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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Smithsonian solar constant data revisited: no evidence for cosmic-ray induced aerosol formation in terrestrial insolation data" *by* G. Feulner

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First I would like to thank the referee for his report which contains helpful comments, although I do not necessarily agree with all of the referee's remarks.

> *This paper is effectively a technical comment on the paper by Weber in Ann Phys (Berlin). For a long time scientific practice has generally been to publish comments in the same journal as the original paper, to allow comments and corrections (and the courtesy of a reply from the author) to be associated with the first piece of work. Nowadays web linking improves this so readers of a paper cannot be unaware of comments and corrections when*

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the original paper is consulted. By submitting this related contribution to a different journal no link will be made from the original journal. It therefore becomes possible that a reader of the original paper could still be unaware of the additional points made relevant to the paper he or she is reading.

While it is true that many journals now directly link from an original paper to a technical comment in their online versions, this is by no means the case for all journals. In fact, from what I could see Ann. Phys. (Berlin) does not provide a direct link of that kind, except via the convenient “Cited by” link where a reply in a different journal would also be visible. Furthermore, I would argue that it is common scientific practice for readers of an article not just to check for technical comments, but for any subsequent discussion of an article’s results in the scientific literature, a procedure which is greatly helped by today’s internet databases. Thus I do not share the reviewer’s concern that readers of the original article might be unaware of my critique.

So why has this extended technical comment been presented as a new paper for a different journal? One reason might be that its author wishes to engage a different audience. Another argument may be that the needs of climate science are such that an immediate high profile response is considered essential. But surely the importance of climate studies is such that the established conventions in science need, more than ever, to be continued rather than being put aside? Of course another convention, by introducing a new paper such as this rather than a targetted comment, is the requirement that new and original material and analysis should be presented.

Since the discussion of my reasons to submit to a different journal is rather elaborate, this point appears to require some explanation: First, in redoing the complete analysis (including extensive Monte-Carlo and bootstrapping simulations) my paper goes

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beyond the typical technical comment. Secondly, I indeed wanted to publish my paper in a dedicated atmospheric-science (and open access) journal rather than a small general-physics journal like Ann. Phys. (Berlin) to ensure visibility within the community and an unbiased and high-quality peer-review process. No "needs of climate science" were involved except for the fundamental need of any scientific discipline to carefully test (and, if necessary, correct) previously published results.

> *It is an important technical consideration to remove seasonality and biases from the data, but perhaps an opportunity to analyse the resulting data in a more sophisticated way has been missed. It is obvious in figs 4, 5 and 6 that a linear model is not appropriate but still this has been fitted. It is therefore not a surprise that the statistical results do not support a linear model. A more appropriate consideration is- is there any change between low and high R in any parameter, e.g. variability or medians? This should be considered.*

Concerning scientific matters, first of all I am glad that the referee agrees with the main point of my paper. While it is not immediately obvious to me why the linear model is inappropriate for the corrected values of the variables shown in the right-hand panels of Figures 4, 5 and 6, one can certainly investigate whether and how the median and the variance change with sunspot number. An analysis of both will be included in the revised version of the paper.

> *Throughout there is an assumption of a linear relationship between sunspot number and cosmic ray aerosol production. What is the basis for this ? The variable experimental work probably does not support this, for example there is a square-root relation between ion production and ion concentration.*

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The choice of a linear function was not really based on an assumption about the physics of some underlying process. A linear relation just represents the simplest functional form to test (a point where I agree with the original paper). As already explained above, this does not exclude a weak dependence on solar activity of a different form. A comment on this will be included in the modified version of the paper.

> *It is necessary to be clear that there is no evidence of a cosmic ray effect on aerosol production on the assumption that cosmic particle formation would somehow generate a linear trend. The title should be changed to reflect the finding that an assumption of a linear trend was not supported. Formation of ultrafine particles from cosmic ray ions behaving differently may still be occurring.*

I agree with the referee that the wording is not entirely appropriate in a few places (including the title). The important point is not to rule out *any* effect of cosmic rays on the atmosphere, but to demonstrate that there is no observational basis for Weber's hypothesis. The title (and the wording in a few other places) will be changed in the revised version of the manuscript (see also a similar comment by the second referee).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2297, 2011.