Detailed Response to “Cosmic Rays, Carbon Dioxide and Climate” by Rahmstorf et al.

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Rahmstorf et al. [2004] published a critique to the ariticle “Celectial driver of Phanerozoic Climate?” [Shaviv & Veizer, 2003]. We show here why the various criticism raised are either irrelevant or erroneous. Thus, the conclusions reached by Shaviv & Veizer [2003] are still valid. In particular, the dominant climate driver on the multi-million year time scale is the variable cosmic-ray flux. CO_2 is important, but it likely plays a secondary role in determining the climate.

An abridged reply to the Rahmstorf et al. critique was accepted and will soon be published in Eos. Since Eos places a strict page limitation, we present here the unabridged reply. The text of the Rahmstorf et al. critique is indented and *italicized* while our response is not.

> *—Several recent papers have applied correlation analysis to climate-related time series in the hope of finding evidence for causal relationships. For a critical discussion of correlations between solar variability, cosmic rays and cloud cover see [Laut, 2003].*

Marsh and Svensmark [2003] provide the state of the art summary of the solar activity → cosmic-ray → cloud-cover relationship that disclaims the statements of Laut [2003] concerning an older dataset (see also `http://www.dsri.dk/response`).

> *—A prominent new example is a paper by Shaviv & Veizer [2003] (henceforth called SV03), which claims that fluctuations in cosmic ray flux reaching the Earth can explain 66% of the temperature variance over the past 520 million years (520 Myr), and that the sensitivity of climate to a doubling of CO_2 is smaller than previously estimated.*
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> *Shaviv and Veizer's paper was accompanied by a press release titled “Global warming not a man-made phenomenon”, in which Shaviv is quoted stating: “The operative significance of our research is that a significant reduction of the release of greenhouse gases will not significantly lower the global temperature, since only about a third of the warming over the past century should be attributed to man”.*

In our view, public relation releases should not be a part of, or justification for, publications in serious scientific literature. If Rahmstorf et al. believe we have made mistakes in our scientific analyses, they should concentrate on specific scientific points, and not on PR statements. The professional practitioners of PR proclamations should take into account that they too live in glass buildings.

The above notwithstanding, a recent analysis by Shaviv [2004] which includes the comparison between the change in the radiative forcing and temperature change over 6 different time periods (The Phanerozoic, the Cretaceous, Eocene, Last Glacial Maximum, Past Century and the Solar Cycle) yields that all time scales are consistent with a sensitivity of 1.15 ± 0.25°C (0.62 to 1.86°C at 99% confidence), which is lower than the values obtained in GCMs ($1.5-5$°C). The estimated 1.4 ± 0.4 W/m^2 of warming attributable to the increased solar luminosity and reduced CRF since 1900 should have therefore contributed about 0.32 ± 0.11°C, or roughly half of the observed global warming.

The low sensitivity obtained over different time scales is clearly below the large range obtained in Global Circulation Models. This implies that (a) Earth has shown us that the GCMs do not predict the global sensitivity accurately (This is most likely because of our poor understanding of cloud feedback [Cess et al., 1989]), and (b) Even if we halved the CO_2 output, and the CO_2 increase by 2100 would be, say, a 50% increase relative to today instead of a doubled amount, the expected reduction in the rise of global temperature would be less than 0.5°C. This is not significant. Thus, the secondary role of CO_2 and lower implied climate sensitivity, as shown by SV03 and corroborated with more research does imply that a “significant reduction of greenhouse gases will not significantly lower the global temperature”.

We should point out that we fully support the effort to cut in the emissions and accelerate the development of alternative energy sources, simply because of real pollution and resource conservation considerations, but this effort should be rational and based on sound scientific research. As openly admitted in the German/Swiss “manifesto”, publicly released by the Potsdam-Institut für Klimafolgenforschung (24.10.2003), the attack on SV03 is motivated mostly by political considerations.

I. RECONSTRUCTING COSMIC RAY FLUXES

—The starting point of SV03 is a reconstruction of cosmic ray fluxes over the past 1,000 Myr based on 50 iron meteorites and a simple model estimating cosmic ray flux (CRF) induced by the Earth's passage through Galactic spiral arms ([Shaviv, 2002; Shaviv, 2003]). About 20 of the meteorites, making four clusters, date from the past 520 Myr, the time span analysed in SV03. The meteorites are dated by analysing isotopic changes in their matter due to cosmic ray exposure (CRE dating [Eugster, 2003]). An apparent age clustering of these meteorites is then interpreted not as a collision-related clustering in their real ages but as an indication of fluctuations in cosmic ray flux (CRF).

One difficulty with this interpretation is that variations in CRF intensity would equally affect all types of meteorites. Instead, the ages of different types of iron meteorites cluster at different times [Wieler, 2002]. Hence, most specialists on meteorite CRE ages interpret the clusters as the result of collision processes of parent bodies, as they do for stony meteorites (ages ≤ 130 Myr) to which more than one dating method can be applied.

It is certainly true that the complete meteoritic data includes clusters of meteorites of the same type, and that such clusters are most likely the result of a single parent body breaking up into many small pieces, but this is totally irrelevant. As detailed in Shaviv [2002] and Shaviv [2003], in order to neutralize this effect, a modified meteoritic data set is generated (using 80 K-dated Iron Meteorites) where clusters of meteorites of the same Iron group classification are replaced with one having an average age. Thus, the clustering can either be because of a variable CRF, or, simply because parent bodies tend to break up more often periodically. However, it is not likely that single bodies generated each of the clusters, since each cluster is now comprised of meteorites that are all of different Iron group classification.

Irrespective, even if the CRF were constant, and even if the origin of the clusters were single heterogeneous asteroids, each giving rise to a heterogeneous cluster, we still find that the periodic pattern in the "celestial" signal correlates with the pattern in the terrestrial one!

Moreover, independent evidence in the Iron meteorite data, based on comparison of different exposure dating methods, clearly shows that the CRF over the past 10 Ma must have been 30% higher than was the average over the past 1000 Ma [Lavielle et al. 1999]. If it was variable recently, it is unlikely that it was constant before. Plus, the astronomical understanding of the origin and diffusion of cosmic rays in the galaxy predicts that the

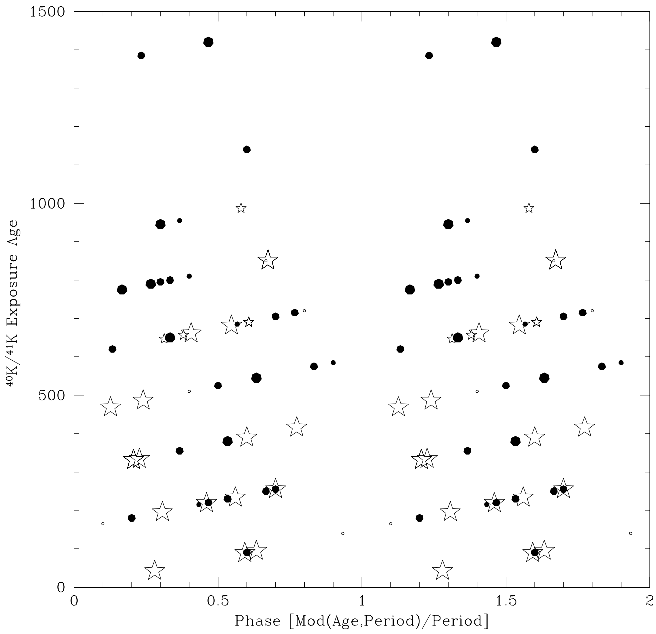

FIG. 1 The exposure age of Iron Meteorites (Chondrites have short exposure ages and are therefore useless for CRF reconstruction over the 1000 Ma time scale), plotted as a function of their phase in a 150 Ma period. The dots are the ^{40}K exposure ages (larger dots have lower uncertainties), while the stars are ^{36}Cl measurements. The K measurements do not suffer from the long term "distortion" arising from the difference between the short term (10 Ma) CRF average and the long term (1000 Ma) half life of K [Laveielle et al. 1999]. However, they are intrinsically less accurate. To use the Cl data, we need to "correct" the exposure ages to take into account this difference. We do so using the result of Lavielle et al. [1999]. Since the Cl data is more accurate, we use the Cl measurement when both K and Cl are available for a given meteorite. When less than 50 Ma separates several meteorites of the same Iron group classification, we replace them with their average in order to discount for the possibility that one single parent body split into many meteorites. We plot two phases so that the periodicity will be even more pronounced. We see that meteorites avoid having exposure ages with given phases. The signal is evident for the past Eon, including just the Phanerozoic. Using the Rayleigh Analysis, the probability of obtaining a signal with such a large statistical significance as a fluke from random events, with any period between 50 and 500 Ma, is less than 0.5%, while the periodicity found is 147 ± 6 Ma.

CRF should be variable. It is therefore not surprising that it is observed, as predicted, in the meteoritic data.

The periodicity in the exposure ages of meteorites, which includes now also exposure ages based on ^{36}Cl, is described in figure 1. As clearly evident from the figure, the meteorites cluster periodically. This is highly unlikely to be a random fluke.

Last, a periodicity in CRF is predicted also by the current astronomical theory. Summing up, we did not use only 20 meteorites to reconstruct the CRF. We used all K-dated meteorites (80 reduced to 50 "heterogeneous" ones) to obtain the most accurate signal possible (147 ± 10 Ma) in order to compare it with climate variations. The fact that just the subset of meteorites with ages less than 520 Ma reveals the same clustering (albeit with reduced statistics), implies that it is valid to assume that the periodic signal obtained for 0-1000 Ma is valid also for just the 0-520 Ma period.

> *—Another problem of the CRF reconstruction is the presumption of "periodicity" of the clusters. The time spans between the clusters' gaps, which correspond to high CRF in their theory, are roughly 90, 90, 140, 130, 190, 140 Myr (Fig. 4 of [Shaviv, 2003]). The claim that these data support a periodicity of* 143 ± 10 *Myr seems not obvious.*

See figure 1, which vividly demonstrates the periodicity. Together with the ^{36}Cl exposure dating, the fit is now even better, with a periodicity of 147 ± 6 Ma.

> *—The passage through the four galactic arms should be a regular process; the high variability of the age gaps is not addressed.*

If Rahmstorf et al. would have taken the time to study Shaviv [2003], which they obviously did not, they would have found Table 2 in Shaviv [2003] which addresses the theoretical uncertainty in the prediction of the spiral arm passages and the uncertainly in the paleoclimatic data determining the peak of the cold periods. In addition, there is an intrinsic measurement error when estimating the difference between two adjacent spiral arm crossings when using the meteiritic data. If one looks at the bottom panel of fig 4 in Shaviv [2003], where the clusters are seen by eye, one can measure by hand that the differences between the mid-points of the clusters, these are: 80, 115, 155, 150, 150, 135 Ma. The width of each cluster is about 70 Ma. Therefore, the error in the determination of a single difference is about $(70\ \mathrm{Ma}/2)\sqrt{2} = 50$ Ma. Compounded to that, one has to add the natural 'jitter' in the spiral arm passage (due to the solar system's epicyclic motion, orbital parameter diffusion and internal structure of the spiral arms, [Shaviv, 2003]). Thus, one finds that the differences are all consistent with their average. The variability is neither larger nor smaller than should be expected given the number of meteorites available.

> *—The CRF model is based on the assumption that cosmic ray density should be concentrated in the Galactic spiral arms, with a time lag of peak CRF of about 15 Myr behind the spiral arm passage. CRF is computed by a simple diffusion model with several free parameters. These parameters are constrained by 'observational constraints', including the meteorite data. These constraints are very weak; the crucial cosmic ray diffusion coefficient can only be constrained to within two orders of magnitude.*

Whether the Astronomical data form weak constraints or not is a vague definition. The astronomical constraints alone do indicate that the CRF should have been variable, that the period should be 135 ± 25 Ma, that the CRF should peak at 31 ± 8 Ma after the spiral arm passage, that the last passage was at about 50 Ma before present, that the CRF had amplitude variations between a factor of 2 to 10. Clearly they are not trivial. Thus, perhaps with the exception of the total amplitude of the variations, the astronomical data does place meaningful constraints on the CRF variability.

> *—Moreover, even the best-fit CRF model does not fit the meteorite data well. For the time span analysed in SV03, the cluster gaps are located near 100 Myr, 190 Myr, 280 Myr and 420 Myr BP (Fig. 4 of [Shaviv, 2003]); they are supposed to coincide with CRF maxima which the best fit' model locates at about 30 Myr, 170 Myr, 360 Myr and 470 Myr BP. This is hardly a good agreement, with an r.m.s. deviation of 60 Myr. Agreement of the three CRF minima (at 80 Myr, 250 Myr, 420 Myr BP) with the age clusters (at 140 Myr, 250 Myr, 360 Myr BP) is hardly better, with two of the three clusters off by almost half a period.*

A careful study of the Shaviv [2003] paper would have revealed that indeed the meteoritic ages are supposed to cluster around epochs with a lower CRF. However, the "time" axis is the K exposure age and not the real age. In other words, Rahmstorf et al. failed to understand that they were comparing K-ages of the clusters to the real ages of geologically warm periods. Since there could be a distortion of up to half a period (depending on the phase of the current epoch) between the K-age and a real age, it is wrong to compare the exposure ages directly to the occurrence of ice-age epochs or to the reconstructed CRF in "real time". This is the reason why the histogram of exposure ages was predicted based on the geological periodicity and compared with the data (i.e., all done in K-CRF exposure time), in which case all the clusters' K-age peak exactly as predicted, within the measurement (i.e., dating) and physical (e.g., epicyclic motion) errors.

Moreover, the largest discrepancy is with the first cluster, but this arises because Voshage & Feldman [1979] excluded from their data base young meteorites, because their method did not date them well enough. Once more meteorites are included (using the ^{36}Cl data) there is no statistically significant discrepancy between any of the clusters and their predicted location.

> *—The only apparent similarity between the CRF model and the meteorite data is the average of the periods.*

Not correct, see above.

> —*The large uncertainty about the timing of spiral arm crossings and the associated CRF maxima is corroborated by the fact that another recent paper ([Leitch and Vasisht, 1998]), which uses the spiral arm crossings to explain biological extinctions, places these crossings at completely different times.*

A comparison with Leitch and Vasisht [1998] shows that their estimate for a spiral arm pattern speed is based on basically two pattern speed measurements. Shaviv [2003] shows that there are more than a dozen estimates for the galactic pattern speed and they cluster around two values. Moreover, it was also shown in Shaviv [2003] based on the standard spiral density wave theory [e.g., Binney & Tremaine, 1988] and the simple observational fact that the Milky Way's outer set of 4 spiral arms extends to about twice our galactocentric radius, that the following conclusions are straightforwardly obtained:

1. One of the clusters of pattern speed measurements should be correct (and it corresponds to a spiral arm passage frequency of 135 ± 25 Ma). A higher pattern speed would require the outer four arms to terminate closer in, while a lower pattern speed would require their inner radius to be further out than our galactocentric radius.
2. A second set with a second pattern speed should exist to explain the inner spiral structure, which cannot be explained by the set of four outer arms. Its pattern speed could very well be the second cluster of numbers obtained.

A recent study by Levi & Shaviv (in preparation) of stellar cluster age distribution corroborates these two conclusions, and also places a limit on the inner spiral arms' pattern speed. To conclude, the astrophysical data does clearly support the existence of a periodic spiral arm passage with the above period (and a phase) that are consistent with the appearance of ice-age epochs on Earth. Rahmstorf et al. failed to bring any reasoning to refute this conclusion, they simply cited earlier work, which neither surveyed the astronomical literature for additional measurements, nor analyzed the astronomical data in the context of the standard spiral density wave theory.

> —*The final parameter choice of the CRF model shown in Fig. 10 of [Shaviv, 2003] is that "which best fits the ice age epochs", i.e., the cosmic ray model has already been fitted to climate data. This circular reasoning compromises the significance of any subsequent correlation with climate data.*

The fact that Fig. 10 of Shaviv [2003] plots the CRF "which best fits the ice-age epochs" is irrelevant for the SV03 paper. In SV03, we state that the period obtained

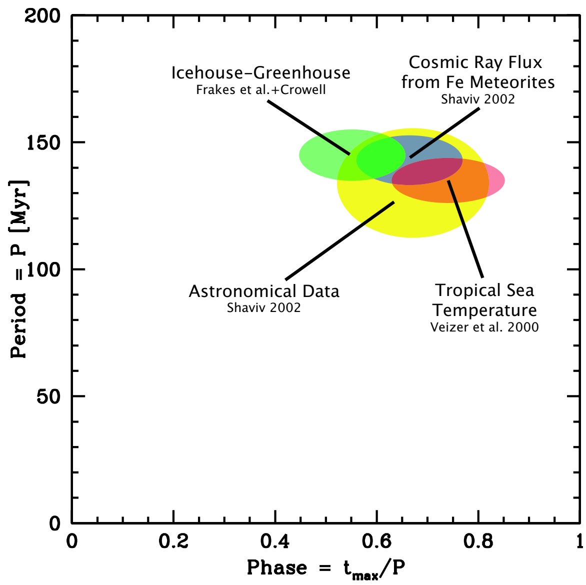

FIG. 2 Two extraterrestrial "signals" have the same periodicity and phase as two independent terrestrial records.

from the meteoritic data is 147 ± 10 Ma (just the meteoritic data!). The reconstructed CRF we used in SV03 is based on this number. The range of CRF reconstructions permissible given the meteoritic data is plotted in fig. 2 of SV03. This range has nothing to do with the climate data whatsoever. i.e., we did not take from Shaviv [2003] a "CRF reconstruction (or reconstructions) which were fitted to the climate data", as Rahmstorf et al. *wrongfully claim.*

When we performed the statistical comparison in SV03, between the reconstructed CRF and the reconstructed temperature, we had to consider the errors in the meteoritic data. We address this point below.

The fact that the celestial signals correlate with the terrestrial ones without any "circular reasoning" is best seen in fig. 2. The figure describes the crux of our result. Two independent celestial data sets (astronomical and meteoritic) give periodic signals with consistently the same phase and period as the two independent geological data sets (sedimentation and isotopes).

II. CORRELATING COSMIC RAY FLUXES TO SURFACE TEMPERATURE

> —*Next, SV03 correlate a CRF reconstruction with a reconstruction of sea surface temperature based on oxygen isotope data from calcite shells from various low-latitude sites. The temperature proxy data were detrended and smoothed with a 50 Myr window to emphasise variations on the ~150 Myr period of the CRF model.*

The detrending, the running means and the calculated temperature trends, including the 10/50 one, were already published in Veizer et al. [1999, 2000], in total ignorance of Shaviv's future work. Moreover, given the intrinsic measurement error in the exposure age data, any variations on time scales shorter than 50 Ma would have to have been smoothed for any proper comparison between the two signals.

> *—The CRF model used in SV03 (shown in Fig. 2 of SV03 as a blue line) is not the same as either of the two different CRF curves shown in [Shaviv, 2003], even though this publication is given as its source. The CRF curves shown in Fig. 7 and Fig. 10 of [Shaviv, 2003] have a CRF maximum near 360 Myr, while that shown in SV03 has a maximum near 320 Myr. [Shaviv, 2003] argues that such a shift of this peak is within the observational uncertainty of the position of the Norma Galactic spiral arm and would "increase the agreement" with climate data.*

Rahmstorf et al. cited Shaviv [2003] which stated that shifting Norma's arm would "increase the agreement" with the climate date. However, Rahmstorf et al., failed to mention that Shaviv [2003] gave the physical reasoning why this asymmetric location of the arm should be suspected. Yet, despite this reasoning, the analysis in Shaviv [2003] assumed a conservative asymmetric location.

Given the current understanding of the spiral arm structure, which has improved since the first work of Shaviv [2002, 2003], it is now wrong to assume that the preferred position of the Norma arm is asymmetrically located between the two adjacent ones. This was an artifact of the location of Norma's arm in the original Taylor and Cordes galactic spiral arm model, extrapolated to our galactocentric radius (see Leitch and Vasisht [1998] and Shaviv [2003]). Such extrapolation is required because the part of the arm at our radius cannot be observed through the center of the galaxy. This procedure assumes that the same set of spiral arms extends from small galactic radii to twice our galactocentric radius. Now that it is evident that there should be at least two sets of arms (see above), this artifact should be removed, and the preferred crossing of the "outer" Norma arm (which clearly is unrelated to the "inner" Norma arm) should have most likely taken place at around 320 Ma, symmetrically between the adjacent arms. There are currently no good physical reasons to place the Norma arm at the location used by L&V and Shaviv [2003].

> *—SV03 then arbitrarily change the time scale in the reconstruction to obtain yet another CRF curve (the red curve in Fig. 2 of SV03), which they call "fine tuned to best fit the low-latitude temperature". This third tuning step shifts the third CRF maximum by another ∼20 Myr to near 300 Myr. This CRF maximum has thus been shifted by ∼60 Myr, almost half a period, compared to those shown in [Shaviv, 2003].*

Since the preferred passage was at 320 Ma, the 20 Ma remaining discrepancy is hardly a cause for concern. For example, just the jitter in the prediction of the spiral arm passage due to our epicyclic motion is 12 Ma (see Appendix A of Shaviv 2003). Moreover, since we are using a different "time base" for both signals, we must allow for fine-tuning as described below.

> *—The correlation between this final cosmic ray curve and the temperature record is r = 0.81 for an "explained variance" of 66%. However, the CRF curve before this final "fine-tuning" (i.e., the "untuned" blue curve in Fig. 2 of SV03) explains only 30% of the variance, which is statistically indistinguishable from zero.*

1. The Pearson correlation coefficient obtained when comparing the ^{18}O data to the most likely CRF reconstruction using the meteoritic data is r = 0.55 (i.e., assuming a period of exactly 147 Ma—the "untuned" blue curve of Fig. 2 of SV03). It is hard, however, to interpret the statistical significance, since the correlation is clearly not linear (see figure 1 of Shaviv [2004]). We therefore use the non-parametric Kendall τ statistic, which assumes nothing on the distribution of data or on the functional dependence between the two. We do so on the ^{18}O data before it was smoothed with a 50 Ma window (to get independent measurements) and the "raw" CRF reconstruction, and obtain that the null hypothesis, namely, that the data sets are uncorrelated, can be ruled out at the 99.1% confidence level. This is "statistically distinguishable from zero".

2. Having shown that the data sets are statistically correlated, any comparison between the CRF and ^{18}O must allow for the fact that the periodicity in the CRF is known to within an uncertainty range. Taking this into account, "We find that at least 66% of the variance in the paleotemperature trend could be attributed to CRF variations likely due to solar system passages through the spiral arms of the galaxy."

3. To put this in perspective, we can look at the routine comparison between sedimentation records (ice or sea cores) to orbital forcing as was done more than once by some of the authors in Rahmstorf et al. In this case, the orbital forcing and sedimentation records do not share the same time base—the orbital forcing is known accurately, while the sedimentation rate varies with time. As a consequence, the sedimentation record is tuned to the

orbital forcing, that is, a time varying correction is applied, which can shift peaks or troughs in the data by as much as *one half of the longest of the orbital periods.* In our case, the correction is invariant (i.e., only one number is applied), the CRF data is known to have an accuracy of $\pm 7\%$, and the actual correction is only -5%. Moreover, in our case here, the null hypothesis of no correlation can be ruled out also for any tuning (see above). This is not the case with orbital forcing.

> —*We thus find that there is no significant correlation of the CRF curve from Shaviv's model and the temperature curve of Veizer, even after one of the four CRF peaks was arbitrarily shifted by 40 Myr to improve the fit to the temperature curve. There also is no significant correlation between the original meteorite data and the temperature reconstruction. The explained variance claimed by SV03 is the maximum achievable by optimal smoothing of the temperature data and by making several arbitrary adjustments to the cosmic ray data (within their large uncertainty) to line up their peaks with the temperature curve.*

As shown above:

1. There is a significant correlation between the CRF curve (this is the case even if one artificially restricts the CRF period to be the most likely one from the meteoritic record, disregarding the error arising from the time base differences).

2. Norma's arm location was not arbitrarily moved. The reason is described at length, both above and in Shaviv [2003]. To support the asymmetric location of Norma's arm, Rahmstorf et al. would have to explain how a single set of spiral arms can extend from a few kpc to twice our galactocentric radius (a fact which wasn't realized previously, including by Shaviv [2002]), clearly penetrating beyond either the inner or the outer Lindblad resonances, counter to spiral density wave theory [Binney & Tremaine 1988].

3. The meteoritic clusters are located around the warm epochs on Earth. Yet to do the comparison both the clusters and the climate signal have to be compared using the same time axis (either real time, or Potassium exposure age) otherwise erroneous conclusions are obtained.

4. There were no arbitrary adjustments. The maximum explained variance was obtained with a CRF history that is consistent with the meteoritic data. It explains 75% of the temperature variance (if the δ^{18}O measurement error is accounted for). The period obtained from the meteoritic data was 147 ± 10 Ma (hence uncertainty of 7%) and the agreement was also in phase. Moreover, the meteoritic data is corroborated by the independent astronomical data.

III. REGRESSION OF CO_2 AND TEMPERATURE

> —*The final argument of SV03 – that CO_2 has a smaller effect on climate than previously thought is based on a simple regression analysis of smoothed temperature and CO2 reconstructions. SV03 conclude that the effect of a doubling of atmospheric CO_2 concentration on tropical sea surface temperatures (SST) is likely to be* 0.5°C *(up to* 1.9°C *at 99% confidence), with global mean temperature changes about 1.5 times as large. Thus they claim that the climate sensitivity to $2\times CO_2$ is around* 0.75°C*, outside the Intergovernmental Panel on Climate Change range of 1.5-4.5 (misquoted as* 5.5°C *in SV03) [IPCC, 2001]. Note, however, that their maximum global sensitivity of* 2.9°C *lies well within the accepted range.*

1. To be scientifically precise, we wrote our best estimate for the sensitivity, as well as estimates on the upper limits at different confidence levels, given our data and model. 2.9°C is indeed the upper limit at the 99% confidence level, using the model which gives the highest upper bound (Rothman). If the GEOCARB III model is favored (as Rahmstorf et al. clearly do), then the corresponding upper limit is lower, at about 2.2°C. Although it is not probable that the sensitivity is this large, it cannot be discounted at 1% probability. It is for this reason that we conservatively wrote "These results differ somewhat from the predictions of the general circulation models (GCMs)". Not less and not more, and any other interpretation by Rahmstorf et al. or the media are their own.

2. As a side note, if one abides strictly by the GCM used in the IPCC 2001 TAR report, then their 15 models have sensitivities of 2.0 to 5.1°C. The SAR report had more models and a larger range. So GCM sensitivities do typically span between 1.5 and 5.5°C as we wrote. The range 1.5 to 4.5°C is described in the IPCC as the "widely cited". They repeat this more than a dozen times while actually citing only two references once. On the other hand, they specifically mention the range of their models used only once (and a few times implicitly). This could be the reason why Rahmstorf et al. believe that the IPCC GCM models cover a sensitivity range of 1.5 to 4.5°C. This begs the side question, why do the scenarios in the TAR use GCM models that have, on average, a sensitivity higher by

0.8°C than the "widely cited" range? But this is not our concern, we simply tried to be accurate and conservative in our citation.

—A critique of the CO_2 and temperature reconstructions used in SV03 will be published by Royer et al [in press], who correct the Veizer et al. $\delta^{18}O$ record for the effect of changing pH. This effect has been demonstrated in culture [Spero et al., 1997] and explained theoretically [Zeebe, 1999; Zeebe, 2001]. The result is a corrected climate record that no longer follows the cosmic ray model but correlates well with the GEOCARB III *CO_2 reconstruction. SV03 challenge the credibility of the CO_2 reconstructions by showing two divergent alternatives to the well-known* GEOCARB III *model, by U. (not R.) Berner (not documented in the scientific literature) and by Rothman [2002]. SV03 argue that the disagreement between the reconstructions reveals them to be in need of "validation", but ignore the large literature of paleosol, stomatal, and carbon and boron isotopic data, which support the Geocarb reconstruction [Royer et al, in press].*

While the pH proposition of Royer et al. [2004] is an interesting modification, and likely has some influence on interpretation, it is by far not the panacea it is claimed to be. Our detailed response to the Royer et al. paper will appear in GSA Today, which shows that the effect cannot be as large as claimed, is described at `www.phys.huji.ac.il/~shaviv/ClimateDebate`, and very briefly summarized below.

Which one of the published CO_2 models, if any, will eventually be validated remains to be seen. We are aware of another recently published model (Wallmann, 2004) that is based on similar principles (GEOCARB, pH) as that of Royer et al. [2004]. This model produces a Phanerozoic CO_2 curve that is, yet again, different from all the others. The claim that GEOCARB yields a trend similar to that derived from the proxies may have some credence, but it should be kept in mind that these proxies are spot estimates, all requiring heavy assumptions, and the ranges of these estimates are very broad.

The pH affects the δ^{18}O of marine calcite in an opposite way to that of temperature. Royer et al. (2004) cancel any discrepancy by simply assuming correspondingly lower pH for seawater. Yet, as stated in the written reply, the price that one has to pay is a multitude of special pleadings for cold intervals at high atmospheric CO_2 levels, such as the glacial late Ordovician times at an apparent 5000 ppm CO_2. Note also that, except possibly for a few boron isotope data in the last 100 Ma, no constraints exist that would enable an independent estimate of pH and the above correction is thus entirely ad hoc. More specifically, we have the following:

1. Royer et al. [2004] do not include the ice-volume correction to the δ^{18}O data. This is important because some (about half) of the δ^{18}O variations should be attributed to the waxing and waning of ice-sheets. Namely, they overestimate by about a factor of 2, the temperature variations.

2. Counter to the above claim, the pH corrected δ^{18}O data still correlate with the CRF. If one signal (δ^{18}O) has a large correlation with another (CRF), then even if a correction (pH) is added to the first signal, a large correlation between the modified signal and the second signal still remains. The reason for the pH corrected signal correlating well with the GEOCARB III data is because the pH correction depends predominantly on the CO_2. Irrespective of the CO_2 reconstruction used, once a function of it is added to the δ^{18}O reconstruction, it is obvious that the corrected signal will correlate with the CO_2! In other words, any signal whatsoever used for the CO_2, would give a pH correction to the temperature that correlates with it. This is nothing short of bootstrapping.

3. Taking the above into consideration, (a) one cannot claim that a CO_2 signature was observed. (b) The CRF still has a large correlation with the temperature. This can be seen even by eye in the temperature reconstruction, which includes the exaggerated correction. And (c) once a more realistic pH correction is considered, the best estimate for $\Delta T_{\times 2}$ increases a bit, but the CRF still explains most of the variance (i.e., it is still the main climate driver). In fact, once it is increased a bit, instead of an upper bound, we obtained an estimate for the sensitivity range, which is consistent with the 6 additional sensitivity estimates in Shaviv [2004].

—Irrespective of the data quality, the simple regression method of SV03 is unsuitable to estimate the climate sensitivity to a CO_2 doubling. The main reasons are that (i) other forcing and feedback factors may co-vary in a statistically dependent way with CO_2 and cannot be separated, (ii) the operation of some climate feedbacks depends on the time scale considered, and (iii) the strength of climate feedbacks depends on the mean climate.

Over a decade ago, [Lorius et al., 1990] used the high-quality records of temperature and CO_2 variations from ice cores to derive information on climate sensitivity. These authors had reliable data available and carefully considered the above caveats. Concerning (i), [Lorius et al., 1990] recognized that CO_2 and methane concentrations co-vary, so that only the joint effect of both gases can be derived *by regression. They accounted for the known orbital forcing and also considered other possible feedbacks, such as the aerosol loading of the atmosphere. They further distinguished slow and fast feedbacks (caveat (ii)). The growth and decay of continental ice sheets represents a slow feedback operating over millennia; if one is concerned with the more rapid response of the climate to CO_2, ice sheets have to be accounted for as a major forcing.*

In contrast, SV03 accounted for none of these caveats. Concentrations of other greenhouse gases, which may have co-varied with CO_2 on the multimillion-year time scale, are not known, and neither is the aerosol loading of the atmosphere or the external forcing of the climate changes on this time scale. Likewise, it is not known which physical, geochemical or biological feedbacks may operate, and at what magnitude, on such long time scales.

We are well aware of these complications, but the available publication space did not permit, nor the subject matter require, such a discussion. Furthermore, we fail to see how this musing would change the statement that "it is not clear whether the CO_2 is a driver or is being driven by climate change since the CO_2 appears to lag by centuries behind the temperature changes, potentially acting as an amplifier but not as a driver".

—Lorius et al. [1990] concluded from their analysis that climate sensitivity to a doubling of CO_2 is $3-4°C$, in good agreement with independent estimates based on the physical understanding of CO_2 forcing and relevant feedbacks as coded in models. Note that the primary driver of glacial cycles is the Milankovich orbital forcing while CO_2 acts as an amplifying feedback; this in no way questions the effect of CO_2 on temperature.

It remains to be seen whether orbital forcing is the primary driver, since the evidence for correlations with cosmogenic nuclides is accumulating (Sharma, [2002]; Christl et al., [2003]; Niggemann et al., [2003]).

If one takes the different estimates for the temperature change and the various contributions to the radiative forcing since the last glacial maximum (Hansen et al. [1993], Hoffert and Covey, [1992]), then a sensitivity of 1.4 to 3.2°C is obtained. If one adds the effect of increased CRF (because earth's magnetic field was weaker than today, and the sun less active), the sensitivity becomes 1.1 to 2.6°C [Shaviv 2004]. So the warming since the last glacial maximum cannot be used to prove that the sensitivity obtained from the Phanerozoic data is wrong. In fact, both are consistent with a "black body Earth", having a sensitivity of $\sim 1.2°C$.

—The dependence of climate sensitivity on the mean state (caveat (iii)) cannot be avoided, but it is a more serious problem for the time period considered by SV03 with conditions very different from the modern climate system. Positions of continents shifted, ocean currents took a different course, and estimated CO_2 levels were between twice and ten times of present values during most of this time. Little is known about the feedbacks operating on these time scales and for high CO_2 climates. There are good reasons to assume that important amplifying feedbacks, such as the snow albedo feedback, become much weaker in warmer climates, which would result in an underestimation of climate sensitivity to CO_2 doubling in such a regression.

It is certainly true that when estimating sensitivity on different time scales it is implicitly assumed that the climate system does not behave much differently when, for example, snow is present or absent. It is for this reason that: "As a final qualification, we emphasize that our conclusion about the dominance of the CRF over climate variability is valid only on multimillion year time scales."

Note also that, by accepting the CRF-climate connection, it is possible to estimate the climate sensitivity for additional time scales (Cretaceous, Eocene, Last Glacial Maximum, 20$^{\text{th}}$ century warming, and solar cycles). All are consistent with a sensitivity of $1.15 \pm 0.25°C$, while without the effects of the CRF, the consistency between the sensitivities obtained declines, yet it is still on the lower end of the IPCC range.

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