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Atmospheric Measurement Techniques Discussions

Interactive comment on "A fast method for the retrieval of integrated longwave and shortwave top-of-atmosphere irradiances from MSG/SEVIRI (RRUMS)" *by* M. Vázquez-Navarro et al.

Anonymous Referee #1

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General remarks The paper represents and evaluates an approach to infer upwelling longwave and shortwave fluxes from MSG/SEVIRI. While similar algorithms have been described before in the scientific literature, this paper is still of interest to future users of the resulting data products. There are, however, a number of concerns in the methodology and presentation of the paper, which I would like to see addressed prior to accepting the paper, and which I list below:

1. Title: maybe add "upwelling" to the title?

2. High spatial resolution: I do wonder whether the authors goal, i.e. to obtain a

higher spatial resolution, is really achievable/physically meaningful. Their calculations are based on independent column (ICA) calculations at SEVIRI 3x3km^2 nadir resolution. Considering 3D radiative transfer effects, however, TOA fluxes are hemispherically integrated quantities. At any given location, e.g. at a height of 100km, the downlooking integration will average radiances from a much larger domain than the 3x3km^2 of a single SEVIRI pixel, and contain contributions from most likely a quite heterogeneous scene composed of different cloud types/surface types. Due to this averaging, small-scale variability present in the radiance field as well as the ICA results will no longer show up in the fluxes. I suggest that the authors at least discuss this point, and do explain why they think that an ICA-based TOA flux at high resolution is physically meaningful.

3. Retrieval of OLR: Having gone through the EUMETSAT(2010) document before, I am somewhat surprised by the form chosen for Eq.1. It assumes that the Earth radiates with a mean temperature which can be obtained as weighted average of the individual channel temperatures. From my intuition, this does not correspond to reality (see Fig.1), where different channel features (clouds/water vapor/CO2) radiate with different effective temperatures. In contrast, EUMETSAT(2010) assumes that the total flux is a weighted sum of the narrowband fluxes, which in turn are derived from narrowband radiances, an approach which does not impose such an assumption. I thus wonder why the authors have chosen to use an alternative form to that of EUMETSAT(2010), and would suggest adding a justification for this. (If this was done only in order to obtain independence from the exact form of the spectral response function, I'd suggest converting SEVIRI radiances to equivalent monochromatic radiances at the nominal SEVIRI wavelengths instead, going via the brightness temperature). Overall, I suggest to compare the accuracy of both approaches (RRUMS vs. EUMETSAT), and to discuss the resulting differences. I think also that additional support for my scepticism regarding the chosen functional form is provided through Fig.2: while the authors state that they do not want to interpret the physical meaning of the coefficients, I am concerned that the strong zenith angle/scene type dependence of the coefficents are

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indications of either high autocorrelation of the channel radiances, or even or violations of the underlying fit assumption of a linear model. Maybe techniques such as stepwise regression could be used to select only the most relevant channels, or dimension reduction techniques such as principal component analysis could be applied to the SEVIRI radiances beforehand. I suggest also to include the fit coefficients derived for calculating OLR in a table or as sumplemental material so other researchers can apply/reproduce the results.

4. Retrieval of RSR: Please specify the training procedure for the neural network: have the validation statistics been obtained for a dataset not used in the training procedure? Otherwise, the authors should split the dataset into disjoint training and validation datasets! I am also suprised by the author's choice of using the neural network algorithm: if I understand correctly, the NN algorithm shows worse performance on the validation dataset, i.e. a bias of 4,5W/mˆ2 and a standard deviation of 33W/mˆ2, compared to 1W/mˆ2 and 25W/mˆ2 for the linear fit. Based on this finding, I was suprised that the NN algorithm performs better than the linear regression compared to CERES and GERB data, and assume that this is due to the too simple scene classification, which only uses two surface types, and no information on cloud type whatsoever (low/high, water/ice, ...) . Due to this limitation, I think there is little insights to be gained from the results from the linear model, as much more sophisticated scene type classifications are typically used for deriving TOA fluxes. Thus, my suggestion is to either completely remove the results for the linear model, or improve the scene type classification (at least by including desert as addtional surface type, maybe some other classes based on the IGBP dataset, and add a classification according to cloud type (ice/water/high/low/thin/thick clouds)).

5. Validation with CERES/GERB: Results in Table 2/3: it would be interesting to add bias/stddev per scene in addition to slope/correlation. I would suggest also to add at least a few comparison for collocated scenes of GERB/CERES, and to list differences between these 2 instruments, so readers can judge whether differences between both

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instruments and those to SEVIRI are of similar magnitude or significantly worse. Please also add some notes on the effects of SEVIRI calibration uncertainties on retrieval accuracy: it is well-known that the nominal calibration of SEVIRI differs strongly from the MODIS calibration for the visible channel (see work by Dave Doelling, as well as Ham and Sohn, 2010), also there has been a correction to the infrared radiance processing scheme in 2008, are the fit coefficients sensitive to this, and how might this affect accuracy?

Specific remarks:

p4971: Kiel and Trenberth (1997): maybe update to more recent estimates?

L13: "resolution possible nowadays“ => "currently available“ (technical feasibility is irrelevant here)

p4972, L19: EUMETSAT is an abbreviation and should be capitalized

p4974, l13: HRV is the more commonly used abbreviation, at least in EUMETSAT documents

p4981, l22: "a linear fit similar to the thermal irradiance“: when I first read the sentence, I only read "a linear fit to the thermal radiances“, and wondererd why thermal radiances were used for the solar part. I'd suggest to write: "a linear fit based on solar radiances similar to that used for the OLR“. This also avoids inconsistent naming (OLR vs. thermal irradiance)

p4983, l21: how do the authors know that "the neural network sometimes fails detecting thin clouds“? Does the neural network output whether it is cloud-free or not? Otherwise, this is speculation, and should be marked as such (and that is one of the drawbacks of neural nets: there is no physical interpretation of results possible)

p4985, "poorer resolution“ => "lower resolution“

Table 2: why is the result on Aug. 13 so bad (correlation of 0.7)? Is this an outlier?

Please check this result, and comment on the reasons!

Tables 2 and 3: I'd suggest to add standard deviation and bias per scene.

Fig. 4: Maybe combine 2 zenith angle bins?

Fig.3, 4, 9, 10, 12: I'd suggest to use densities instead of points, as it is hard to see how many points lay inside the black aeras.

Fig13: please add the time/date of scene acquisition

References

http://www-pm.larc.nasa.gov/ceres/pub/conference/Doelling.Eumetsat.04.pdf

Ham, S. and Sohn, B.: Assessment of the calibration performance of satellite visible channels using cloud targets: application to Meteosat-8/9 and MTSAT-1R, Atmos. Chem. Phys., 10, 11131-11149, doi:10.5194/acp-10-11131-2010, 2010.

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