

Discussion

Response to comment on “Are there connections between Earth’s magnetic field and climate?, *Earth Planet. Sci. Lett.*, 253, 328–339, 2007” by Bard, E., and Delaygue, M., *Earth Planet. Sci. Lett.*, in press, 2007

V. Courtillot*, Y. Gallet, J.L. Le Mouël, F. Fluteau, A. Genevey

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The main points raised by [Bard and Delaygue](#) (hereafter referred to as BD07) essentially address issues originally published in [Le Mouël et al. \(2005\)](#) (BD07’s point 3) and [Gallet et al. \(2005, 2006\)](#) (BD07’s point 4) in the same journal. We are happy that the paper which brings these issues together ([Courtillot et al., 2007](#)) has attracted BD07’s attention. We respond in the order selected by the authors of the comment.

Point 1 is concerned with the relative amplitudes of variations in radiative forcing due respectively to the Sun and CO₂. The 2 Wm⁻² value for CO₂ change is not disputed. The value of 1–2 Wm⁻² we quote for the Sun is disputed by BD07, who quote Crowley’s (2000) value of 0.5 Wm⁻². This point is admittedly unclear in our paper and deserves clarification. Satellite observations of solar irradiance are available since 1976 ([Fröhlich and Lean, 1998](#)). Longer term variations are indeed in the 0.5 Wm⁻² range quoted by BD07. We note however that shorter term fluctuations reach 1 Wm⁻² (in comparison to the incoming 345 Wm⁻²) and that these might be relevant if non linear forcing effects are involved. In any case one can argue that variations in solar irradiance at operative frequencies may not be as negligible compared to CO₂ variations as generally assumed.

Point 2: Fig. 1 of our paper indeed represents the “tuned” version of $\delta^{18}\text{O}$ and $\Delta^{14}\text{C}$ of [Mangini et al. \(2005\)](#). The oxygen isotope values are made to better fit the carbon isotope curve, yielding a highly significant correlation ([Mangini et al., 2005](#)). However, this tuning does not change matters considerably. [Mangini et al. \(2005\)](#) indeed themselves state in the legend of the “non-tuned” curve: “Periods of lighter isotopes coincide with lower $\Delta^{14}\text{C}$ concentration in the atmosphere”. And the Mangini et al curve is tied to 13 Th/U ages. Uncertainties in these ages are a few decades and will affect the untuned curve. Another source of uncertainty (which tends to be minimized because of the large number of age determinations) lies in the presence of gaps in stalagmite recording when temperature falls below 0 °C. Yet, the visual correlation seen on the non-tuned curve which incorporates all these uncertainties is already obvious in [Mangini et al. \(2005\)](#). The match can of course not be perfect because of the uncertainties. If solar variability played only a minor role in the past two millennia, tuning could not improve the correlation. The correlation coefficient is only 0.6, and other forcing factors have to be taken into account.

It is therefore not surprising that the tuned curve should reveal the link between solar activity and $\delta^{18}\text{O}$. It is moreover interesting to note that this correlation, obtained on an Alpine stalagmite, and therefore evidence of the influence of solar variability on climate, is also found in proxies from other regions around the

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* Corresponding author.

E-mail address: courtill@ipgp.jussieu.fr (V. Courtillot).

globe: correlation between times of solar minima and cold episodes in western Europe (Magny, 1993; Holzhauser et al., 2005), modulation of precipitation in the tropics in Northern South America and Yucatan (Haug et al., 2001), in Eastern Africa (Verschuren et al., 2000), and Arabia (Neff et al., 2001); influence on droughts in North America (Yu and Ito, 1999).

Coming back in part to point 1, climate modeling allows one to quantify the influence of several forcing factors over the past millennia. Energy Balance Model (EBM) calculations do suggest a notable role of solar forcing on temperatures. Calibration of EBM sensitivity allows a discussion of changes in climate variability, not climate amplitude: and indeed long term first order climate trends can be correlated with solar variability. For instance, Crowley and Kim (1999) show that 18 to 34% of observed variance is associated with solar and volcanic forcings. Crowley (2000) in the same way links 41 to 64% of pre-anthropogenic temperature variations to these forcings. Transient Global Climate Model (GCM) simulations go the same way in stressing the role of solar variability on global climate (e.g. Cubasch et al., 1997; Rind et al., 1999; see for instance Bradley et al., (2003) for a detailed summary). Changes in pCO₂ are on the order of 10 ppm over decades and provide only a modest contribution in pre-anthropogenic GHG release times.

Point 3: We do propose new proxies of magnetic activity which are introduced and extensively discussed in Le Mouél et al., (2005). We show that using a single component from any observatory (a quick and light operation) is for our purpose as efficient as indeed using the aa index which requires far lengthier calculations, a point that may have been missed by BD07. The solar irradiance daily time series we used is that from the SOLAR2000 research grade model upgraded to v1.23A (file Five_cycle_v1_23a.txt dated 23 April 2003) which covers the time period from 14 February 1947 to 31 May 2002 (Tobiska, 2001; note that in Le Mouél et al., (2005), this data set was erroneously attributed to Solanki, 2002, although resulting changes are negligible). Because of the 11-year filtering, five years are lost as an end-effect and the series starts in 1952, as noted by BD07. No fiddling with the data or intended false impression. And in fact this is of no consequence for the issue we are pursuing (moreover, in Le Mouél et al., (2005) the purpose of the paper was essentially magnetic and not climate). Indeed, as is readily seen, applying a vertical translation and a scale reduction (i.e. the operation applied to all data curves in order to take into account the very different units of the data and normalize them) to the truncated record makes it coincide essentially with the “full record” shown in BD07’s Fig. 1 over the joint time interval. The comment makes it clear

that the meaning of the term “trend” over this century long period needs to be specified. We believe that BD07 imply actually a “linear trend” over the interval, whereas we emphasize what in Le Mouél et al., (2005) we call the “overall magnetic trend” or OMT, rising from 1910 to 1950, decreasing from 1950 to 1970, rising from 1970 to 1990 and decreasing since. This OMT is the same for the Eskdalemuir and Sitka magnetic data (and for that matter for any component from any observatory), and for solar irradiance (“full” or “truncated” records likewise): this is our main observation, and the point we stress. We are therefore principally looking at shapes and phases (or if one prefers the sequence of changes in the sign of the derivative), and the matches are not an “artefact” giving a “false impression” but an interesting observation which we reiterate. Our next observation is to note that when plotted in the same way, the global temperature curve (whichever version is used, and there are several which are not exactly identical) displays a rather similar trend (given uncertainties; see Brohan et al., 2006) until about 1990, when the curves depart, with global temperature continuing to increase and solar and magnetic indices decreasing. The temperature series we actually used is obtained from Briffa et al. (2001) – specifically, column 7 of ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/reconstructions/n_hem_temp/briffa2001jgr3.txt, that is, years 1871 to 1997 – which is, originally, from Jones et al., (1999) as quoted. All we did was to average it over an 11 yr sliding window. Interestingly, this essential conclusion of our paper is also clearly seen in BD07’s Figure 1, who therefore support it. All they say is that Solanki (2002) already made that point, which we duly acknowledge in our paper (e.g. page 329), and that “a correlation had already been proposed” by several authors, namely Lockwood and co-authors, and Bard and co-authors.

The final statement of BD07 regarding the fact that aa correlates “much better” than magnetic data with solar irradiance and global temperature being wrong, the “clues” the authors discuss are actually irrelevant. The “long term trend” is the OMT and not a linear trend as recalled above. We should recall that the ESK and SIT magnetic series and the aa series do not measure exactly the same magnetic phenomena and are not expected to be related in a linear way. But the point of Le Mouél et al., (2005) was to show with very simple indices that the entire ionospheric and magnetospheric current systems, despite their differences, pulsate on the long term at the rhythm of the Sun, as exemplified by the OMT. BD07’s next interrogation regarding open versus total magnetic flux is not founded. For example, it has long been known that a simple terrestrial proxy such as the amplitude of the quiet day solar variation of declination provides a wonderful

representation of the eleven year cycle of sunspots (i.e. the Wolf number).

Point 4: we first reiterate the fact that the “claims” made in our paper regarding correlations between cooling periods and archeomagnetic jerks were actually put forward by Gallet et al. (2005, 2006). We do note that the causal relationship between cosmic ray flux and cloud cover suggested by Marsh and Svensmark (2000) would result in a correlation opposite to the one we find if the field geometry were axial and dipolar and this is precisely why we propose a mechanism of dipole tilt or non dipole geometry to interpret our observations. Gallet et al. (2005) write: “Another hypothesis is to assume that the incoming charged particles are deflected towards the poles, where the overall low humidity level due to cold temperatures limits cloud formation. If archeomagnetic jerks indeed correspond to periods of strongly inclined dipole, then the charged particles would interact with more humid air from lower latitude environments, leading to significantly larger cloud production and cooling.” And if this happens, there is no need to “overcome the more direct effect”, as (mis)understood by BD07 (who seem to understand that a growing axial dipole is superimposed on a tilted dipole, which is not the case).

BD07 cite work by Moros et al. (2006) which complements the work of Bond et al. (2001) which we had cited, and in their view (their Fig. 2) lead one to reject the correlation with archeomagnetic jerks proposed by Gallet and co-authors. Moros et al. (2006) and Andrews et al. (2006) discuss the existence of two climatic zones in the North Atlantic ocean, both influenced by the East Greenland current (EGC) and the North Atlantic drift (NAD). Moros et al. (2006) find differences between sediments influenced by both currents (proxies leading to conflicting climate trends in the past 4 kyr), leading them to somewhat ambiguous conclusions. Yet, the multi-decadal and centennial events noted by Moros et al. (2006), i.e. a cooling period at ~ 2.7 ka, the Roman and Medieval Warm Periods and the Little Ice Age, are the same as recognized by Bond et al. (2001) in NAD areas. Furthermore, cold periods observed in the North Atlantic are clearly correlated to Swiss glacier advances (see e.g. synthesis in Bradley et al., 2003, pages 126–130). So we do not agree with BD07 that recent studies refute the interpretation of Bond et al. (2001).

Also, we wish to recall that evidence of a correlation between archeomagnetic jerks and cooling events (in a region extending from the eastern North Atlantic to the Middle East) now covers a period of 5 millenia and involves 10 events (see f.i. Figure 1 of Gallet and Genevey, 2007). The climatic record uses a combination of results from Bond et al. (2001), history of Swiss

glaciers (Holzhauser et al., 2005) and historical accounts reviewed by Le Roy Ladurie (2004). Recent high-resolution paleomagnetic records (e.g. Snowball and Sandgren, 2004; St-Onge et al., 2003) and global geomagnetic field modeling (Korte and Constable, 2006) support the idea that part of the centennial-scale fluctuations in ^{14}C production may have been influenced by previously unmodeled rapid dipole field variations. In any case, the relationship between climate, the Sun and the geomagnetic field could be more complex than previously imagined. And the previous points allow the possibility for some connection between the geomagnetic field and climate over these time scales.

Finally, BD07 note that the lower atmosphere warmed by $0.8\text{ }^\circ\text{C}$ during the 20th century, that this is the time when greenhouse gases of anthropogenic origin rose and yet that the exponential shapes of their growth suggests that they were not the main cause for warming from 1920 to 1940 and temperature dip and plateau observed between 1940 and 1970. And that “the observed acceleration of temperature rise since ~ 30 years probably exceeds the natural variability” (we note that BD07’s Figure 1 extends to the year 2000, whereas our Figure 3 coming from Le Mouél et al. (2005) stopped in 1992 for T and 1996 and 1998 for magnetic and irradiance data). For a forceful statement that “all the trends in the Sun that could have had an influence on the Earth’s climate have been in the opposite direction to what is required to explain the observed rise in global mean temperatures (...) over the past 20 years” see Lockwood and Fröhlich (2007). These are precisely some of the points we make in concluding our paper and this is what led us to enter the dangerous realm of global warming: temperature rises fast in the early part of the 20th century, when CO_2 rises only slowly, dips when CO_2 accelerates, and rises again fast afterwards when CO_2 rises fastest. Our OMT curve shows the same trend until the mid-1980s and could qualitatively support a solar origin for the pre-1980 fluctuations, particularly the 1920–1940 rise and following dip, contrary to what has often been said in recent years (a more detailed analysis of regional temperature curves is under review — Le Mouél et al., 2007. So we fail to see the point of a comment which basically reiterates our conclusions.

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