Response to the Comments by Kossobokov, Le Mouël and Courtillot

(corrected and updated version; corrected sentences are underlined; updated sentences are underlined and in blue)

1/ Issues on the correlation with solar forcing

In our paper, we criticized the oversimplified use by Le Mouël et al. (2010) and Kossobokov et al. (2010a), hereafter LMKC and KLMCa, of the sunspot number (SSN) as a proxy for solar forcing. In response, Kossobokov et al. (2010b), hereafter, KLMCb state that *"the international sunspot numbers are the only and therefore, the best available proxy of solar activity in the last 250 years"*. This statement is wrong because other solar proxies cover this time period such as records of aurorae (e.g. Silverman 1992), cosmogenic isotopes (e.g. Delaygue & Bard 2010) and the aa geomagnetic index (over 240 yr, e.g. Lockwood & Stamper 1999).

In addition, it has been shown by many authors that past observations on sunspots can be used to derive better solar forcing records than the raw SSN used by LMKC & KLMCa. For example, there are alternative solar irradiance curves based on a variety of properties of sunspots including the envelope of the sunspot number cycle, the length of the cycle, the structure and decay rate of individual sunspots, the average sunspot number and/or the group sunspot number the solar rotation and diameter, sunspot group areas, Greenwich sunspot maps and p-mode amplitudes estimated from sunspot numbers (see reviews by Bard et al. 2000 and Gray et al. 2010 and references therein).

Lean et al. (1995) proposed that the solar forcing record could be divided into two superimposed components: an 11-year cycle based on the parameterization of sunspot darkening and facular brightening and a slowly-varying background. Solanki and Fligge (1998) reconstructed a solar irradiance record back to 1874 AD by using different relation active regions (quadratic calibration between the SSN and the spacecraft TSI record over a decade) and to the long-term component (linear calibrations between brightness and chromospheric emission or length of the activity cycle). LMKC & KLMCa could also refer to Solanki (2002) who published solar forcing curves over the past 150 years.

Consequently, in clear contrast with the statement by KLMCb, there is indeed a full literature of works describing solar forcing curves beyond the satellite era of the past 30 years. This is the reason why Judith Lean (2010) introduced her recent review paper on the subject with this general statement: *"With newly available modeled reconstructions of historical solar irradiance (albeit with large uncertainties), terrestrial studies are no longer relegated to using geophysically meaningless sunspot numbers as a proxy for solar irradiance, and direct comparisons of solar and other climate forcings are possible."*. Lean's remark directly applies to the papers by LMKC and KLMCa.

KLMCb cite Le Mouël et al. (2009) for the evidence of a correspondence between long-term SSN changes, the aa-index and some geomagnetic proxies (such as the geomagnetic components at the Eskdalemuir ESK observatory also used by Le Mouël et al. 2005 and Courtillot et al. 2007).

The fact that the long-term sunspot number and geomagnetic activity are tightly correlated is indeed known since decades (e.g. Bartels 1932), in particular with the aa geomagnetic index (Mayaud 1972, Cliver et al. 1998a,b). However, it was shown by Bard & Delaygue (2008) that the correlation with the single station local record of ESK (or SIT) is much poorer, because the geomagnetic activity levels reached in the 1920-30s is similar with those of the 1960-70s, in strong contrast with the large systematic differences observed for the two periods in the SSN and aa index records. This poor correlation is obvious when these time series are compared over the full 20th century (see Fig. 1 in Bard & Delaygue 2008 who corrected an incomplete plot published by Le Mouël et al. 2005 and Courtillot et al. 2007).

Unlike their previous papers, Le Mouël et al. (2009) splitted the comparison in two panels: Fig. 3a includes the smoothed SSN and aa index and Fig. 3b the ESK H and Z components. Had they tried to plot these curves as they published them in 2005 and 2007, a strong discrepancy would have appeared for the 1960-70s.

In their attempt to respond to our criticism that they forgot to take into account multiple forcings, KLMCb state that the effects of a volcanic eruption last only a few years. Indeed, a major explosive event occurring in the tropics, such as the 1991 Pinatubo eruption, generates sulfate aerosols directly in the stratosphere, rapidly leading to significant impacts on the global climate (Robock, 2000, 2003, Shindell et al. 2004; N.B. the Icelandic eruption of the Laki, cited by KLMCb, was mostly confined to the troposphere and only had regional impacts in Europe and North America, Thodarson & Self 2003). However, the recurrence of large stratospheric eruptions can lead to cooling over decades (e.g. Gao et al. 2008, Cole-Dai et al. 2009), which could indeed be confused with other forcings such as the solar variability (11-yr and longer) and internal oscillations such as the ENSO variability.

Numerous climate model simulations clearly suggest that solar and volcanic forcings were collectively responsible for some extended cold periods of the past few centuries (e.g. Wagner & Zorita 2005, Ammann et al. 2007 , Gao et al. 2008). Wagner & Zorita have shown that climate variations during the Dalton Minimum, 1790-1830, cannot be understood without taking into account the volcanic forcing. Ammann et al. specifically refer to *« cumulative volcanic cooling »* … *« simulated in the [mid-13th], mid-15th, 17th, the early 19th, and (to a lesser degree) the late 19th to early 20th century »*. These authors further state that *« volcanically active periods occur contemporaneous to solar induced cooling (e.g., Maunder Minimum) and thus make a clear separation of climate response at the low frequencies difficult. »*. This statement fully applies to the early 19th century with the superposition of the Dalton solar minimum and some of the largest volcanic eruptions, notably the cataclysmic Tambora eruption in Indonesia (Cole-Dai et al. 2009). By using a suite of ocean-atmosphere GCM, Glecker et al. (2006a,b) further demonstrated the persistence over decades of the ocean heat content perturbation by large volcanic eruptions such as that of the Krakatoa in 1883.

We are happy that the importance of the volcanic forcing is now acknowledged by KLMCb in their response when they say that *« three major eruptions occurred in the second half of the 20th century which LMBY note is a period of High (multi-decadal) solar activity »*. However, KLMCb still fail to grasp the full complexity of this second half of the 20th century which is characterized by a third significant forcing linked to greenhouse gases of anthropogenic origin, which cannot be forgotten in the attribution analysis.

While discussing about the solar irradiance record based on satellite data and the difficulty in detecting a baseline evolution over the last 30 years of precise data, KLMCb refer to *“ongoing controversies, such as that between Scafetta & Willson (2009) and Krivova et al. (2009)”*. This statement is misleading because there is no “ongoing” debate between these two particular studies: Scafetta & Willson (2009) used the model developed by Solanki et al. (2005) and Krivova et al. (2007). These authors (Krivova et al. 2009) simply discovered that Scafetta & Willson had made fatal mistakes in using Krivova et al.’s model, but that its correct use leads to a stable irradiance baseline. Hence, the study by Krivova et al. (2009) constitutes a clear-cut and definitive refutation of the previous claim by Scafetta & Willson (2009).

2/ Issues on the use of multi-century long time series

Obviously, the authors of LKMC and KLMCb are specialists of geomagnetism data, and have no idea of what is homogenization (referred in the comment as "blind correction"), and why homogenization is necessary prior analysis of long climate data series, as it is recommended in WMO publications. We kindly suggest to the authors of this comment to have an eye at the bibliography provided in our article. "Blindly" using raw data leads to false results. We wonder what conclusions can be drawn about climate evolution or climate interactions when working on data exhibiting artificial changes of 1°C or more. The fact that some teams have used inhomogeneous data to produce results does not mean this is a good practice.

Homogenization of early data is problematic, and in many cases dealing with this implies to replicate older measurements with old instruments, a time (and money) consuming task. Please refer to Brunet et al. (2006) and Bransdma & Van der Meulen (2008): you shall see that this is not an "automatic" "blind" correction.

Concerning the Bologna series, the artifact is so large (the authors of LMKC themselves had quoted it) and the fact that there was no attempt to understand its causes is so obvious that we will not even argue. We note that it corresponds fortuitously to the maximum of an intense solar cycle and makes a spurious contribution to the solar shift.

KLMCb are confounding quality control (QC) and homogeneity checks. Individual flags just refer to testing whether each daily data is present or missing, and may be considered as an outlier or not, as individual spurious data can occasionally occur in the records. This is only a preliminary step. Once QC is performed, homogeneity of the series is then tested. It turns out that most of series are not homogeneous on longer period, which is not surprising for climate data scientists. The simple fact that we have to explain this reveals that the authors of KLMCb do not know the nature of the data they are using.

3/ Issues on the application of statistical tests:

KLMCb pretend to have found an error in our reasoning that would invalidate our criticism of LMKC and KLMCa. This error would be to have split the average into an average of averages and to have missed in this way the true number of degrees of freedoms in the system, resulting into an overestimate of the standard error by a factor $21^{1/2}$.

This argument is only displaying that the authors of KLMCb still persist in misunderstanding the basis of mathematical statistics. Let us explain why.

There is nothing wrong in using an average of averages and it is indeed beneficial to proceed in this way in the present case. It is perfectly legitimate to use our formula (3), which can be found in any standard text book, including Press et al. (1992) miss-quoted by KLMCb, to estimate the variance of the solar shift. The only assumption is that daily 21-day averages for a given day of the year are uncorrelated over successive years. If daily temperature fluctuations were themselves uncorrelated, the variance of 21-day averages would just be the variance of daily fluctuations (up to bias) divided per 21 and in this case the standard error shown by LMKC and KLMCa would be recovered. We have checked this result by performing a random redistribution of temperatures for each day of the year among the ensemble H and L that preserves the solar shift but generates a totally uncorrelated dataset (see p. 781). This important point that invalidates their reasoning has been totally missed by KLMCb.

Hence, our estimate of the confidence interval does not neglect by construction a factor $21^{1/2}$ because it is able to get the standard error of LMKC and KLMCa when an uncorrelated series is employed. This factor is obviously hidden in the variance of 21-day filtered data as explained above. The crucial advantage of our approach is that it is still valid when the data are correlated and when the ratio of the variance of 21-day averages to the variance of daily fluctuations is larger than 21 , which is indeed the case for daily temperatures. Notice also that the validity of formula (3) has been checked by random permutation tests (see p. 780). All our results fit perfectly together and lead to conclude that those of LMKC are void of any significance.

It can be added that this conclusion does not depend on the 21-day filtering. The solar shift has been calculated without filtering, or with 11-day or 41-day filtering (see the electronic supplement) and the conclusion is always that this quantity does not display any significance once the period where solar forcing overlaps anthropogenic forcing is removed.

KLMCb also discuss a secondary point which is based on a curve shown in the electronic supplement (p.20 of the PDF version). This curve shows the autocorrelation function for daily fluctuations and provides a rough estimate of 9 days for the correlation time. This time is only used on p.780 to illustrate our reasoning and to introduce the rigorous demonstration which follows on p.780 and 781 (also discussed above) without involving this time at all. This time is anyway interesting. It is not influenced by seasonal effect as pretended by KLMCb for the reason that it is based on a data series from which the mean seasonal cycle has been removed. There is no usage of any 90- or 150-day interval in the calculation of the autocorrelation as can be checked from the supplementary material. We consider our calculation as perfectly valid even if this value, 9 days, does not pretend to be more than a rough estimate to decide how many degrees of freedom are in the temperature series. This rough estimate is well confirmed by the statistical analysis which shows a year-average underestimate of 2.7 in the standard error by LMKC and KLMCa.

KLMCb show in their figure 1 three curves from which they conclude that the correlation time is about 3 days and not 9 days. We note in passing that 3 days would still mean a factor 1.7 in the standard error with respect to LMKC and 2.8 to get the proper 90% confidence interval, but KLMCb fail to reach this conclusion. It is impossible to figure out what is really shown in this figure since the only indication provided by KLMCb is that it shows « autocorrelations of the daily temperatures in 21-day intervals ». We may infer that some sort of high-pass filtering at 21-day period has been applied and this obviously leads to an artificial reduction of the autocorrelation. We will not comment further on this figure until KLMCb provide a full description that allows to reproduce their curves. We have ourselves provided an electronic supplement with all the data and methods needed to reproduce and check our results. This is very easily performed by simply playing a documented Mathematica notebook. We challenge our critics to be fair and to do the same. We do not intend, in any case, to enter a discussion over undocumented curves.

Our conclusion is that there is nothing in the criticism of KLMCb that can challenge our refutation of LMKC and KLMCa but enough to show that their description of our work as irrelevant or erroneous is mere gesticulation.

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