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Interactive comment

# *Interactive comment on* "GREEN: A new Global Radiation Earth ENvironment model" *by* Angélica Sicard et al.

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The GREEN model is a collage of several regional models, mostly those developed by the authors themselves. The models are superimposed upon each other by direct replacement with a priority scheme based on location, energy, and species. It is described in the manuscript (but not in the title or abstract) as a beta version, meaning it will need additional work before it can be used for satellite design. I concur with publication only minor changes if it is designated as a beta, but I do not think this is made clear in the title or abstract, and it should be made absolutely clear. Satellite designers will be confused or even led astray trying to use this model when it arrives in OMERE if it is not brought up to a higher level of quality or more obviously indicated as a beta, not yet ready to be used for design.



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Discussion paper

==>The term “beta version” has been added in the title of the paper.

As a developer of the AE9/AP9-IRENE global radiation belt climatology models, I was very interested to see whether this team of very gifted scientists has developed any new ideas to address the problems that AE9/AP9-IRENE continues to wrestle with. Unfortunately, at this stage of development of GREEN, its synthesis of the different constituent models is very superficial, meaning the underlying challenge of producing a truly global model has not really been attempted. As shown in figures 13 and 14, the GREEN model has large, sharp discontinuities at the boundaries of the underlying models. This will lead to strange results for orbit surveys looking at variations with altitude or inclination.

==>Yes, you’re right, there are some sharp discontinuities at the boundaries of the underlying models. A comment of the other reviewer was also on these discontinuities so I add in the paper some comments about this point. I try to show what can be done with a very simple smooth function but obviously it is not so easy. For now, we do not know how to attenuate discontinuities otherwise than improving each of the underlying models. As you well know, it is a critical step in the development of a global model.

Further, the treatment of temporal variability is similarly superficial: "worst cases" are taken over whatever duration of time the source data provided, and so cannot be applied to the user’s mission duration with any corresponding confidence level. How will the engineer know whether to add additional margin (and how much) on top of the model output?

==>It is a choice on our part to not take into account confidence level in GREEN model. For now, we try to answer as best as possible to the first need of space industries, which is a “mean” model. We do not pretend to make a “worst case” model for internal charging for example. The notion of “maximum mean flux” in the outputs of GREEN is just an upper envelop which take into the variations of flux from one solar cycle to another (as in the case of IGE-2006). The notion of worst-case is not addressed in his paper.

The data-model disagreements shown in Figures 15-18 are comparable to the ones that apparently lead AE9/AP9-IRENE to be "very controversial." How are we to know whether the first non-beta release of GREEN will actually resolve these discrepancies? I suspect from my own experience with AE9/AP9-IRENE that some discrepancies are essentially impossible to resolve definitively because the underlying data sets themselves do not agree. This means a more robust approach to model errors will be required.

==>You make a point. . .GREEN is probably as “controversial” as AE9/AP9-IRENE at the boundaries of the underlying models. I remove form my paper the word “controversial” that seems to bother you. You’re right, some discrepancies are very difficult to resolve. We hope in a future to smooth GREEN model using our physical model Salammbô. But the way we could do that needs to be defined.

I am also uncomfortable with the inner zone correction. It appears to be rather adhoc. The Boscher et al 2017 paper (which is a very nice paper! and is now available on the IEEE explore website) only really looked at observations of one energy channel. Likewise, the IGP model of protons at geostationary orbit is rather ad hoc and has not been validated. I agree with the authors that these models make "reasonable" assumptions and extrapolations, but they need to be validated somehow. Also, it would be good to cite the RBSP papers by Li (doi:10.1002/2014JA020777) Claude-Pierre (doi:10.1002/2016JA023719) for the inner zone electrons.

==>For the inner zone electrons, I add the two references you mentioned. For the IGP model, it is really difficult to validate it because there are no good proton data at GEO. CRRES/MEB and THEMIS/SST are contaminated, RBSP has no data at GEO. So unfortunately, I can not validate IGP model.

This is a fair start to what will likely be a long and challenging effort to build a truly global model. The paper needs to be a bit more straightforward about the model’s state of progress. There is still much work to do. But, this is real progress worthy of publication.

Minor points: In the abstract, I suggest noting that the model covers all local times (in addition to "all along the magnetic field lines.").

==>Ok

In the introduction, first sentence, add "satellite" before industry.

==>Ok

Page 2, line 2 "no doubt that the obtained averages are more accurate than AP8 and AE8..." I hope the authors are correct! But, they should include AE8/AP8 and probably AE9/AP9-IRENE v1.5 in figures 15-18.

==>Comparison with AE9 in Figure 16 and Figure 17 and some comments have been added in the paper.

Section 2.1: L* and B/Beq are incompatible coordinates. L* is a drift invariant, but varies with B/Beq on any given field line, while B/Beq varies around the drift orbit. This means that particles with different coordinates are being mixed together depending on where they are measured. Replacing B/Beq with Bmirror solves this problem (or, as Selesnick doi.org/10.1002/2017JA024661 has done, mapping Bmirror and L* to a dipole value of B/Beq. I believe Salammbo does something similar).

==>I am not sure to understand what you want to say. Actually, in the GREEN model, we use the 3D grid of Salammbô, in Ec, yeq=sin($\alpha$eq) and L*. But, as the term yeq or even the term “pitch angle” is not always known or understood by the reader, we have chosen to speak of B/Beq in the paper. When we develop a model using in-situ data, we consider that a given location (x,y,z) corresponds to a couple (L*, $\alpha$eq). $\alpha$eq is calculated by considering the first invariant such as $\sin^2(\alpha\mathrm{eq})/\mathrm{Beq} = \sin^2(\alpha\mathrm{mirror})/\mathrm{Bmirror}$ in which we consider that Blocal at x,y,z is Bmirror.

What value of F10.7 should be used when running the OPAL model? Is there a long-term forecast built in for using it to design satellites to be flown in the future? This should be discussed, especially since LEO proton fluxes are anti-correlated with F10.7, and some naive users might attempt to be conservative by inputting a very large value of F10.7.

==>These comments have been added in the paper: “As OPAL depends on the radio flux F10.7, an input of OPAL is the date. So, for a given date chosen by the user in the past, the real F10.7 value is used to calculate proton fluxes. But for a given date in the future, it is not so easy because the F10.7 value is unknown. Consequently, a statistical study has been done on F10.7 values from 1947 to now in order to define a mean F10.7 value for each of the eleven years of a solar cycle. Thus, for a given date chosen by the user in the future, the year of the solar cycle is predicted (from year -6 to year +4, 0 being the year of the minimum) and according to this one, the corresponding mean F10.7 value is used in OPAL to calculate proton fluxes. Moreover, added to the mean protons fluxes, OPAL provides an upper envelop considering the variation from one solar cycle to another. Taking into account that high energy proton fluxes are anti-correlated with F10.7 values, this upper envelop is calculated using the minimum of F10.7 value measured since 1947 for each year of solar cycle.”

I do not think Figure 18 supports the claim on page 15, line 15 that, "If the period of time of in-situ measurements is long enough (several solar cycles) or is representative of a mean flux, data will easily be compared to GREEN- results." What it tells me, instead, is that the model-data disagreement can be factor of 4 or more, even when matching the solar cycle phase of the model and data. This suggests either a systematic issue with the model, the data, or something that must be addressed with error bars on one or the other.

==>What I wanted to say here is that differences between mean model and data can not be due to a problem in the model or in the data but just in the fact that the time period of the data can correspond to a calm period in term of magnetic activity or flux. So using short term data (few years only) to develop a model can be dangerous because the time period of the data (active or calm) can have significant consequences on the resulting model and can not be representative of a mean flux. However, as a model developer, I am fully aware of the difficulty of having good quality data for a long period of time. However I agree with you when you said that “the model-data disagreement can be factor of 4 or more, even when matching the solar cycle phase of the model and data”. Actually, due to the variation of flux from one solar cycle to another, even when the solar cycle phase is the same some differences can be observed between data and model. This is why, as it has been done in IGE-2006 model, we try to estimate an upper envelop of flux taking into account the variation from one solar cycle to another.

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