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# *Interactive comment on* "Decoupling the effects of clear atmosphere and clouds to simplify calculations of the broadband solar irradiance at ground level" *by* A. Oumbe et al.

**Anonymous Referee #2**

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GENERAL COMMENTS

This paper aims to quantify the errors in assuming that properties of the clear-sky atmosphere and cloud properties can be decoupled in modelling the solar irradiance at the Earth's surface. It does this by analysing an extensive suite of radiative transfer simulations. The paper concludes that, except in extreme conditions, the error of the decoupling assumption is generally no more than the errors in high quality surface radiation measurements.

This paper is a useful contribution to the development of physically based operational systems for estimating surface solar irradiance (SSI) from satellite data. Computational efficiency is vital if such systems are to be practical, so it is important to quantitatively investigate the trade-off between speed and accuracy in the formulation of the underpinning models. This paper is novel in its evaluation of the clear-sky/cloud modelling split. The paper considers all of the important parameters in the simulations, and in most cases their realistic ranges (see the comment on droplet effective radius below). The analysis and conclusions are appropriate. All of this work is thoroughly though concisely presented. The writing is well structured and fluent.

This paper is suitable for publication in Geoscientific Model Development.

SPECIFIC COMMENTS

Pnnnn and Lnn below refer to page and line numbers.

The authors do a nice job of ensuring realistic combinations of model parameters. Nevertheless I wonder if 20 cases of P_c are enough, considering that each P_c is comprised of 7 parameters. Especially since statistics over these 20 cases are the key measure of whether the separability assumption is satisfied (the 95th percentile will separate a single case from all of the rest). If computational burden is an issue, perhaps fewer steps could have been used in some of the other parameters. Nevertheless the quality of the results, such as shown by their consistency between solar zenith angles, demonstrates that the set of cases studied is sufficient to support the conclusions made.

P2011, L26: A fixed value is used for the cloud droplet effective radius (r_eff) for each of water and ice cloud. What is the sensitivity to variations in r_eff? For instance, the near-infrared cloud reflectance would be expected to change with r_eff, and Nakajima and King (1990, Journal of the Atmospheric Sciences, 47, 1878-1893) studied the retrieval of r_eff over a range of at least 2 to 32 micrometres.

P2019, L2: The authors might consider noting the maximum albedo expected for desert regions (no more than around 0.5 I think), since many users of downstream products



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will be interested in the performance in snow-free arid regions.

Figures 3 and 4 plot results in relative terms. Consider plotting the results in absolute terms as well, since as you point out the absolute errors are important to consider in practice. Perhaps a second panel could be added to Figure 4 showing the same results as the current Figure 4 plot but in absolute terms.

P2016, L11-12: It is true that the uncertainty contributed by the assumption that changes in Pc can be neglected is within the WMO criterion of high quality measurements. However, in application there will be other contributions such as uncertainties in cloud property retrievals, aerosol amount and type, satellite calibration, radiative transfer model approximations, etc. The total uncertainty of output from any system using this assumption will be higher and probably outside the WMO high quality threshold.

TECHNICAL CORRECTIONS

P2019, L4: Change “pyranometer” to “pyrheliometer”.

P2019, L28: The authors intend to mean a reduction for each of points (i), (ii) and (iii). Therefore, “reducing” should be moved before “(i)” to say this.

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Interactive comment on Geosci. Model Dev. Discuss., 7, 2007, 2014.