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Earth System Dynamics
Discussions

Interactive comment on "Biases in the albedo sensitivity to deforestation in CMIP5 models and their impacts on the associated historical Radiative Forcing" *by* Quentin Lejeune et al.

Ryan Bright

ryan.bright@nibio.no

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This is an interesting and timely study. Although its novelty aspects pertain to the surface albedo extraction methods, the authors convert albedo changes to instantaneous radiative forcings (RF) which are subsequently benchmarked to results of several climate modeling studies and to IPCC AR5 estimates. Since the RF quantification and associated benchmarking is made an integral part of the paper, I would encourage the authors to reflect on the uncertainty of their RF estimates, which are based on a very simple parameterization [i.e., Eq. (12) and Cherubini et al. (2012)] that does not account for the spatio-temporal variation in atmospheric optical properties affecting

transmittance of reflected solar radiation.

Bright & O'Halloran (2019), for instance, benchmarked the performance of Eq. (12) to four GCM-based kernels and found persistent positive biases (see f. ex. Figures 1 & 2) and a relative RMSE of about 20% globally. Bright & O'Halloran (2019) proposed a new simplified RF parameterization (see Eq. (17)) that substantially reduces RF "error" (rRMSE of about 6% globally) and made a gridded RF kernel product based on this parameterization freely available (archived here: https://doi.org/10.6073/pasta/d77b84b11be99ed4d5376d77fe0043d8). This product is based on the same underlying CERES v4 data that has been employed in this study and includes uncertainty layers.

I would therefore encourage the authors to either: a) Drop the RF quantification and benchmarking part altogether and keep the focus on the novel albedo methods and merits, or b) Provide a strong justification for using Eq. (12) in light of its uncertainty, or c) Adopt an alternate RF kernel/model that has lower uncertainty.

Bright & O'Halloran 2019:

https://www.geosci-model-dev.net/12/3975/2019/gmd-12-3975-2019.html

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