

Interactive comment on “On misleading solar-climate relationship” by B. Legras et al.

Anonymous Referee #4

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This publication points out reasons that can lead to misestimation of the role of solar forcing in individual station records. The paper appears sound and useful for a wider audience, and although it starts out as a criticism of a published paper, it is interesting beyond that. Therefore I recommend acceptance pending some revisions

Major comments:

- 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 relatively 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
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should show the cleanest and clearest signal and are best suited to estimate its effect.

- 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.

minor comments:

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

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

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

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 chapter).

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.

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.

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

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

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?

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.

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.

Interactive comment on Clim. Past Discuss., 6, 767, 2010.