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Climate of the Past Discussions

Interactive comment on "On misleading solar-climate relationship" *by* B. Legras et al.

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legras@lmd.ens.fr Interactive comment Legras et al.

Answer to referee #4

We thank referee #4 for his comments and suggestions. Comments by the referee are highlighted and followed by our answers.

Homogenization need: it is not necessarily clear why spurious homogeneity errors would project on solar forcing and lead to erroneous conclusions however, I agree it can occur in work such as the one discussed here because of the rel-

atively few instances attributed to high vs low solar forcing (see figure 2, which coincides with hi solar as discussed). However, another argument against the finding of a single-station solar correlation is that since solar forcing affects the energy balance globally, averaged stations, and large-scale aggregated datasets should show the cleanest and clearest signal and are best suited to estimate its effect.

Homogeneity errors do not need to project always on solar forcing but we have a nice example with the spurious bump in the Bologna TX series between 1865 and 1880. This period coincides with an isolated high solar cycle according to LMKC an contributes significantly to the solar shift as shown in our supplementary file. An other example is the jump near 1950 in the Maastricht temperature series. Such jump correlates with both the the initiation of anthropogenic forcing and a sequence of active solar cycles.

Although solar influence is global, the observed signature of solar variations has a regional pattern (Gray et al., 2010, see) at the ground and in altitude. The mechanisms that convey solar influence through stratospheric ozone imply such regional character and other more speculative mechanisms related to cosmic rays as well. This does not necessarily mean that the signature of solar influence would be radically different in Belgium and in the Czech Republic as implied by LMKC. We have added a calculation of the solar-shift for the northern hemispheric mean of the HadCRUT3 dataset Brohan et al. (2006) in the Supplementary Material (). We find a positive significant signal of the order of 0.1 K during summer which is consistent with previous results and the comments of Referee #4. However, this result is only indicative since the HadCrut3 dataset begins only in 1850 and after removal of the second half of 20th century, only three cycles remain in the H ensemble and all other forcings are neglected.

There is an important string of literature not related to that is very relevant as well attribution work uses estimates of internal climate variability (including ENSO indirectly) and climate response to multiple forcings simultaneously to attribute

observed changes to causes. This literature seems entirely disregarded in the paper, although it is arguably the most rigorous work that attributes changes to external drivers and hence distinguishes between individual drivers and climate variability. An assessment of evidence for solar forcing to observed changes can be found in Hegerl et al., 2007 (IPCC chapter 9) which includes many relevant citations, eg papers by Stott, North and Hegerl. All of these point out that only if all important forcings are considered, conclusions on causes of change are reliable. In addition, the IPCC chapter also explains why it is more difficult to detect changes in response to large-scale forcings on small scales, therefore it would be extremely surprising to detect solar forcing on individual stations if its not detectable from global surface temperature.

Of course we do not disregard the literature on attribution to multiple forcing and we quoted some contributions including the pioneering work of Judith Lean and collaborators that we consider as highly relevant even if it based on empirical modelling. The references quoted by Referee #4 have been included in the revised version.

Introduction 1rst para: North and Stevens, 1998 as well as Stott et al., 2003 also very relevant

The references to North and Stevens (1998) and Stott (2003) have been added to the revised manuscript.

p. 769: do the authors mean White 2000 is an example of erroneous correlation?

We mean "as shown by White 2000". The sentence has been corrected.

p. 771 l1-5: Volcanic forcing is also important as it can spuriously correlate.

We fully agree and this is discussed on p.770 l.27 and p.771, l.10-13.

l8: attribution of the observed warming trend to external forcing is not done in the papers cited, but in attribution papers (Stott et al., 2007; Huntingford et al., citations see Hegerl et al., 2007 IPCC chapter9).

We have modified the sentence to quote the references provided by Referee #4 (Stott et al., 2006; Huntingford et al., 2006; Hegerl et al., 2007)

L 23: other papers have argued that the solar forcing spatial response pattern is quite similar to that to greenhouse gases, but either are of course distinct to volcanism and ENSO.

We do not see to which paper Referee #4 is referring. The regional character of the response to solar variations is described in Gray et al. (2010)

p. 772, l10-17: Again, the role of external forcings in the little ice age is also discussed in IPCC AR4 chapter 9, quite extensively, which should be cross referenced along with the papers cited in section 9.3 related to the topic.

The reference has been added.

Following pages: the critique of the method of LMKC seems reasonable, and it appears that the authors managed to reproduce their results and point out problems and errors, most severely, use of a 66% confidence interval and neglecting daily autocorrelation, both of which are very severe problems.

This severe problem invalidates the results and the conclusions of LMKC.

p. 778 eqn 3: This is a bit terse can an extra step be added?

See the answer to Referee #1.

p. 783 l 12 and thus it is not meaningful: not clear what is meant here it is no longer significant after removing the solar cycle coinciding with the inhomogeneity in TX?

We mean exactly that, but then the H ensemble contain only 3 cycles between 1834 and 1856 and between 1946 and 1954, and is likely to be sensitive to other inhomogeneities or other forcing during this short range. Hence we do not attach a lot of weight to this observation. The sentence has been modified.

p. 785, attribution discussion: This is referring to empirical and modelling studies, but not using detection and attribution studies, which at times detect solar forcing effects, but usually small, and often find insignificant effects of solar forcing on large-scale temperatures.

We have added references to attribution studies but we do not wish to open a discussion on that matter.

Same page, paragraph around line 20: If the solar forcing were enhanced via cosmic rays, then the empirical studies cited, or the attribution studies (neither of which assumes a fixed amplitude of the solar response) would have noticed that. Which is another argument against the idea, and supports the deduction against enhanced solar cycle.

We agree but again we do not wish to extend the discussion too far in that direction.

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