Response to comment: "Statistical issues about solar-climate relations by P. Yiou and 7 co-authors, Clim. Past Discuss., 6, 461-487, 2010".

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A long comment (Yiou et al, 2010) has recently been published regarding three papers by the present co-authors (Le Mouël et al, 2008, 2009; Courtillot et al, 2010), the latest two having been published in the *Journal of Atmospheric and Solar Terrestrial Physics*. We find it unusual that the comments were submitted to a different journal: the comments and our response would normally belong where the original papers were published. Our response to these comments follows the order used by Yiou et al (2010). Their comments belong to one of three categories: either immaterial, in that they do not apply to or alter our earlier conclusions, irrelevant, or in some cases wrong. They likely reflect profound differences in approaches to long time series of observatory data and the way they can be handled, and on the potential physical significance of the results.

In their introduction, Yiou et al (2010) list a number of general comments, advice and criticism on possible misuse of results of statistical analyses and the dangers of “data snooping” that apparently do not address our papers explicitly and therefore do not call for a response.

1. The next section of Yiou et al (2010) is on “Data”. Yiou et al (2010) use only daily mean temperature data from the same ECA&D database we use (we also used GHCND and found similar results); we used not only mean but also minimum and maximum daily temperatures. Yiou et al select stations “yielding less than 10% of missing or doubtful data”. We used only the best quality data available in the data base. Yiou et al (2010) note the existence of homogeneity problems and point out that “more than 94% of stations are flagged as “*doubtful*” or “*suspect*””. “Data quality” and “suspect stations” call for some definition: the ECA&D database has three quality control checks for data values: “*Flag=0*” or “*valid*”, “*Flag=1*” or “*suspect*” and “*Flag=9*” or “*missing*”. Stations are put in three classes according to four homogeneity tests: “*Class 1*” or “*useful*” when no more than 1 test rejects the null hypothesis at the 1% level, “*Class 2*” or “*doubtful*” when 2 tests reject the null hypothesis at the 1% level and “*Class 3*” or “*suspect*” when 3 or 4 of the four tests reject the null hypothesis at the 1% level. Note that due to the definition used by the database editors, the best data in their database are designated as “*suspect*”! We used only valid data and series with no gap larger than a year. Moreover, using the most complete data sets from the Netherlands and Switzerland, we checked that the presence of these (limited) gaps made no difference to our conclusions.

Although we have stated clearly in our papers our views and past experience in using long time series of data from observatories, we repeat here that we strongly disagree and warn against blind, automated correction and homogeneization of these data: it is highly unlikely that one can do better (except of course for easily detected very large errors) than the original observers, particularly for old data. Our past experience from magnetic observatories over century long time scales is that proper homogeneization of data would imply extended stays in the original observatories. We also strongly object to the removal of a mean seasonal cycle from the data to construct “temperature anomalies”. This process has (too) long been used, for instance in magnetic observatories: the mean seasonal curve was used as a reference to determine “anomalies” and was called by some the “iron curve”. The dangers of using these iron curves have been underlined, for instance by Mayaud (1965). The differences between observations and iron curve still contain systematic signals which are not random noise. Moreover, the techniques we used in our papers make data pre-processing such as homogeneization useless.

In any case, Figure 1 of Yiou et al yields, for the single observatory of de Bilt and for the mean of European stations, curves which are essentially identical to and confirm those of, for instance, Le Mouël et al (2008, Figure 6; 2009, Figure 2): there is no significant trend (either warming or cooling) prior to 1986 and a significant jump by about 1°C occurs near 1986. Yiou et al (2010) "*randomly focused*" on the Paris-Montsouris data, which display a positive (warming) trend, with an acceleration after 1987: this has been discussed previously in our group for a series spanning from 1783 to 2000 (Perrier et al, 2005). The trend is likely affected by an urban heat island effect. The most important thing is to note that this trend is not seen in de Bilt, or more importantly in the European mean.

Yiou et al (2010) note that there is no significant correlation between the mean daily temperature and geomagnetic time series, which supports the need for other diagnostics if one wants to investigate potential relationships between the two variables, and therefore supports our own approach. We note that Yiou et al (p. 465) erroneously indicate that "the intensity of the geomagnetic field is measured in two directions" when of course the full 3D vector is measured; they point out that "the fast variability of those components are very similar" but forget to provide a reference, when this was the central topic of the study by Le Mouël et al (2005).

2. The next section of Yiou et al (2010) is on "Method". It focuses on AR(1) processes for which it reproduces the three quantities we defined in our papers, i.e. the mean interannual squared variation Q, the mean squared daily variation ζ and their ratio, the lifetime L. We use Q, ζ and L as empiric indicators of the long-term behavior of the shorter-term variability of the signals we analyze (which can be temperature, pressure, wind direction, sunspot numbers, geomagnetic components in a given observatory,…), in the expectation (which we check a posteriori) that they retain significant aspects of the long-term history of these signals.

Yiou et al (2010) seem to think that lifetime estimates cannot be applied to a process with memory other than AR(1): this is a false assumption. Natural processes are generally too complicated to be successfully modeled by a simple AR(1) process. Yiou et al (2010) think that the lifetime as we define it "is not connected with the lifetime notion in statistical survival theory". However, estimating process memory using the concept of lifetime is possible and does capture real physical properties. Such is the case for instance for sunspots: one component of the process is the appearance of sunspots as random events and another the persistence of long-lived sunspot groups, and this is well described by an autoregressive process. This has been shown by Blanter et al. (2006) and subsequently verified by an independent study (Henwood et al, 2010). The same observations may in some cases also apply to weather events and patterns.

Our approach involves a study of correlations between the L and Q transforms of temperature (pressure, wind direction, etc) and solar activity, represented for instance by the ζ transform of geomagnetic components. Yiou et al. restrict their comment to the significance of the correlation between temperature and geomagnetic activity and forget the fact that they share to some extent a well-known natural and common physical forcing factor: the Sun and its variable activity.

We note in all cases that the amplitude of the variations is an important factor in assessing the physical significance of a correlation. Whereas total solar irradiance varies by only about 1 to 3.10^{-3} over the time scales of interest (annual to secular), the variations we evidence amount to tens of percent (e.g. Le Mouël et al, 2008, Figures 4, 7; Le Mouël et al, 2009, Figures 4,5,7,9,10; Courtillot et al, 2010, Figure 4). Moreover, we do not seek in all cases a quantitative evaluation of correlations but use them as qualitative tools to guide the investigations of potential forcing factors of climate (or magnetic) variability. At the end of this section, Yiou et al remind one that "significant correlation between two time series" implies "no proof (or even a suggestion) of causality". Our commentators may have failed to notice that in none of our papers have we implied a direct causal link between magnetic and temperature variability, in the sense that one would be the direct cause of the other (see last paragraph of this response). But we have indeed noted that when one observes a correlation, it is better to analyze it rather than ignore it: in the case of (daily) magnetic and (inter-annual) temperature variability, it is physically reasonable to think that solar variability could be a common cause and hence could impose its signature on temperature variability (through photons, cosmic ray deflection or cloud formation) on one hand, and on magnetic variability (through the solar wind and charged particles interacting with the Earth's magnetic field in the magnetosphere and ionosphere) on the other.

We also recall, as we have noted in our papers, that we are not seeking or expecting perfect correlations. Figure 3 of Le Mouël et al (2009) illustrates this: we compare the long-term variability of climate variables with four direct or indirect "proxies" of solar activity: the 11-yr running means of the sunspot number, the aa-index, the squared daily variation of the horizontal (H) and vertical (Z) component of the geomagnetic field in the Eskdalemuir observatory (Scotland; we could have used almost any component from any geomagnetic observatory as shown in Le Mouël et al, 2005). If the averaging is sufficient and if there exists a common forcing factor acting in a similar way on a number of processes, all indicators will indeed behave in the same way and reflect the variations of the forcing factor (here solar activity expressed in various ways: photons of various wavelengths, charged particles, solar magnetic field acting directly on cosmic rays,…). What is emphasized by Figure 3 of Le Mouël et al (2009) is that these four solar indicators display quite similar large amplitude changes (with a characteristic signature in the form of an upper case M). Direct calculation of the usual correlation coefficient between these four curves may well yield rather low values; yet a common signature is clear, as shown in our papers. Another case provides a good example: the analysis of temperature series from Oregon and Washington meteorological stations over the past half century (Courtillot et al, 2010, e.g. Fig 4).

Data treatment and quality of the database are of course important, but only to a limited extent: heterogeneous and noisy data may also reveal an existing signal if this signal is strong enough. For example, the solar signature is obvious in the daily disturbances (squared daily variation) of the H and Z geomagnetic components at Eskdalemuir observatory from 1915 to 2005 (Figure 1b, where they are averaged over a 11-yr centered sliding window). But, due to noise, the common part of the signal does not yield a perfect correlation between the components, even when they are from the same station, and the correlation coefficient reaches a value of “only” 0.84. In addition, Figure 1a displays the evolution of the daily absolute variation between Z and H: the correlation coefficient is 0.87. The absolute daily variation and the squared daily variation, which are two rather distinct operators, display essentially the same long-term signature, whose robustness is further confirmed.

3. The fourth section of Yiou et al (2010) is on "Results", starting with bias in the Q and L transforms. There is a major lack of understanding of our work in the comments at the end of their section 4.1 (also in section 2). We emphasize that we **do not** restrict the use of the Q and L transforms to AR(1) processes, or at any point assume that the time series we investigate are to be considered as AR(1) processes, contrary to what Yiou et al (2010) imply. We only illustrate the effects of these operators on AR(1) processes. The statement that the sum of AR(1) processes is not an AR(1) process is true, but irrelevant to our study of European mean temperatures. Moreover, nobody claims that the temperature of a single station (such as de Bilt or Paris) may be successfully represented by an AR(1) process. The local temperature is strongly influenced by air currents (e.g. zonal winds) which depend on season and therefore displays significant variability of the lifetime, also depending on season (e.g. the lifetime is greater in winter than in summertime).

Yiou et al. then "point out that the average of independent AR(1) processes is almost never an AR(1) process". As an example that may be easily checked, one can take the sum of two AR(1) processes with different parameters $a1<a2$ and amplitudes $A1$ and $A2$: the lifetime estimate a, which is such that $a1<a<a2$, depends on the respective amplitudes $A1$ and $A2$. When $A1>>A2$, a tends to $a1$; when $A2>>A1$, a tends to $a2$. For more on lifetimes, readers are referred to Blanter et al (2005, 2006) in addition to the appendix in Le Mouël et al (2009). Notwithstanding the fact that we do not imply or need that the time series under analysis be an AR(1) process, to which Yiou et al restrict their analysis, the temperature series we use over Europe are of course not independent. On the contrary, we expect them to demonstrate some common behavior (which serves to define a consistent climatic zone). Choosing too large a zone would blur the common component we are seeking. Not only the regional mean curves, but also the individual departures of station curves from the regional mean contain some common signature (which varies somewhat in amplitude and shape) and are therefore not an independent random noise from station to station. The departure from the overall average is itself an organized physical signal. We therefore do not consider temperature fluctuations from different stations as small or independent. The final sentence of the paragraph "the interpretation of λ - the estimator of the mathematical expectation of L - of the European mean temperature in term of process memory is a priori not possible" is therefore wrong. We expect that second order moments contain information about the common solar signal that can be extracted through the evolution of the lifetime. We may cite here the recent analysis by Lockwood et al. (2010) who "*show that cold winter excursions from the hemispheric trend occur more commonly in the UK during low solar activity, consistent with the solar influence on the occurrence of persistent blocking events in the eastern Atlantic* ». Yet, they « *stress that this is a regional and seasonal effect relating to European winters and not a global effect* ». Spatial and seasonal heterogeneity of the correlation between temperature lifetime and daily geomagnetic disturbances representing the solar signal provide important information concerning climate dynamics in the European region and should be considered not as a limitation but as a source of new knowledge, as we have discussed in several papers (Le Mouël est al, 2008; Courtillot et al, 2010). In these papers, we have seen that some conclusions could be extended for instance to the US: the Sun may not influence only the UK or Europe…

The next subsection of the comment is on variability of Q and L for temperature. Yiou et al calculate and illustrate (their Figure 4) these for de Bilt and Paris, and mean daily European temperatures. In the case of de Bilt, they find that the large variations of Q are meaningful and that the overall shapes remain the same when the averaging window is varied from 7 to 22 years, within an overall amplitude factor. Figure 4 of Yiou et al provides an illustration of some of our observations (note however that Yiou et al have exchanged Figure 4e - which is a L not a Q - and Figure 4f). The de Bilt and Paris L and Q curves are quite well correlated, as can be checked simply by visual inspection. But, in addition to being inadvertently inverted, the L and Q curves for the mean European temperatures do not correlate well at all, when we find quite good correlation: indeed, Le Mouël et al (2009) show in their Figure 5c that Q and L correlate very well from 1920 to 1990, not only for mean temperatures but also for minimum and maximum temperatures and for pressure. We cannot understand this and believe an additional error must have slipped in Figure 4 of Yiou et al. It is unfortunate that these commentators have not performed the same analyses (with averaging window varied from 7 to 22 years) for the mean European curves as well. In any case, the special role of the 11-year window in analyzing a potential solar effect should again be emphasized.

We were of course aware of the necessity to check the stability of our results and to ensure that they were not artefacts: several kinds of security tests were performed as a routine procedure before publication. In addition to the figures included in our papers, we present here a comparison of temperature lifetimes with the absolute daily variation of the geomagnetic H component (Figure 2): this shows almost the same similarity and correlation with temperature lifetime as did the squared daily variation of the Z-component. We see that the problems linked to the geomagnetic Z-component reported by Yiou et al. do not affect the result: replacing Z by H and the squared daily variation by the absolute daily variation does not change the results, a strong check of the robustness of our conclusions.

Yiou et al note that “the *L* transform for raw temperature (i.e. without removing the seasonal cycle) has the same general behavior and order of magnitude as for the temperature anomalies”, thus vindicating our approach and remark (above) that removing the seasonal cycle and using the so-called “temperature anomalies” is at best useless for our purpose.

The next subsection is on “Significance of correlations”. Contrary to what Yiou et al write, it is not difficult to justify why it is interesting to study the correlation between the ζ variations of one variable (say a magnetic component) with the *L* variations of another (say a temperature). Indeed, Yiou et al (2010) to some extent confuse properties of the second order moment of a process with properties of the first order moment and of the process itself. Their table 1 illustrates this confusion. The correlation between temperature lifetime and squared daily differences of the geomagnetic field Z component is natural and expected, because both are influenced by solar forcing. Increased correlation and significance for the mean European temperature curve further supports the lifetime approach as a way to underline the solar signature. The commentators might usefully take a next step in their tests and use wintertime seasonal lifetimes (see Le Mouël et al, 2009; Figure 7c) in order to obtain “significant p-values by the usual standards”.

Although we prefer to show full pictures rather than single correlation coefficients, all these correlation coefficients may be calculated and their significance may be estimated under the hypothesis of an AR(1) model. Correlation coefficients between temperature lifetimes and mean squared daily variation or mean absolute daily variation of the geomagnetic H and Z components, from 1915 and 1940 to the present, averaged over an 11-yr sliding window are presented in Tables 1 (for annual lifetime series) and 2 (for winter lifetime series). The temperature series are for Europe or the Netherlands only, and include the minimum, mean and maximum temperatures. Correlations are always found to be positive, and almost always larger and more significant after 1940. In the case of wintertime temperature lifetimes in Europe, all 12 correlation coefficients are larger than 0.76 and all are significant at the 99% level ($1-p<0.01$). This fully vindicates the conclusions made in our former papers.

Note here, in reference to Yiou et al’s comment (page 472), that it is useless to remove the slow internal secular variation from the geomagnetic signals to identify variability, as they do, which Yiou et al acknowledge a few lines later. Moreover, removing it using an (arbitrary) spline function with 20 degrees of freedom may generate spurious effects in the timescale range we are interested in (~2.100/20 = 10 years). Also the sentence “the correlation between *L* transforms does not allow for an inference of a mutual relation between the original time series, unless a specific model of covariation is provided” is wrong. When a stable and reliable correlation between two series can be evidenced, it implies some relationship in the parameters governing their evolution or a statistical dependence of the two series. The opposite case of a lack of such correlation does not imply that the time series are independent, since this may be the result of high stochastic noise in one or both series. The wide use of data filtering is grounded on this common knowledge.

The methods used by our commentators are valid for normal (Gaussian) random variables, which the lifetimes we use are clearly not. Again, in order to establish a link between two observed variables, one needs not only to evaluate their correlation but also the amplitude of oscillations as already recalled above. When amplitudes are large enough (which is the case of the series analyzed in our three papers), the link can exist even if the correlation coefficient, calculated following the standard formula, is not very high. Such is the case for instance between the *L* and *Q* transforms of the de Bilt or Paris temperatures, of the solar indices illustrated in Figure 3 of Le Mouël et al (2009), of the *L* and *Q* transforms of mean, maximum and minimum temperatures and pressures in Europe (Figure 5 of the same paper), but also of the amplitude of the 6-month spectral line in length of day and sunspot number (Le Mouël et al, 2010), or the mean period of the Madden-Julian oscillation and solar proxies (work in progress). All these potentially important links are established using jointly information on correlation of these time series and amplitudes of their oscillations.

We fail to understand the points the commentators attempt to make in their subsection 4.4 and in Figures 5 and 6. It is for instance written that "Le Mouël et al. (2009) … computed the correlation between $\zeta_{\Theta}^{(Z)}(t)$ and $L_{\Theta}^{(Paris)}(t)$" which we actually did not compute anywhere. The reasons why the Paris data are noisy and should be avoided are discussed elsewhere (see e.g. Perrier et al, 2005); we avoid calculating transforms for single stations and mostly use averages of stations over a country (such as the Netherlands) or better a whole region such as Europe, which improves the regularity of the signatures we are seeking. Next, the authors discuss the correlation between $F^X(t)$ and $L_\Theta^Y(t)$ (top of page 475), when apparently F^X controls linearly the memory parameter a^Y whereas lifetime $L_\Theta^Y(t)$ is non-linearly related to a^Y as $1/(1- a^Y)$: it is therefore not surprising, if we understand this rather obscure comment correctly, that Yiou et al find small correlation coefficient values (see Blanter et al, 2005, 2006 for more on some properties of lifetimes and their estimates). Finally, the authors repeat their worries about insignificant correlations. They apparently fail to be impressed by (or to have noticed?) the fact that the long-term trends of squared interannual variations or lifetimes of independent temperature series from various regions in the US (Le Mouël et al, 2008, Figure 4), from the USA, much of Europe and some stations in Australia (Le Mouël et al, 2008, Figure 7; 2009, Figure 4,5,7,9,10), 24 stations in Oregon and 29 in Washington state (Courtillot et al, 2010, Figure 4), display similar behavior over the 20th century, a behavior that parallels that of most solar activity indicators (Le Mouël et al, 2009, Figure 3 and Figures quoted above).

The last sentence of the paper is either puzzling or revealing: ”The increase of temperature (or temperature anomalies) after 1940 is still unexplained by the variations of the geomagnetic field anomalies”. It should be clear that nowhere in the papers referred to by the commentators do we propose such an explanation, which the sentence might make readers erroneously believe. What we did in another paper (Le Mouël et al, 2005) was to point out that the long term (secular) trend of higher frequency variability of the (external) geomagnetic field correlates well with solar activity changes at those longer periods (not a major surprise) and also with global temperature (never implying that the magnetic field could cause the temperature variations but that both could share a common original forcing factor linked to the Sun). This observation stands.

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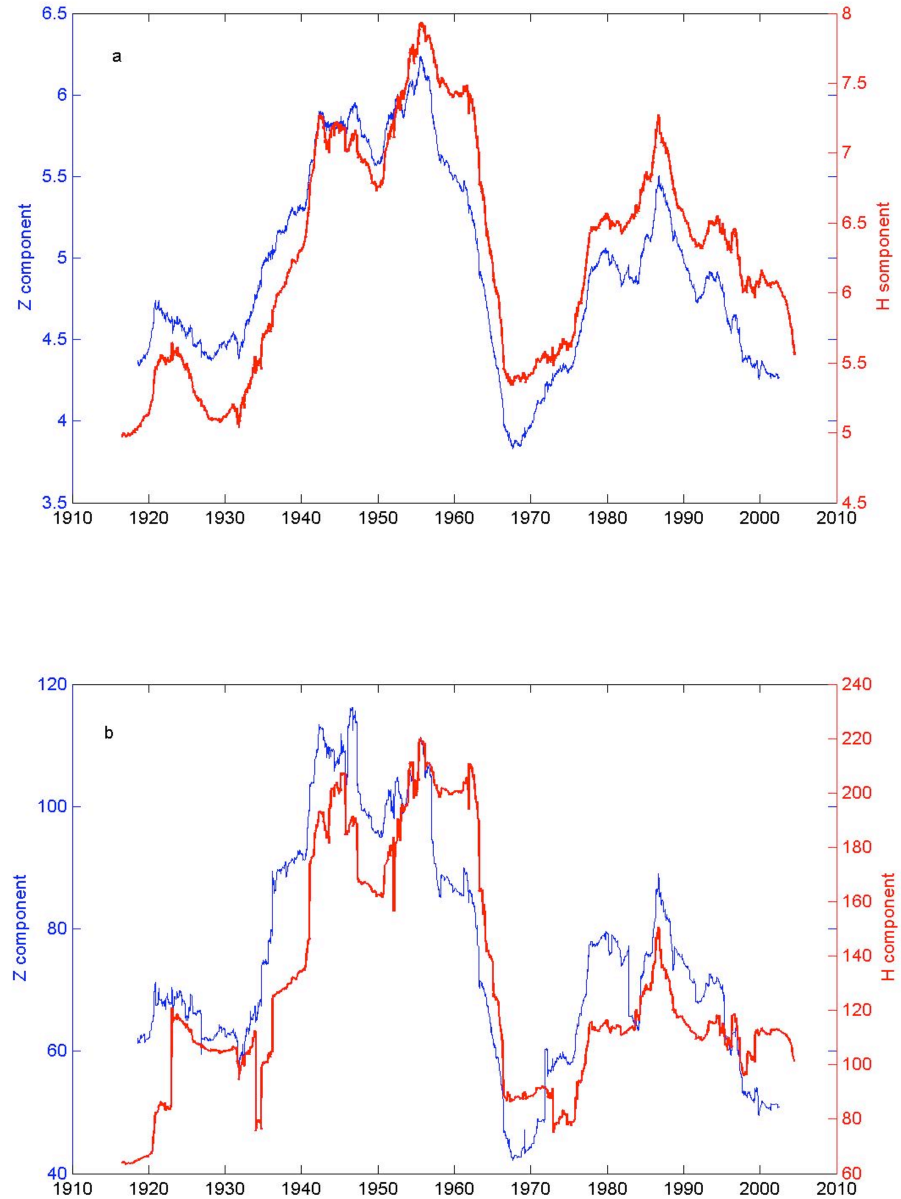

Figure 1. Evolution of the absolute daily variation (a) and squared daily variation (b) of the Z (blue) and H (red) geomagnetic components at Eskdalemuir observatory averaged over an 11-yr centered sliding window. The correlation coefficients estimated over the common interval of the records from 1915 to 2005 is equal to 0.87 (a) and 0.84 (b) respectively.

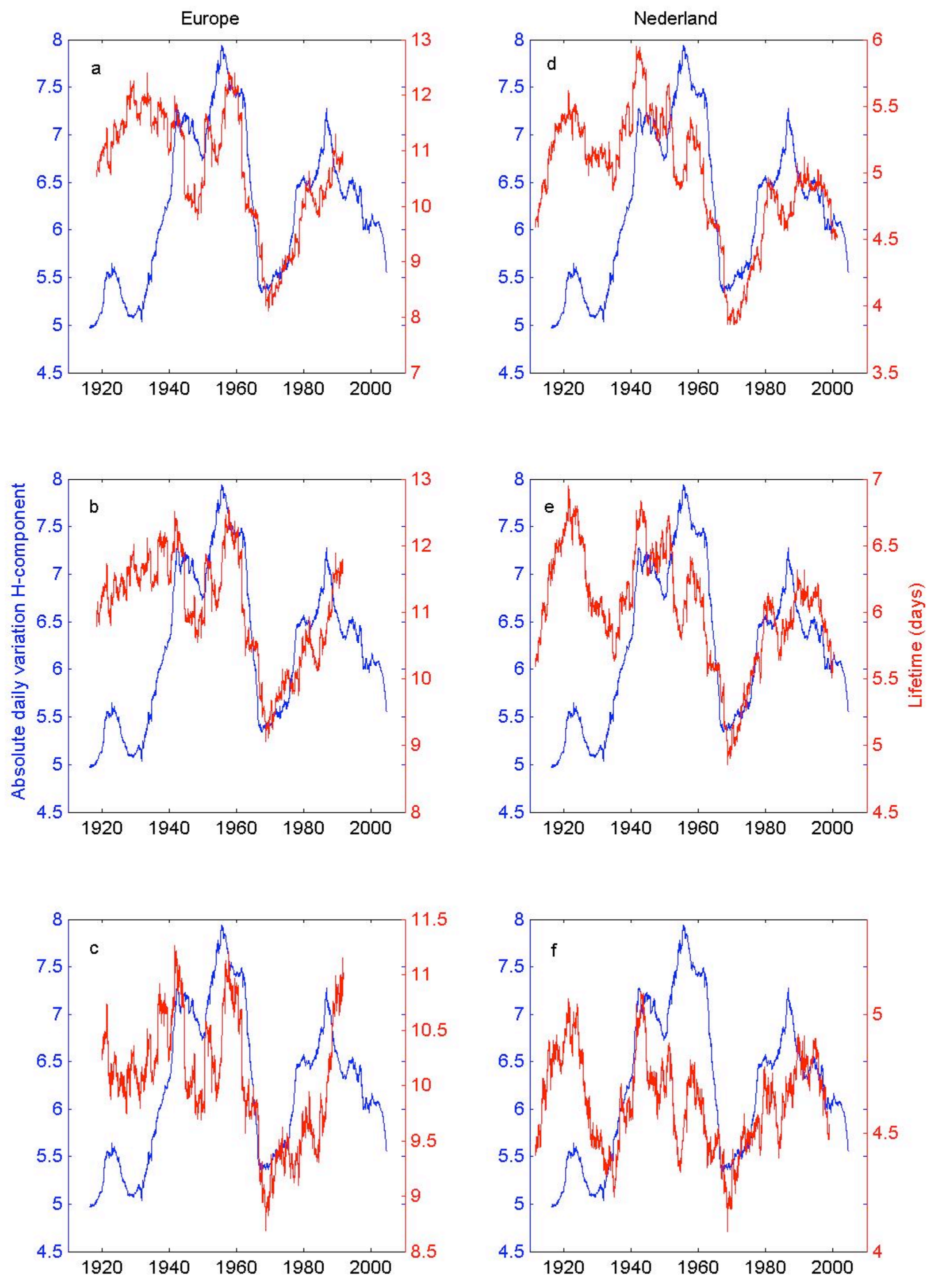

Figure 2. Evolution of the absolute daily variation of the geomagnetic H-component at Eskdalemuir (blue) and of the lifetime of minimal (a, d), mean (b, e) and maximal (c, f) temperatures in Europe (a–c) and the Netherlands (d–e) averaged over an 11-yr centered sliding window.

Table 1. Correlation coefficients between annual temperature lifetime (Tmin, Tmean, Tmax) and geomagnetic series (H and Z) averaged over an 11-yr sliding window. η denotes the mean absolute daily variation; ζ the mean squared daily variation of geomagnetic components.

Lifetime series	From 1915				From 1940			
	H-component		Z-component		H-component		Z-component	
	η	ζ	η	ζ	η	ζ	η	Z
Tmin, Europe	0.2	0.40	0.46	0.45	**0.88**	**0.81**	**0.81**	***0.73***
Tmean, Europe	0.3	*0.50*	*0.57*	*0.58*	**0.85**	**0.80**	**0.80**	**0.74**
Tmax, Europe	0.42	*0.53*	*0.6*	*0.61*	**0.75**	***0.69***	***0.69***	*0.63*
Tmin, NL	0.31	*0.50*	***0.62***	***0.67***	**0.76**	***0.71***	**0.80**	**0.77**
Tmean, NL	0.31	0.43	*0.58*	***0.62***	***0.72***	*0.61*	**0.79**	**0.76**
Tmax, NL	0.31	0.27	0.41	0.45	0.44	0.24	0.48	0.47

Table 2. Same as Table 1 for wintertime temperature lifetime.

Lifetime series	From 1915				From 1940			
	H-component		Z-component		H-component		Z-component	
	η	ζ	η	ζ	η	ζ	η	ζ
Tmin, Europe	0.4	*0.61*	***0.66***	***0.66***	**0.87**	**0.86**	**0.86**	**0.81**
Tmean, Europe	0.47	***0.64***	**0.7**	**0.70**	**0.85**	**0.82**	**0.84**	**0.78**
Tmax, Europe	*0.51*	***0.66***	**0.7**	**0.71**	**0.80**	**0.78**	**0.80**	**0.74**
Tmin, NL	**0.71**	**0.75**	**0.87**	**0.86**	**0.83**	**0.76**	**0.87**	**0.84**
Tmean, NL	**0.68**	**0.68**	**0.82**	**0.82**	***0.74***	*0.64*	**0.80**	**0.79**
Tmax, NL	***0.64***	***0.65***	**0.76**	**0.79**	*0.63*	0.56	***0.72***	***0.73***

Bold numbers <0.01; ***bold italic*** <0.02; *italic* <0.05