Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-138-RC1, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "How can we understand the solar cycle signal on the Earth's surface?" *by* Kunihiko Kodera et al.

Anonymous Referee #1

Received and published: 5 April 2016

Summary

The authors perform a variety of methods to understand the atmospheric response and surface response to solar changes. They mix observations, reanalysis and model experiments. The study culminates in the modelling assessment, where induced momentum (analogous to solar forcing) is directed into experiments to assess the surface response.

The manuscript is well written, and allows the reader plenty of spin up time in on solar influence on climate. The methods seem sound, although more discussion over different methods would help guide follow on research from this study.

My main criticism is that the paper has a lot of repeated material already in the literature. In fact up to figure 8 there is nothing substantially new. I think the most exciting

parts of the paper certainly come from the model experiments, which culminate in figures 10 and 11. If I were the authors, I would have expanded this section more (at the expense of the early sections), or simply submitted it is a shorter letter based article. Nevertheless, as the authors have submitted the paper as it is, it may be OK to publish, although I believe it needs to be made far more clear that Figures 1-8 are mainly reproducing previous work. Therefore the manuscript needs to be better referenced, and clear in the discussion of the paper structure.

General Comments

1. Make the paper clearer as to what is new and what is not. 2. There are inherent issues with some of the analysis that the authors use. Namely that solar signals can interact in a nonlinear way with the atmosphere and even more so the surface. We know some of the responses are non-linear, and therefore multiple linear regression may not be the most appropriate tool. My feeling is the MLR is probably OK for some assessment of the surface, but the issues with it should certainly be addressed in a standalone paragraph. For instance, machine learning methods (Blume et al, 2012) which are naturally non-linear, optimal detection (Stott et al, 2003; Mitchell, 2015) which gets around some of the non-linearity by using model predicted responses as the regressors (they also do not assume noise free regressors, another issue with the standard MLR), and final non-linear attribution, (Kuchar et al, 2015). The latter study does some comparison with the MLR technique as well, although does not directly address your analysis. 3. From the title I thought it would be a rather different paper. I do not think you have answered the question 'how can we understand. . .', I think you have simply performed an analysis of the surface response. I would therefore change the title.

Minor comments

P1L27: This sentence does not fit so well. In longer term studies (of centuries) solar influence on climate has been known about for a while. Do you mean just short term?

P2L11: 'global mean temperature' to you mean 'surface temperature'?

Section 2.2: Here I would address my General Point 2.

P4L14: A number of studies use additional regressors. Maybe a point or two on why these are OK.

P5L4-5: Maybe cite some papers that look at solar influence on climate using the Tiao method.

P5L27-30: It is not clear to me exactly where you refer to here. Is it literature, or is it panels a and b? If the latter, I still do not see all the features that are mentioned.

P6L5 2-3 years should probably be 2-4 years. In the literature it is often written in both ways, but I think if you look at the figures in the relevant literature, the signals at 2 years are as large as those at 4 years (with the signal max at 3 years).

P6L11-22: Is there perhaps cross correlation in the regressors between solar and volcanic? For instance the response seems anticorrelated say at 45N which is a max in solar, and a min in volcanoes.

P6L32: A forcing of the vortex nearly always leads to a response in the NAM, so why is it remarkable?

P8L15-16: The temperature response seems very large over Eurasia. Is this real? I find it hard to believe that the temperature response is over 2K. I think this needs to be investigated and discussed more.

P10L15-22: Is there a QBO in the model? How does the momentum forcing interact with the QBO. Surely at some points they will not be consistent with the H-T relationship?

P11L8-19: The authors are very sure about the casual links here. I think they need to be more speculative about the comments, or back it up with modelling evidence from their model.

P12L13-19: So are the authors suggesting they do not believe the Haigh mechanism?

I think it is still important, but the paragraph does not read that way. I would also cite Simpson et al, 2009.

P13L13-16: Hood et al only show a subset of models, and not even all the coupled chemistry models from CMIP-5. Are there better (or additional) references that could expand on this point?

Figures

Figure 1: I would make a and b more comparable. Use the same contour intervals and only plot of the oceans. Also use the same latitude ranges.

Figure 4: There is a lot of detail in panel c, and it can't really be seen. Can you enlarge it to the size of the other panels.

References

Blume, Christian, Katja Matthes, and Illia Horenko. "Supervised learning approaches to classify sudden stratospheric warming events." Journal of the Atmospheric Sciences 69.6 (2012): 1824-1840.

Kuchar, Ales, et al. "The 11-year solar cycle in current reanalyses: a (non) linear attribution study of the middle atmosphere." Atmospheric Chemistry and Physics 15.12 (2015): 6879-6895.

Mitchell, D. M. "Attributing the forced components of observed stratospheric temperature variability to external drivers." Quarterly Journal of the Royal Meteorological Society (2015).

Simpson, Isla R., Michael Blackburn, and Joanna D. Haigh. "The role of eddies in driving the tropospheric response to stratospheric heating perturbations." Journal of the Atmospheric Sciences 66.5 (2009): 1347-1365

Stott, Peter A., Gareth S. Jones, and John FB Mitchell. "Do models underestimate the solar contribution to recent climate change?." Journal of Climate 16.24 (2003): 4079-

4093.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-138, 2016.