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***Interactive comment on* “The complex dynamics of the seasonal component of Earth’s surface temperature” by A. Vecchio et al.**

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First of we thank the Referee for his/her very interesting comments very useful to clarify some aspect of the paper. The changes made on the manuscript are underlined in bold in the new version of the paper.

In the following, our detailed response.

1) In the title the authors refer to "Earth’s surface temperature", however in the paper they study the US record. I would suggest to change the title to better agree with the contest of the paper. That is, they should refer to "USA’s surface temperature"

Response: we agree with the the Referee suggestion about the title. We change it as: "The complex dynamics of the seasonal component of USA’s surface temperature"

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2) A possible climatic effect of the 18.6 luni-solar nodal cycle has been noted and studied by several other authors. The present paper should reference at least some of those studies. For example: McKinnell, S. M., and W. R. Crawford (2007), The 18.6-year lunar nodal cycle and surface temperature variability in the northeast Pacific, *J. Geophys. Res.*, 112, C02002, doi:10.1029/2006JC003671. Rob Wilson, Greg Wiles, Rosanne D'Arrigo, Chris Zweck, Cycles and shifts: 1,300 years of multi-decadal temperature variability in the Gulf of Alaska, *Clim Dyn* 2006 DOI 10.1007/s00382-006-0194-9. The argument advanced in those papers is that the 18.6 luni-solar nodal cycle induces climate change by means of tidal forces more than by means of a variation of the insolation due to the nutation of the Earth. I suggest the authors to discuss this issue in their comment. That is, is it possible by means of their analysis to determine whether the effect the authors find is due to insolation or to tidal forces driving ocean oscillations?

Response: we thank the Referee for suggesting us missing references. Since they are very important and pertinent they have been added to the bibliography together with other four papers:

+ Currie, R. G.: Evidence for 18.6-year lunar nodal drought in Western North America during the past millennium, *Journal of Geophysical Research (Oceans)*, 89, 1295–1308, doi:10.1029/JD089iD01p01295, 1984. + Cook, E. R., Meko, D. M., and Stockton, C. W.: A New Assessment of Possible Solar and Lunar Forcing of the Bidecadal Drought Rhythm in the Western United States., *Journal of Climate*, 10, 1343–1356, doi:10.1175/1520-0442(1997)010, 1997. + Yasuda, I., Osafune, S., and Tatebe, H.: Possible explanation linking 18.6-year period nodal tidal cycle with bi-decadal variations of ocean and climate in the North Pacific, *Geophys. Res. Lett.*, 33, 8606, doi:10.1029/2005GL025237, 2006. + Royer, T. C.: High-latitude oceanic variability associated with the 18.6-year nodal tide, *Journal of Geophysical Research*, 98, 4639–4644, doi:10.1029/92JC02750, 1993.

Moreover we add in the Conclusions a brief discussion about the 18.6 periodicity, as

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found in other climatic records, and interpreted by means of lunar tidal forcing. Our simple model, based on the variation of the insolation due to the Earth's nutation, represents just a simple example to explain the observed behavior of the USA's temperature records.

We have to remark that, previous papers, analyzing different climatic records, do not provide a physical proof that insolation and/or lunar forcing is operating to fix the 18.6 periodicity.

The Referee asked if our results show any indication that the found effect would be generated by insolation or lunar tides. This is a very interesting comment and it should deserve to be deeply analyzed. However it needs some extra work that would take much time. Anyway we perform a rapid test by plotting the number of anomalies, detected for each station, over the USA geographical map (fig 1 of this letter). The map has been built by computing, for each point corresponding to a station, the Voronoi polygon. This represents the region closer to that point than to any other point. In our opinion, if the anomalies were due to the effect of tidal forces driving ocean oscillations, they would be more visible near the sea. Actually the situation is not clear (see fig. 1). In fact an increase of the number of anomalies can be observed in south-est regions (Florida and Gulf of Mexico) but it can not be generalized to all areas near the sea . We plan a deep investigation of this interesting problem.

3) I would suggest the authors to add a spectral analysis of the data depicted in Figure 4. In fact, a simple visual analysis of the data may be misleading. For example, apparently the period from 1904 to 1984 appears to be covered by exactly four cycles that would imply a 20 year cycle.

Response: according to the Referee suggestion we add the Fourier periodograms, reported in the new figure (fig 5.), for both black and red curves of fig 4. Moreover, in the new version of the paper, we report a table in which the values of the periods, obtained from both A and B methods, have been indicated. The periods have been

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calculated through a sinusoidal fit over the red and black curve of figure 4 and by identifying the dominant peak in the Fourier power spectrum.

sin fit: 18.8 ± 0.4 (method A); 18.7 ± 0.2 (method B) Fourier: 20 ± 5 (method A); 18.5 ± 3.5 (method B)

The uncertainties have been calculated from the fitting procedure and from the Fourier period resolution at the peaks.

The Fourier spectra, reveal other peaks at low energy with respect the dominant one, corresponding to the following periods: $P1=13.9 \pm 0.6$ yr; $P2=10.1 \pm 0.8$ yr; $P3=8.5 \pm 2.1$ yr The periods $P1, P2, P3$ have correspondence in previous works. In particular, $P1$ is consistent with the ~ 15 yr periodicity in coastal surface air temperature in the Gulf of Alaska (Wilson R. et al. Clim Dyn 28:425–440, 2007) attributed to large-scale coherent Pacific climate variability. $P2$ can be related to the ~ 11 yr periodicity in ice core sequences (Royer T. C. J Geophys Res, VOL. 98, NO. C3, PAGES 4639-4644, 1993) attributed to solar cycle effects. $P3$ might be attributed to changing tidal current speeds due to interannual variability of the lunar orbit, in particular to the period of rotation of the lunar perigee around the Earth of 8.85 yr (McKinnell S.M., J Geophys Res VOL. 112, C02002, 2007). It must be remarked that a periodicity of about 7.8 yr has been also found in drought data (Cook E. R. et al., J Clim 10:1343–1356, 1997).

In the new version of the paper we add a more detailed explanation about the period determinations and the caption of figure 4 has been rearranged. Moreover a brief discussion about the physical meaning of $P1, P2, P3$, as reported in previous works, has been added.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/10/C7776/2010/acpd-10-C7776-2010-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 15537, 2010.

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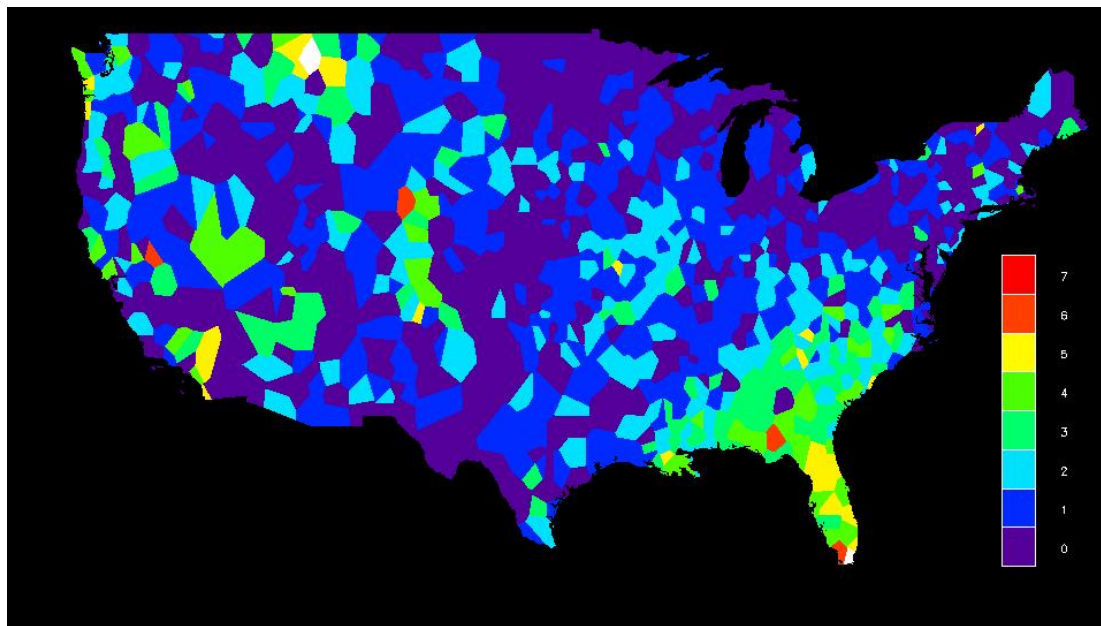


Fig. 1.

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