FORUM

Comment

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Rahmstorf et al. [2004], in their "critique" of *Shaviv and Veizer* [2003], assert that the proposed correlation between cosmic ray flux (CRF) and paleoclimate during the Phanerozoic does not "hold up under scrutiny" because its astrophysical background is based on "questionable assumptions" and circular reasoning, and because the meteoritic and terrestrial databases and statistics are manipulated.

They further claim that the *Shaviv and Veizer* [2003] treatment of the CO_2/climate relationship is not scientifically sustainable, and that the oxygen isotope record is likely a proxy of oceanic pH and not of paleotemperature. They make a host of additional assertions that cannot all be restated here.

Due to space restrictions of *Eos*, we cannot explain all disputed points in our response, and the reader is referred to http://www.agu.org/eos_elec/000631e.html and to http://www.phys.huji.ac.il/~shaviv/ClimateDebate/ for the detailed rebuttal.

At the outset, note that the allegations in Rahmstorf et al. concentrate on issues that are not even discussed in *Shaviv and Veizer* [2003]. In that article, we deliberately stated, "we emphasize that our conclusion about the dominance of the CRF over climate variability is valid only on multimillion-year time scales. At shorter time scales, other climatic factors may play an important role....", precisely to avoid being drawn into the divisive, politicized debate about global change. Unfortunately, the "offending" issue is not *Shaviv and Veizer* [2003], but the simple notion that there may be a potential alternative, or complementary, force to CO_2 as the principal driver of climate.

We are all well versed in the standard Intergovernmental Panel on Climate Change (IPCC) greenhouse scenario that clearly has some merits. At the same time, a slew of recent empirical observations demonstrates convincingly that climate in the past correlated with the abundance of cosmogenic nuclides and solar/celestial parameters. These publications [e.g., *Bond et al.*, 2001; *Neff et al.*, 2001; *Solanki*, 2002; *Rind*, 2002; *Foukal*, 2002; *Carslaw et al.*, 2002; *Usoskin et al.*, 2003] provide a more objective and definitive view of the subject than *Laut* [2003], an article that is challenged in *Marsh and Svensmark* [2003] and at www.dsri.dk/response.

The discussion of the cosmic ray flux in *Rahmstorf et al.* [2004] is based simply on incorrect premises. For example, the reconstructions in *Shaviv* [2002, 2003] were based on all K-dated meteorites that were reduced to 50 "heterogeneous" groups, and the calculated periodicity of 147 ± 10 Ma is confirmed also by the new data based on ^{36}Cl exposure ages

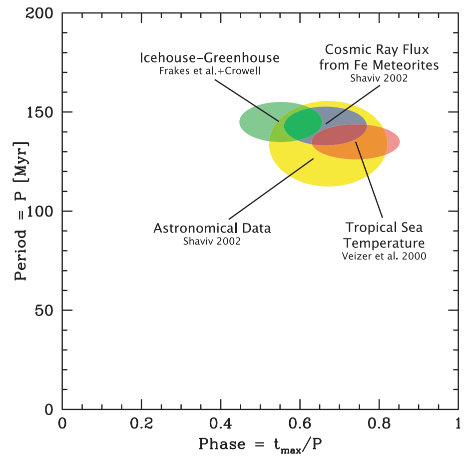

Fig. 1. Two extraterrestrial "signals" have the same periodicity and phase as two independent terrestrial records. The celestial/climate link is both statistically significant and redundant.

(http://www.phys.huji.ac.il/~shaviv/ClimateDebate/). The age clustering is therefore unlikely to single out broken-apart meteorites, yet this celestial signal still correlates with climate.

The alleged large discrepancies in ages arise from the fact that *Rahmstorf et al.* [2004] failed to realize that the time axis in the Shaviv publications is the K-exposure age, not the "real time" of the CRF flux. In contrast to their statement, note that the CRF is expected to have been variable also from astronomical theory and observations, with the same period and phase, and independent direct evidence shows that it varied for at least the last 10 Ma. The remaining, alleged, smaller temporal discrepancies are all well within the 2σ uncertainties of the four data sets discussed in *Shaviv and Veizer* [2003] (Figure 1). Since these data sets are independent of each other, no circular reasoning is involved.

The 50 Ma window for statistical evaluation of oxygen isotope data was already selected in *Veizer et al.* [1999, 2000] because this was a realistic window for the data set with a temporal resolution of ~10 Ma, and not because of telepathic anticipation of Shaviv's publications. The correlation of this δ^{18}O trend with the CRF (Figure 2 of *Shaviv and Veizer* [2003]) is real, as evidenced by a simple naked eye inspection, irrespective of the CRF curve utilized or the statistics chosen. We emphasize that the astrophysical and geological studies and curves were published entirely independently, without knowledge of each other's research and existence. Without entering into arguments about specific degree Celsius changes resulting from CO_2 doubling, let us first point out that none of the proposed CO_2 trends in Figure 1 of *Shaviv and Veizer* [2003] showed any correlations at all with the paleotemperature or paleoclimate trends. This pertains particularly to the GEOCARB III model of atmospheric CO_2 over Phanerozoic time that is reproduced in the summary chapter of the IPCC (their Figure 10).

The latest inventories of sedimentary climate indicators (www.scotese.com/climate.htm; *Boucot and Gray* [2001]) show a similar temporal pattern to that in our δ^{18}O data, and their comparison with CO_2 curves would lead to similar conclusions. On this basis alone, we would have been justified in concluding that either CO_2 plays only a limited role in Phanerozoic climate evolution, or that the CO_2 model estimates do not reflect the reality.

Instead, we took a very conservative approach in order not to discount the role of CO_2, by first assigning the entire unexplained residual to CO_2, and afterwards, estimating the potential error in the "explained" data, we assigned even this to CO_2. It is in this way that we derived a likely tropical temperature increase of less than 1°C and an upper limit of ~2°C for CO_2 doubling.

This was followed by further caveats, such as, that the global average could have been 1.5 × this value, or even doubled for an unlikely scenario of no ice correction; and by a final qualification that these propositions are valid only on a multimillion-year time scale. In our view, this is a reasonable treatment of the data. A more specific response is given in http://www.phys.huji.ac.il/~shaviv/ClimateDebate/.

The detrended δ^{18}O of calcareous shells reflects the ambient temperature of seawater and the quantity of water locked in the polar ice caps, each contributing about one half to this signal. As pointed out by *Rahmstorf et al.* [2004], it has lately been realized that seawater pH drives the δ^{18}O of shells in the opposite direction. *Royer et al.* [2004] utilized this observation to reconcile the GEOCARB III and the δ^{18}O trend of *Veizer et al.* [2000], assuming that any discrepancy of the two variables is due to pH.

This is an interesting proposition that may have some merit, but there is a price to pay. To explain the recurrence of cold intervals at times of apparent high atmospheric CO_2, such as most of the Paleozoic and the mid-Mesozoic, one has to resort to a multitude of special pleadings. The Ordovician glaciation at ~5000 ppm atmospheric CO_2 is a classic example.

Moreover, note that this correction is entirely arbitrary, because we do not have any constraints for the pH of Phanerozoic seawater, except possibly some boron isotopes for the youngest portion of this record. From geological considerations, there is no a priori reason why the greenhouse oceans should have been more acidic than their icehouse counterparts, but there are good reasons for them to be warmer.

Note also that the model of *Royer et al.* [2004] does not consider the mitigating "ice volume" effect arising from waning and waxing of ice sheets. If included, the required pH correction (and the GEOCARB III CO_2 levels) would have to be approximately doubled for CO_2 forcing to reach a par with the CRF. For all these reasons, we argue that the δ^{18}O trend is still chiefly a reflection of the temperature history of the past oceans.

In this response, we are not commenting on caveats such as aerosols, other greenhouse gases, lags, feedbacks, ice sheets, etc. The topic of *Shaviv and Veizer* [2003] was the "primary" climate driver on Phanerozoic time scales, with no space, or need, for any more discussion than that. Furthermore, we fail to see how any of the above would make CO_2 the "driver" in the Antarctic cores, when the temperature rises preceded those of CO_2 by centuries. We not only never denied but specifically highlighted the qualifying proposition that CO_2 may act as an amplifier.

In conclusion, the above response demonstrates that the "critique" of *Rahmstorf et al.* [2004] has little substance, in addition to the fact that it deals with time scales that are not even discussed in *Shaviv and Veizer* [2003]. Moreover, the statistical argument advanced in this issue of *Eos* as disproving the validity of the CRF/paleotemperature correlation is simply invalid (for details, see http://www.phys.huji.ac.il/~shaviv/ClimateDebate/).

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—NIR SHAVIV, Racah Institute of Physics, Hebrew University of Jerusalem, Israel; and JAN VEIZER, Institut für Geologie, Mineralogie und Geophysik, Bochum, Germany, and Ottawa-Carleton Geoscience Centre, University of Ottawa, Ontario, Canada

Reply

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In our analysis [*Rahmstorf et al.*, 2004], we arrived at two main conclusions: the data of *Shaviv and Veizer* [2003] do not show a significant correlation of cosmic ray flux (CRF) and climate, and the authors' estimate of climate sensitivity to CO_2 based on a simple regression analysis is questionable. After careful consideration of Shaviv and Veizer's comment, we want to uphold and reaffirm these conclusions.

Concerning the question of correlation, we pointed out that a correlation arose only after several adjustments to the data, including shifting one of the four CRF peaks and stretching the time scale. To calculate statistical significance, we first need to compute the number of independent data points in the CRF and temperature curves being correlated, accounting for their autocorrelation. A standard estimate [*Quenouille*, 1952] of the number of effective data points is

$$N_{EFF} \cong \frac{N}{1+2\sum_{k=1}^{N} r_1(k)\, r_2(k)}$$

where N is the total number of data points and r_1, r_2 are the autocorrelations of the two series. For the curves of *Shaviv and Veizer* [2003], the result is $N_{EFF} = 4.8$. This is consistent with the fact that these are smooth curves with four humps, and with the fact that for CRF, the position of the four peaks is determined by four spiral arm crossings or four meteorite clusters, respectively; that is, by four independent data points. The number of points that enter the calculation of statistical significance of a linear correlation is (N_{EFF} - 2), since any curves based on only two points show perfect correlation; at least three independent points are needed for a meaningful result.

Shifting one of the four peaks to fit climate data reduces the number of independent points by one, and tuning the time scale to improve the fit uses up another degree of freedom, leaving between zero and one independent points in the significance calculation. Hence, no correlation is significant after the tuning steps of *Shaviv and Veizer* [2003]; given the few degrees of freedom in the data, the data were over-tuned. The fact that their tuning is within data uncertainty is irrelevant to statistical significance. It just means that a correlation might be possible without contradicting the data.

The consistency of the periods presented is still not convincing, since these periods are only averages of a few points with high variability. While it is possible that better data will demonstrate a correlation of cosmic rays and climate, our conclusion is that the data presented by *Shaviv and Veizer* [2003] are insufficient for this. As an aside, we did not confuse the exposure ages and real ages of meteorites.

Concerning the regression analysis to estimate climate sensitivity, Shaviv and Veizer write in their Comment, "we are not going to comment on caveats such as aerosols, other greenhouse gases, lags, feedbacks, ice sheets, etc." This is unfortunate, since these issues are not caveats, but central to the determination of climate sensitivity to CO_2. As we pointed out, the strength of any individual forcing factor can only be estimated by a regression analysis if it is statistically independent from other forcings, which is very unlikely for the examples mentioned, or if these other forcings are explicitly taken into account, as in *Lorius et al.* [1990]. Since this was not done, we maintain that the regression is questionable.

Finally, it is worth pointing out areas of agreement.

Shaviv and Veizer state, "we fail to see how any of the above would make CO_2 the 'driver' in the Antarctic ice cores." We fully agree that CO_2 is not the driver of the climate variability seen in these cores. There is a host of excellent empirical evidence and widespread agreement that climate variability on glacial-interglacial time scales is driven by variations in the Earth's orbit, the Milankovich cycles, with CO_2 responding as a positive feedback.

The earliest analysis of Antarctic cores, and the derivation of climate sensitivity from these data, was already based on this premise (see *Lorius et al.* [1990]). Hence, climatologists have long expected a time lag of CO_2 behind temperature in the ice core data, and some of us were involved in pioneering the measurement of this lag using a gas-based temperature proxy that resolves the problem of the age difference between gas bubbles and the surrounding ice [*Caillon et al.*, 2003]. The result is a lag of 800 years at termination III (240,000 yr B.P.), a warming that occurred over a 5000-yr period.

This means that one-sixth of the warming at the end of this glacial period occurred before the CO_2 feedback started to be felt. This is consistent with recent climate model simulations of glacial cycles, which show that CO_2 changes are not required to explain the initiation of glaciation or deglaciation, but that the CO_2 feedback is needed to explain their full extent [*Yoshimori et al.*, 2001; *Meissner et al.*, 2003]. The time lag in ice core data gives no information about the climate sensitivity to a given CO_2 change, such as that caused by anthropogenic emissions.