

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

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Misleading, not reviewing, is what this paper does to SRPM. I did not participate in this paper text and had not seen it before was sent to ACP. In the acknowledgements, your paper has the misleading statement: “and J. Fontenla for helpful comments”, which is false since I gave no comments to this paper besides the current. Moreover I only learnt about your paper submission from a third party colleague. Therefore, you should drop out from the acknowledgements any reference to my name.

My reply to yours is, in short, that your answer is not fully satisfactory because it does not indicate that you will let the atmospheric modelers know the important issue about SRPM, i.e. that SRPM current models are based on published ground-based and space-based observations and that the negative visible and IR behavior was first in-

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troduced into the Fontenla et al models in 1999 in response to ground-based observations.

Your reply it does not indicate your purpose of really reviewing the proper SRPM material, and including the complete paragraph in Fontenla et al (2011) saying that the negative visible and IR behavior was introduced in SRPM modeling in 1999 in response to the published observations during the late 1990s. Instead, your reply seems to imply that you will continue to arbitrarily cite words out of context and twist the context of my papers.

Specifically about your points:

1) You should include that: "SRPM presents models of solar surface features that are the improvement of the previous semi-empirical FAL models (e.g. 1993). The current models are based on all observations of radiance and irradiance from the ground and space. The spectra computed from such models consider NLTE in 50 species and many lines and continua and is validated by comparison with observations of radiance and irradiance." This is not a lot of text but gives a couple of other essential points.

2 and 3) are one and the same issue. Your paper contains several misleading sentences: "Fontenla et al. (2011) also somewhat modified the original plage and penumbra models, to achieve better agreement with SORCE/SIM and SORCE/SOLSTICE SSI observations."

Then you also have the following sentence "While spectral contrasts calculated with SATIRE and COSI are very similar, the SRPM model predicts a very strong decrease of the contrasts in the visible and even yields negative contrasts at some wavelengths. This is not very surprising, however, since the SRPM models shown in Fig. 6 are the most recent versions of the Fontenla et al. (1999) model family that were tuned by Fontenla et al. (2011) to better match SORCE/SIM measurements in the UV and visible (see the description of the SRPM model later in this section)." Which contains comments to other models that are irrelevant to the description of SRPM, here you can

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save space on that.

The first statement misrepresents SRPM in that that the changes in the penumbra were motivated by Ly alpha, Ca II and Mg II observations. Besides, it contradicts you own words in a following statement. In the second of your statements the words “to better match SORCE/SIM measurements in the UV and visible” were taken from my paper out of context. The complete paragraph in Fontenla et al (2011), i.e. the paper you currently reference, is very clear and reads as follows about the negative trends of the visible and FIR: “These effects are due to the change of the photospheric/low-chromospheric temperature derivative with respect to pressure of the various feature models. In the current models this derivative is slightly shallower for increasing activity models (except for the sunspot ones), and the temperature versus pressure curves cross at pressures slightly lower than that where the optical depth at 500 nm is unity. A similar choice for the models was present in FAL models and in work by Fontenla et al. [1999] and corresponds to published observations that show a negative correlation of the continuum with magnetic field at some wavelengths [see Topka et al., 1997; Sobotka et al., 2000]. Table 4 Further improvements were done in the models in order to better match the SORCE/SIM data shown by Harder et al. [2009] that covers more wavelengths.”

Your sentence: “Since the solar atmospheric models were specifically modified to assure better agreement with SORCE/SIM observations (including the unusually strong variability in the UV and the reversed variability in the visible), the SRPM reconstruction cannot be considered as an independent test of these data. Despite this adjustment, the UV variability in the SRPM reconstruction is significantly weaker than what is measured by SORCE/SOLSTICE (Fontenla et al., 2011) and no detailed comparison to the SORCE data on rotational and cyclical time scales has been presented by now.” This sentence makes up that recent SRPM models produce such unusually strong UV variability, but in fact none of FAL models have modified the FAL models to match SORCE near-UV observations (only the Ly alpha variations were matched through a model

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change in the “cloud layer”). My current models underestimate the near-UV variations with respect to SORCE and it is for that reason that they do not match the TSI. Your text is making up unjustified statements and misleading in that way too.

Your reply claims to simplify but it actually completely misrepresent SRPM current results and models. My 2011 paper is very clear about the facts, and these can be easily corroborated by unbiased reading of the Fontenla et al (1999, which is an original paper not a review). But you need not do that; you should use my 2011 paper reference that has the very clear statement above. (FYI, the negative visible/FIR behavior was not in FAL 1993, which Solanki/Unruh 1996 used, because the ground based observations used in 1999 were not yet published. FAL introduced them between 1993 and 1999.)

4) You should also drop from SRPM section the sentence: “though see comments in the OAR description below), or that the temperature structure of some of the model atmospheres does not fully represent the solar atmospheric structure.” In the first place OAR never used SRPM, they have used the atmospheric models of SRPM within a very different radiative transfer code, and even then it is not clear that they included all the data in the SRPM models. Also, it is your speculation that “or that the temperature structure of some of the model atmospheres does not fully represent the solar atmospheric structure” maybe someone’s opinion but has nothing to do with a review.

Besides, OAR knew, even with their LTE code, in 2004 (before SRPM had shown the trends) that Fontenla et al (1999, extensively listed) models produced negative visible and IR contrast. The paper: V. Penza¹; B. Caccin¹; I. Ermolli, and M. Centrone, A&A 413, 1115–1123 (2004) DOI: 10.1051/0004-6361:20031397 Says: ” However, a careful analysis of Fig. 11 and of Tables 1 and 2 reveals the presence of a negative contrast at the center of the disk for modP (both in the red and in the blue band) and for modH (only in the red band). This behavior does not have an experimental verification in the PSPT data and is due to the particular photospheric temperature structure of the P and H models, that is colder than the average Sun. Fontenla et al. (1999) affirm to have intentionally modified such models in this way in order to reproduce the center-

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to-limb variation and facular contrast measured at 1.6 m (Foukal et al. 1990; Topka et al. 1992; Wang et al. 1998), that shows a negative contrast. Actually, as discussed by Foukal & Moran (1994), Sobotka et al. (2000) and Sanchez et al. (2002), faculae and pores seem to share the same physical mechanism (lateral heating and inhibition of convection), which simply changes its contribution as a function of the size of the structure. So structures presenting negative contrast both at 1.6 m and in the visible range should be defined, more correctly, as a pore.” Which shows that OAR was aware of the changes introduced in 1999, contrary to what the sentence in your paper (in the OAR section) says: “Thus the set of components in the OAR model is essentially the same as used by Fontenla et al. (2011), though their atmospheric structure is based on Fontenla et al. (2009), i.e. before the modifications to better match SORCE spectral observations.” This statement is incorrect and should be changed. In the first place it is not clear what OAR model is and the atmospheric structure by Fontenla et al 2009 was not numerically published so I do not know how your paper can state that they used the same as mine. Besides, it is plenty clear in the Fontenla et al (2009) paper Fig. 2 that that set of models produces the same negative visible and IR and already discusses the same issues as the 2011 paper (see the end of its DISCUSSION and CONCLUSIONS section). After this, it is not clear to me what is that the OAR people did and which of FAL models they used, and I doubt very much anybody in the atmospheric community can figure this out either.

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