

Interactive comment on “27-day variation in cloud amount and relationship to the solar cycle” by Y. Takahashi et al.

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Dear Dr. Deneke,

First of all, authors deeply appreciate your careful reviewing and constructive suggestions.

1. In the introduction, the paper refers to a paper of Svensmark(1998), which reports that cloud cover is modulated by cosmic rays. In a subsequent paper by Marsh and Svensmark (2000), the proposed effect is attributed to low-level clouds. For balance, the authors should probably also refer to papers contesting their conclusions, e.g. Damon and Laut (2004).

Answer: We will quote Marsh and Svensmark (2000) in the revision. Also the existence
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of skeptical standpoint toward solar effect, such as Damon and Laut (2004), will be mentioned there.

2. The paper states that OLR is a proxy of cloud amount, a characterisation which is somewhat imprecise. The OLR is mainly modulated by high clouds (in particular convective clouds, as well as upper tropospheric humidity, see e.g. Schmetz et al., 1990), and is not very sensitive to low clouds. The findings of this paper would thus indicate a response of high-level clouds to solar variability, in contrast to Marsh and Svensmark (2000) who suggest a link with low-level cloud amount.

Answer: Although, generally speaking, it might be true that the OLR is more sensitive to high clouds, at this moment we cannot exclude the possibility of contribution by low-level cloud modulations. But we will mention your point in our manuscript that the present results can not be exactly the same phenomena reported by Marsh and Svensmark (2000).

3. The paper does not consider alternative mechanisms to the proposed link with sun rotation. In particular, the review of the MJO of Madden and Julian(1994) already reports on a 26-day period in the MJO for 1980-1985 (see their fig.4), and stresses the "broadband nature" of the oscillation. They also report on studies which attribute the change in the period of the oscillation to warm water and El Nino. Can the authors rule out this hypothesis? If not, they should mention these alternatives. Could they use other datasets used to study the MJO in the review article to extend the period of their investigation, e.g. the pressure at Truk island?

Answer: We consider that, at this moment, nobody can rule out various kinds of reasonable hypothesis concerning sun-earth climate connection, including Madden and Julian(1994). Therefore, we will follow your suggestion to introduce their results and interpretation in discussion section of our revised manuscript. On the other hand, our intension is to emphasize the clear relationship between the peak around 27-day and 11-year solar activity for about 30 years. (We conformed this relationship is still valid

for 2006-2008.)

4. The authors only study the Fourier power spectrum (thus the amplitude of the Fourier transform). Maxima at the same frequency are not a sufficient condition for a causal link. If the suggested link is real, phase coherence between the 27 day oscillations in F10.7 flux and OLR seems also a necessary condition. Hence, the relation of the phases (i.e. the lag) of the two oscillations should be studied. Is it constant over time? The phase lag between the oscillations could also provide an important hint towards the underlying physical mechanism, and should be reported. Also, as recommended in the first comment, confidence intervals for the power spectrum would be beneficial. A much more detailed spectral analysis of the MJO is given i.e. in Whitcher et al. 2000 (even if it uses the wavelet transform instead of the Fourier transform), and might serve as a good example.

Answer: We had examined the phase relationship between F10.7 and OLR. As written in line 8-19 on page 15332, the results show that the unstable relationship between them though the one-year averaged periods are almost the same, suggesting some kind of synchronization to 27-day by weak but persistent outer forcing by solar activity. Also we may need to consider some complicated processes from solar input to the behavior of OLR. For example, "if" the GCR modulates the global electric circuit by changing ionization rate of the atmosphere, which may change the lifetime of the cloud or growing rate of the thunderclouds in maritime continent, the OLR in WPWP could change with large and unstable phase displacement from original solar input. Therefore, we believe that the next step to examine our hypothesis is to investigate the phase relationship between OLR and other meteorological parameters in/near WPWP. We will refer Whitcher et al. 2000 according to your suggestion in the revision.

5. a short description of the datasets used and the accuracies and limitations seems appropriate. It should be stressed that the OLR is taken from narrowband radiances and polar-orbiting satellites (as can be learnt from the reference). Can the authors rule out that the reported 27 day period is an aliasing effect due to orbital sampling of

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polar-orbiting satellites (see paper by Tremberth, 2002)?

Answer: We will add some more information on OLR dataset according to your suggestion in discussion section. As you mentioned, we should pay attention to the problem of aliasing effect by orbital sampling. However, if the 27-day period is an aliasing of shorter period variations, we need to discuss the 11-year solar activity dependence of this shorter periodicity, which may sound more unrealistic.

6. the acronym MJO is used without definition in the abstract

Answer: We will put the full spelling.

Thank you so much again for your careful review.

Yukihiro Takahashi

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