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Geoscientific Model Development Discussions

Interactive comment on "An interactive ocean surface albedo scheme: formulation and evaluation in two atmospheric models" *by* Roland Séférian et al.

Anonymous Referee #2

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General Comments: The manuscript presents a new scheme for computing ocean surface albedo (OSA), and evaluates the scheme in two numerical models. While the existing suite of OSA schemes consider variations only due to solar zenith angle, the new scheme considers the various processes that contribute to the overall albedo. These processes include reflection by the ocean surface free of whitecaps, reflection by the ocean surface covered with whitecaps, and the reflection of solar energy that first passes downward through the air-sea interface is scattered within the ocean and then passes back through the air-sea interface to the atmosphere. Additionally, the study considers direct and diffuse irradiance separately as they can have very different OSA values due to their difference in angular distributions (via dependence of OSA

on solar zenith angle). Spectral variations are also considered. In summary, the new parameterization incorporates the relevant physics. In that sense, there is scientific merit to the improved parameterization.

To judge a broader scientific merit, the change in the skill of climate simulations that use the new OSA parameterization must be evaluated. This is not performed in the study. Rather, the manuscript indicates it will be performed in a subsequent study. The new scheme is justified by showing it gives OSA values that are much closer to observations than the old scheme. This is well and good, but isn't it the skill of climate parameters with societal influence (such as air temp, rainfall, storm frequency, etc.) that are the greatest importance? While the improved scheme gives OSA values that better match observations, the scientific merit of the scheme can only truly be judged once it is shown the improved scheme elevates the skill of models in simulating parameters beyond OSA. Ideally, this study would be published in conjunction with a study giving a broader evaluation of the new scheme's impact on climate simulations.

Along these same lines, it's not clear what, exactly, motivates the study. Climate model upgrades are typically motivated by the need to improve some aspect of climate model simulations, such as reducing a regional temperature bias (for example the warm bias in the eastern tropical Pacific). The Introduction indicates that OSA interacts with bio-physical processes, OSA receives little attention, and OSA parameterizations don't include all the underlying physics. While not clearly stated, it seems like the study is motivated by the fact that existing OSA albedo parameterizations are dated and there is now sufficient computer power for including more computationally intense OSA schemes. The lack of connection to overall climate model skill detracts from the overall scientific quality.

The science is presented in a clear and organized manner that allows for reproducibility. In the broad sense, the presentation quality is high. The paper is well organized and generally clear. However, the manuscript could be improved with additional attention to detail. The manuscript discusses a broad range of topics from details of ocean optics to atmospheric model specifics and results. Given inconsistencies and inaccuracies, primarily in the OSA parameterization development sections of the paper, it appears author expertise is in climate modeling and not ocean optics. In addition, there are some key references neglected in the work (indicated below).

Despite the criticisms above, OSA schemes in the existing suite of climate models do not reflect state-of-the-art knowledge of OSA physics (as the manuscript correctly indicates). A thorough evaluation of the sensitivity of climate models to OSA is not known to exist. It could be argued there is scientific merit to improving the representation of the underlying physics represented in climate models (this is not clearly done in the manuscript; one could also argue that hindcast skill is the only thing that matters). By presenting an improved OSA scheme, the authors are opening a door to further investigations of the sensitivity of climate model results to OSA.

Specific Comments (#'s 1, 2 and 3 are fairly significant): 1) Upper ocean models still largely utilize OSA values presented by Payne (1972; Albedo at the Sea Surface, JAS). Given the paucity of albedo observations, the authors are encouraged to evaluate their improved scheme in the context of the Payne (1972) values as well as the COVE station values considered in the manuscript. 2) The OSA scheme being presented is referred to as an "interactive" OSA. It's not clear exactly what "interactive" means or why that term is necessary. 3) There is a two-part series of papers published by Ohlmann and Siegel (2000; JPO) that parameterize OSA in terms of solar zenith angle, cloud forcing (i.e. direct vs diffuse light), and chlorophyll concentration. That scheme captures similar physics to the one presented in this manuscript with the exception of whitecaps and appears computationally more efficient than the scheme presented. Could it be adapted for use in climate models. If so, how does it compare? If not, why? 4) Abstract indicates "precise OSA calculation without penalizing the model elapsed time". However conclusion states a 2% increase in elapsed model run time. While 2% is small, it is technically a penalty to run time. 5) Line 57. Incorrect statement. Photosynthesis is not necessarily directly related to the amount of solar radiation. 6) Line 130. Should

be "direct and diffuse". 7) Line 167. The clear water absorption coefficient for lambda < 400 nm is set to zero "due to lack of available data". Smith and Baker (1981; Applied Optics) present data that are available but not considered. 8) Below water albedo (Line 229) is not a technically correct quantity given "surface" (OSA) albedo includes a contribution form this term. What is referred to as below water albedo is technically irradiance reflected back to the atmosphere by the ocean interior that contributes to OSA. Further (line 232) the manuscript states it has multiple reflections, but technically only a single reflection is necessary. 9) Line 234. It is stated that DOM "can influence radiative properties in the ocean" but the authors go on to neglect it in their parameterization. Neglecting DOM because chlorophyll has the largest impact is not justification. The authors should be clear as to why DOM is neglected (the corresponding signal in OSA is relatively small, parameterizations based on DOM don't exist). 10) A nomenclature is defined/utilized where a indicates the various OSA components. For the "surface" component the superscript s is used, for the "below water" component the superscript w is used. But then for the "whitecap" component, the wc is given as a subscript. Making wc a superscript would improve consistency with other terms. 11) Line 505 indicates "old OSA schemes are unable to capture seasonal variations as observed". Technically neither the new nor the old schemes capture the observed seasonal cycle exactly. The point is that the new scheme does a better job of capturing the seasonal cycle than the old scheme. The manuscript should clarify this. 12) Line 610 indicates a "better coupling between atmosphere and ocean components". Technically the study only demonstrates that the new OSA scheme enables an improved air-sea exchange of solar radiation. 13) The Larsen et al. (1972) reference in Figure 4 is missing from the reference list. It's possible that the Figure 4 label should be Larsen and Barkstrom (1977).

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