

Review of Lolli et al., ACP -2016-980

I applaud at the inclusion of the new midlatitude dataset to the comparison, as well as a better evaluation of the origin of discrepancies between the FLG and CP models. However, I still think the authors stop just a step short of going to the bottom of the problem. Also, separating the CRE into the LW and SW components would help in understanding the results better in some of the manuscript sections.

Comments

1.) Lines 17-18: "Specifically, CP simplifies...LW..."

You don't mention the simplification of the SW CRE calculation here, despite referring to the parameter y^* (related to simplifications of SW flux calculations in CP model) further in the text?

2.) You might want to mention the Greenbelt site in the abstract.

3.) Chapter 3.1 – model sensitivities for -> daytime only? Please specify!
It would be also valuable to include information on LW and SW CRE component, not only NET.
I think that should be done already in this study (considering its simplicity), and not simply left as an outlook (which you state in lines 310-313).

4.) Chapters 3.2 and 3.3
I don't see what is the advantage of having both years separately, and not clustering the results together. If you keep them separately, you need to understand what caused a different value of the CP model bias (suggestion: Does it correlate with the surface temperature?)

And – if you keep two separate panels - Were the 2 years, meteorologically significantly different? Different ENSO phase, etc.?

5.) Lines 205 – 214 :
I would suggest first stating the general conclusion (CP larger values for 40-60%) , followed by an example (1 W m^{-2} vs. $1.4\text{-}1.6 \text{ W m}^{-2}$).

"..shows higher variability depending on the year..."
Yes, but year isn't the root cause of the change. What was different between 2010 and 2011? Maybe surface temperature?

Lines 213-214: What do you mean by "must carefully be determined with these models"
Which models? CP? FLG? Other?

6.) Lines 235-236:

I don't see any significant differences in the error between the 2 sites.

7.) Mention why CP2009 used σ^* and k^* and not simply k and σ .

8.) Lines 247-250:

Making a step further and trying to understand better what exactly is causing the error is something one could quickly look at. For instance, based on the Fig. 1 in CP2009 and your text lines 261-265, one would expect the error to be related to temperature, at least in the tropics. Your current conclusions of Chapter 3.4 do not add much to what already stated in CP2009.

Therefore I would suggest (at least) one new sensitivity test in which you would mask out lidar measurements in Singapore with $T > 288$ K and try to understand if that leads to a better agreement with FLG model. (or select measurements at warmest/coldest surface temperature conditions for both Singapore and Greenfield).

This sensitivity test would be useful in better determining the conditions at which the CP model's bias is still at acceptably low levels (maybe those stated in CP2009 paper?).

9.)

Figures 1-6:

I still don't see why not making a technical effort with Matlab to cluster the years 2010 and 2011. Couldn't you just make a sum of the two histograms/bar plots, as the x-axis values do not change?

This would sharpen the main points of the paper.

Minor comments

-be consistent with units – you use both $W m^{-2}$ and W/m^2

-line 53: delete the dot (.)

-line 98: Polar Regions => polar regions

-lines 99-100:

Why Singapore more digits than the mid-latitude site?

-line 104 – What do you mean by:

“...polar clouds that should be net cooling elements”

-line 140: 39m => 39 m

-line 273: ...value for **theses** parametrizations (?)

-line 285-287:

-Why are the COD ranges different in Greenbelt and Singapore? Can that lead to differences in CRE?

-Why are the net CRE so different when comparing 20 sr with 30 sr results?
Could you briefly mention that in the text?