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Interactive comment on "Assimilating solar-induced chlorophyll fluorescence into the terrestrial biosphere model BETHY-SCOPE: Model description and information content" *by* Alexander J. Norton et al.

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First, the paper is mostly well written and quite timely. Thank you for this valuable contribution.

I have two large concerns/recommendations and a few small ones.

First, the conclusions of the paper are shocking to many people familiar with modeling SIF and using it in the context explored here, to constrain GPP. The expectations of uncertainties on the order of 2.8PgC /year on GPP seem far too optimistic. Why do they come up so optimistic? I am left to believe that the assumption of a perfect model

structure and the errors only arising from the uncertainty in parameters lead to such results. For example, most models can not come close to reproducing the magnitude of GPP from SiF, only temporal dynamics. The authors' conclusions are fine given that the readers are working from the correct set of assumptions. I felt that this assumption of perfect model structure should have been introduced as a stronger caveat to warn those "abstract surfers" who rarely have time to digest an entire paper, about the practical limits of the work, as our models now stand.

Second, I won't imply there are errors in Section 2.4 but it is written in such a confusing manner that my guess is over half, if not most readers, will not be able to follow it. I don't believe there are any complicated statistics in there, but the relationships between the number of independent samples and grid observation resolution is really unclear. I recommend a complete rewrite of this section (Page 7, lines 9-26 mainly) and/or a short appendix/supp material section illustrating EXACTLY what you are talking about with a concrete example.

Pg 5/Line 29: I assume this assumption "This means that we optimize . . . quantities." is tantamount to ignoring any model error that would stand in the way of a "true" estimate of GPP from satellite SIF? If so, it should probably mentioned.

Pg 7/Line 13: Are the actual units for SIF ever mentioned?

Pg 7/Line 9: It is often hard to interpret whether the random variable of interest is the spatial variability of grid cell means or the variability of a single grid cell mean. Again, more precise terminology and definitions would often help.

Pg 7/Line 24: Again, the main problem here is that readers are used to seeing satellite observations whose associated errors are large at the single sounding level but get smaller as many samples are averaged together (larger spatial scale mean value). This text runs counter to that thinking. One is essentially *assuming* far stronger constraints on the data as you move to finer scales. Again, an example along w/ the equations would make this much more clear to all readers, let alone those w/o an extensive statistics background.

Pg 8/Line 2: Traditionally, the issue w/ uncertainty in SW radiation has been w/ an overestimate of SW from GCM reanalysis. This is thought to result from a lack of characterization of fine scale clouds due to poor model resolution. See http://nacp.ornl.gov/docs/AGU_Ricciuto2009.pdf, not sure if Ricciuto ever published it but it's a reasonably well known problem. So, I guess the question is, how would an unknown overestimate of 20% in shortwave radiation affect the conclusions?

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