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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Recent variability of the solar spectral irradiance and its impact on climate modelling" *by* I. Ermolli et al.

Anonymous Referee #2

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This manuscript contains a lot of good information from authors with a breadth of experience in the field. The manuscript, however, often reads more like a meeting summary and advertisement for future research and missions (both warranted). Although it summarizes a number of problems/issues with the measurement and modeling of SSI and modeling of the atmospheric response to SSI variations, I do not find this manuscript to provide any particular clarity in resolving or understanding them much beyond what is already extant in the literature. It is not clear to me what the take-away message to a climate modeler is, for example. What SSI dataset do I put in my model now?

I understand that the authors are trying to motivate a more formal intercomparison of

measurements and/or models, and in the case of the CCM results they are necessarily comparing apples and oranges, as the simulations were conducted independently. It is important, however, that the authors be as precise as possible regarding what data and model output are presented in the manuscript, and several of my Comments are related to this.

MAJOR COMMENTS:

(1) What "SORCE" are we talking about?

The SORCE mission has two instruments relevant to this work: SOLSTICE (115–310 nm) and SIM (200–2400 nm). There are numerous instances in the manuscript where the authors say "SORCE/SIM" or "SIM" where I think they mean "SORCE" (implicitly meaning SOLSTICE + SIM). "SORCE/SIM" is called out in the Abstract (p. 24559, l. 24). What happened to SOLSTICE? This must be clarified throughout the manuscript. Both instruments imply a larger-than-expected solar cycle variation, not just SIM (e.g., p. 24563, l. 24). There is significant wavelength overlap between these two instruments. Which was used in the various plots and model simulations in the manuscript? It is not always clear. How do SOLSTICE and SIM compare over 200–310 nm, the band responsible for SW heating by ozone and ozone production?

Note that the SORCE PIs have been recommending for the last couple years (at least) that the community use SOLSTICE for wavelengths short of 240 nm and SIM at longer wavelengths, and this threshold should be moved to even longer wavelengths when the Sun is relatively less active. This recommendation stems from the fact that SIM's SNR is essentially 1 at shorter wavelengths. This is absolutely critical to this discussion. When results in this manuscript report the use of SIM data at wavelengths

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less than 240 this should be noted explicitly with that caveat (and I frankly question what such results even mean).

There are also a number of versions of the SOLSTICE and SIM datasets used in recent papers or now available. SOLSTICE v. 11 (and now 12) released in spring 2012 reduced the magnitude of the inferred solar cycle in SSI by essentially a factor of 2(!) compared to v. 10, so most if not all the previous work cited and the model results presented used a larger solar cycle in this region (if they used the SOLSTICE data at all). Further, SIM data are only publicly available at wavelengths longer than 310 nm (despite the fact that SORCE launched nearly a decade ago), so the source of the SIM SSI needs to be clarified.

The altitude of the change from in-phase and out-of-phase ozone response (with respect to TSI) is discussed on p. 24602, in both the satellite observations and models. This is critically dependent on the selection of SIM vs. SOLSTICE, data version, and wavelength range. This manuscript has to be more precise about what is being presented.

LESS-MAJOR COMMENTS:

(2) The manuscript reads like three separate papers stitched together, with separate outlines and conclusions in each section. Perhaps the collection of authors is "inter-disciplinary," as stated in the the final conclusions (p. 24609, l. 11–12), but the paper seems more multi-disciplinary than inter-disciplinary. The outlines and conclusions should be consolidated, and I think the bulletized overviews, while fine in a talk, are overkill for an article of this length. The essential conclusions should be reflected in the Abstract.

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(3) There is an excessive use of section references in the text: “see Sect. 2.3” “see previous section.” It really makes for a choppy read, on top of all the outlines and conclusion bullets.

(4) The next-generation SSI instrument (TSIS) referred to on (p. 24562, l. 17) should be named and/or referenced.

(5) The manuscript talks of the precision *goals* of both SOLSTICE and SIM (p. 24568, l. 25; p. 24569, l. 15–17). What has actually been achieved would be more useful at this point.

(6) p. 24570 sounds like a workshop report (it is). Why the meeting was held, what it concentrated on, new methods being developed, etc., are not particularly useful in the context of a review article. (What would help move this work forward is publication of the new degradation models and revised datasets.)

(7) The first paragraph of p. 24571 sounds like an advertisement for TSIS. The fact that it was undergoing calibration at LASP at the time this manuscript was written is not useful information. Future missions would more appropriately be mentioned in the final conclusions.

(8) *“. . . may be smaller by half based upon new calibration corrections”* (p. 24577, l. 4–5). Are we really talking about SIM, or SOLSTICE? In either case, there is no reference. I assume you are referring to the discussion on p. 24570. Again, that work needs to be published or described in more detail here.

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(9) Table 2 should include information on which SORCE data (SIM vs. SOLSTICE), versions, and wavelength ranges were used in the model calculations, because *"When using SORCE/SOLSTICE data the temperature response is approximately half of the SORCE/SIM response indicating the large sensitivity of this model to difference SSI data sets"* (p. 24602, l. 3–5). Precisely, and I think that sensitivity has to be true for all models that are computing SW heating correctly.

(10) Fig. 2: The time period should be spelled out. The reader should not have to search Sects. 2 and 3 to find the time period.

(11) Fig. 7: Please add a legend to the figure (like Fig. 8).

MINOR COMMENTS:

(12) It is not necessary to mention soft x-rays in the Abstract; they have no climate relevance.

(13) It is not necessary to capitalize solar spectral irradiance and total solar irradiance in the Introduction (p. 24560, l. 12 and 15).

(14) *"Variations of solar UV lead to changes in stratospheric ozone and heating, and hence to indirect amplification. . . ."* (p. 24561, l. 3–4). What is meant by indirect amplification should be explained.

(15) Typo: *"challe.g."* (p. 24564, l. 20).

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(16) *"...raises doubts about the consistency of the observations from SORCE"* (p. 24574, l. 8–10). Consistency between SIM and SOLSTICE or SORCE and other measurements?

(17) *"Haigh et al. (2010) were first to publish the important implications of the SORCE/SIM data...."* (p. 24599, l. 24–26). Actually, Cahalan et al. (2010) published six months earlier, showing the effect on middle atmosphere temperature with the GISS modelE.

(18) Typo: *"tthe"* (p. 24604, l. 3).

(19) Typo: *"is is"* (p. 24605, l. 11).

(20) *"In order to better understand solar-induced climate variability and estimate uncertainties and sensitivities of single climate model responses in a more robust way, coordinated climate model simulations are needed, using a range of SSI estimates which are presented for the first time in a comprehensive way in this paper"* (p. 24605, l. 12–16). I personally think effort would be better spent getting the SSI measurements right first. The solar cycle derived from SOLSTICE data has already been revised down significantly. It is quite possible that SIM will also be reduced and be more consistent with previous measurements.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 24557, 2012.