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Interactive comment on "Coupling sky images with three-dimensional radiative transfer models: a new method to estimate cloud optical depth" *by* F. A. Mejia et al.

Anonymous Referee #3

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Review of "Coupling sky images with three-dimensional radiative transfer models: a new method to estimate cloud optical depth" by Mejia et al.

The manuscript introduces an interesting approach to derive optical depth of clouds from ground-based radiance measurement in a few spectral channels. The combination of red-blue radiance ratio and an absolute radiance measurement is suggested. Obviously the measurement of optical thickness (OT) from simple standard RGB all-sky cameras would be valuable information for the solar energy producing sector as suggested by the authors. Unfortunately the manuscript has some major deficiencies.

I recommend major revision of the manuscript before publication should be considered.

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Major points in order of relevance (most important first):

"3D issue":

Already the title of the manuscript leaves the false impression that 3D radiative transfer (RT) is of central importance to the method and that the consideration of it is a core innovation of the method. Both is not true. A 3D model was used to solve RT for simple 1D homogeneous cloud/ sky situations only. Something that could have been accomplished with every 1D RT model. The use of the 3D model might have been for convenience only, but reasons are not given. Neither a detailed quantitative analysis of 3D RT influence specific to the suggested method nor any way to compensate or correct for it is presented. This false impression should be completely removed or the content with respect to 3D RT should be considerably strengthened.

Discussion of results/ requirements:

Accuracy requirements for the method to be developed should be derived in the introduction section. These are missing completely until very late in the discussion section. Errors found for the new method seem large. Especially for the regime of small OT most relevant for solar energy production. Discussion of this with respect to the needed accuracy is weak.

Calibration issue:

The authors introduce the use of a radiance measurement as fundamental to the method. The needed absolute calibration of the proposed RGB cameras to provide such a radiance stays very unclear and seems to be insufficient. Related to the "discussion of requirements and errors" issue.

Unprecise presentation:

In some places throughout the manuscript acronyms are not introduced. Quantities

are called "large" or "small" without orientation on values. Important information on requirements and assumptions is given too late.

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Details:

11286, line 2: What is USI?

11286, abstract and beginning of introduction: Already after the first mentioning of errors the reader wants to know which range of optical thickness values is of interest for you and which range of values were tested. What OT range and result accuracy is relevant for solar energy production?

11287, line 19: What is the threshold OT to differentiate between clouds and clear sky?

11288, l 2: some text missing here

11288, l 20: Unclear sentence. Where do the contrails come from? You mean, if one knows the given clear sky values, one can detect thin clouds? What dependencies have been considered to do so? Please clarify.

11289, sentence line 19 ff: What is a large optical thickness typical for clouds? Larger than 0.3? Please clarify. I do not understand the end of this sentence.

11290, line 15 and 11291, line 3: Your RT simulations are on 161 homogeneous single layer cloud cases only? No need for 3D model.

11290, l 24: You do this for liquid water clouds only? This would be an important information which should be given much earlier (abstract, title).

11292, line 11: These are strong assumptions which are most likely only valid to some extent. How big are the likely errors due to the non-perfect lens?

11292, line 16: You calibrate with a remote sensing retrieved OT? No lab calibration? I guess accuracy of such a "calibration" of absolute radiance can only be in the range of

10-25% or whatever is the accuracy of the retrieval used for calibration. What accuracy do you need for your radiance measurement? You have to discuss that.

11293, line 10: These have to be calibrated somehow. I guess most are not. Please discuss.

11293, line 22: What is "near 0"? Stray light probably affects the measurement up to optical thickness of about 5, don't you think? As long as you do see a clear solar disc through the cloud. Again this is related to the question in which OT values PV solar energy production is interested in?

11294, line 4: It is hard to understand terminology only introduced in the following sections ("interpolant described in Sect. 5").

11299, line 6: Shouldn't the references read "Min and Harrison, 1996b, 2003"?

11299, line 16: GHI not have the ambiguity problem as radiance, right? That means, growing OT is directly related to optical thickness always. Please mention.

11299, line 17: What is the impact of your 8 mu assumption on accuracy? How do you decide that you have liquid water clouds? What is the assumed accuracy of the Min et al method? Which version do you use in the following, with or without MWR? Please discuss.

11300, line 9: Does that mean that the accuracy of the MWR is about 50% in the OT range 6-12? This is a large uncertainty?

11300, line 15: It's not only the non-linearity. There are real 3d effects also. One relevant here is named "tunnelling" or "channelling", which works in the same direction. Then there is the apparent cloud cover problem at oblique solar zenith angles, working the opposite direction ...

11300, line 16: How does that adjust for heterogeneity? Please give more detail? Equation 9 does not make sense without the sum over all logs.

11300, line 20ff: Please explain. You used the Min OT data to calibrate your sensor in the first place. Instead of finding perfect match of results, you do find 20-50% RMSE. What does that mean? Does your method worsen the results from Min algorithm by using RBR? Why should one use your method if Min is the more reliable for overcast skies as you state. You do not have any solution for broken skies either? You have to explain and discuss these points.

11301, line 6: You think 48% RMSE is good agreement? You have to explain your requirements.

11301, line 8: What is this in percent? Must be much higher than above mentioned?

11301, line 11: Who measures "entire hemisphere"? The MWR? Rather not? Please explain.

11301, line 20: What is MBE? Some bias? Please introduce terms.

11302, line 2: All uncertainties for thin clouds seem to emphasize that no method produces trustworthy values for OT<5. This leads to the question: above which OT value is it useful?

11302, line 13: You obtained this A value by comparison to RRBR measurements and then you validate RRBR against the DNI measurements? This sounds awkward. This way any bias is excluded?! Please clarify or discuss.

11304, line 10: What do you mean with "consistent"? RMSE 20-80% and bias up to 30% in some regimes is pretty questionable, isn't it?

11304, line 18: Finally a requirement is given. OT 0-10 is relevant for PV. You need to discuss all errors with respect to this range. Unfortunately this means that no method, neither old nor your new one is good enough.

11305, line 13: You have to introduce these points much earlier (introduction). Throughout the manuscript it stayed unclear what you try to reach.

Table 1: What is an effective radius of 3.9 mu? Is this a cloud value? I thought you use 8 mu? Please explain in text.

Table 2: What is MAE? How can it be that R2 values for "All" cases of Min method are not reached in any sub-category? Same for MWR. Does the line "MWR OT<5" mean that a bias of 5.1 is reached for values smaller than 5?

Figure 4: For tau_c=10 please use a different line style. It is hard to differentiate form tau_c=0. Caption: "red and blue channel".

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