, we see (*5*, p. 638) that the second period of e (94,945 years) comes from the periods of terms 3 and 1 of the expansion of precession:

$$\frac{1}{94{,}945} = \frac{1}{18{,}976} - \frac{1}{23{,}716} \qquad (5)$$

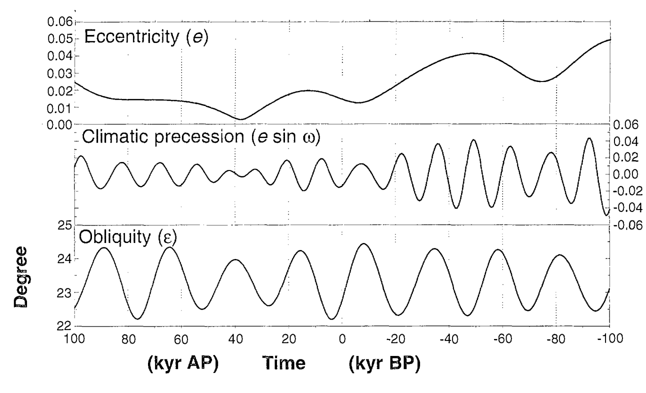
If we consider the shortening of the eccentricity and precession cycles over the period between roughly 100 kyr BP and 30 kyr AP, we may give an astronomical interpretation to the additive combination tone calculated by McIntyre and Molfino. By calculating (1/72 + 1/22 = 1/16.85) they tentatively compute one of the spectral component of precession (16,850 years), provided it is assumed that 72 kyr is the eccentricity period and 22 kyr is the other spectral component of precession.

This result might be tested by analysing the spectra of precession and eccentricity over the last 100 kyr. Although it is not easy to compute accurately a high-resolution spectrum from limited data, we performed a moving maximum entropy spectral analysis [for example, (*6*)] from 100 kyr BP to 100 kyr AP with a window of 60 kyr moving by steps of 5 kyr. For the whole 200-kyr period, the main precessional peaks were found at 24.86, 21.17 and 18.45 kyr (over the last 1 Myr they are 23.71, 22.41, and 18.98). From the moving spectrum analysis, we see a progressive decrease of the main period from 25.36 kyr between 75 and 15 kyr BP to 18.45 kyr between 15 kyr BP and 45 kyr AP, with 21.96 kyr found between 45 kyr BP and 15 kyr AP. For all these spectra, the multitaper method by Thomson (*7*) gives only one significant peak. But because of the considerable shortening and damping of the eccentricity cycle and the exceptional low value of eccentricity now and over the next 50 kyr, this method shows clearly two peaks for the five sub-intervals from (10 kyr BP to 50 kyr AP) to (10 kyr AP to 70 kyr AP). These spectral peaks are (29.7 and 17.5 kyr), (31.15 and 17.4 kyr), (31.63 and 17.39 kyr), (30.68 and 17.47 kyr), and (28.35 and 18.41 kyr), respectively. The negative combination tones leading to the eccentricity cycle, as in equation (5), are, in these cases, 42.6, 39.4, 38.6, 40.6, and 52.5 kyr, respectively, which expresses a considerable shortening of the eccentricity cycle.

Although these results do not give any information about the bipartition of the precessional period over the period of interest by McIntyre and Molfino (the last 40 kyr), they tend to confirm that the additive combination tone they calculated may be related to the spectral components of precession that would, therefore, confirm their proposal that 8.43 kyr is related to precession (*1*).

However, the instability of the precession spectrum and the difficulty to compute it numerically over two precession cycles point to more work to be done. If the eccentricity cycle is larger than assumed by McIntyre and Molfino over the period of interest (let us assume 85 kyr instead of 72 kyr—what our data tend to show), such a length of 85 kyr and a spectral precession component of 16.85 kyr would lead to 21

Fig. 3. Long-term variation of eccentricity, climatic precession, and obliquity between 100 kyr BP and 100 kyr AP. See the damping and shortening of the cycle of eccentricity and precession between now and 50 kyr AP (*3, 9*).



kyr for the other component of precession:

$$\frac{1}{16.85} = \frac{1}{85} + \frac{1}{21}$$

If, on the other hand, we assume that one component is 22 kyr (as did McIntyre and Molfino) and allows for a larger eccentricity cycle, we are left with an additive combination tone that is longer than 16.85 kyr (17.48 kyr):

$$\frac{1}{85} + \frac{1}{22} = \frac{1}{17.48}$$

and leads to 8.74 kyr for the first harmonic, not 8.43 kyr, which was stressed as important. But is that difference really significant? A sensitivity analysis gives a half-tone ranging between 9.3 and 8.1 kyr. Within the range of uncertainty in the data, can we consider that these values are significantly different from 8.43 kyr? We would guess not.

But because, according to our data, the length of the precession cycle can hardly be less than 19 kyr, 16.85 kyr has to be considered as a spectral component of precession, the other one being of the order of 22 kyr. This implies that one big problem remains: Why would this period of precession, which does not appear to be the most important in spectral analysis of precession, play such an important role in the climate of the last 40 kyr?

A. Berger
M. F. Loutre
Institut d'Astronomie et de Géophysique Georges Lemaître (Unité ASTR), Département de Physique, Université Catholique de Louvain, B-1348 Louvain-la-Neuve, Belgium

REFERENCES AND NOTES

1. A. McIntyre and B. Molfino, *Science* **274**, 1867 (1996).
2. A. Berger, M. F. Loutre, Ch. Tricot, *J. Geophys. Res.* **98**, 10,341 (1993).
3. A. Berger, *J. Atmos. Sci.* **35**, 2362 (1978).
4. ______ and M. F. Loutre, *C. R. Acad. Sci.* **323**, 1 (1996).
5. A. Berger, *Rev. Geophys.* **26**, 624 (1988).
6. P. Pestiaux and A. Berger, in *Milankovitch and Climate*, A. Berger, J. Imbrie, J. Hays, G. Kukla, B. Saltzman, Eds. (Reidel, Dordrecht, Netherlands, 1984), pp. 417–446; A. Berger, J. L. Mélice, I. van der Mersch, *Philos. Trans. R. Soc. London A* **330**, 529 (1990).
7. D. J. Thomson, *Philos. Trans. R. Soc. London A* **332**, 539 (1990).
8. T. K. Hagelberg, G. Bond, P. de Menocal, *Palaeoceanography* **9**, 545 (1994).
9. P. Bretagnon, *Astron. Astrophys.* **30**, 141 (1974).

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Response: We agree with Berger and Loutre's first remark. We made a mistake (*1*, p. 1896, column 2, lines 14–17) in transferring the concept from a paper by Short *et al.* (*2*, pp. 169–170). We should have used a direct quote.

We are pleased that Berger and Loutre's computations indicate that our documentation, in the equatorial Atlantic paleoceanographic record, of 1/2 of a combination tone, 8.4 kyr, of eccentricity and precession may be valid. One approach to proving validity of this atypical period would be to examine older intervals of time that are forced by essentially the same orbital geometry.

The comment by Berger and Loutre and our report both indicate that modern insolation-forced climate is influenced by rhythms that differ in period from the primary periods of precession. Accurate prediction of future climate change on millennial scales may require use of these atypical harmonics. A point made in our report, but not addressed in the comment, is that the perceived wisdom of high-latitude, Northern Hemisphere control of climate at millennial scales may in fact be low-latitude modulated.

Andrew McIntyre
Lamont-Doherty Earth Observatory, Columbia University, Palisades, NY 10964, USA

REFERENCES

1. A. McIntyre and B. Molfino, *Science* **274**, 1867 (1997).
2. D. A. Short *et al.*, *Quat. Res.* **35**, 157 (1991).

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