Biogeosciences
Discussions

Interactive comment on "Radiative forcing bias of surface albedo modifications linked to simulated forest cover changes at northern latitudes" *by* R. M. Bright et al.

Anonymous Referee #2

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general comments:

The article describes an evaluation of six albedo schemes, which are commonly employed in climate models. For three sites in Norway these albedo schemes are driven by meteorological data (snowfall, snow depth, temperature, wind speed) and measurements of vegetation structure (LAI, height, fcover) to compute the surface albedo of forests as well as non-forested area in the vicinity of the meteorological sites. The difference in albedo (forest - non-forest) is then translated by means of a radiative transfer model in a radiative forcing (RF) caused by land cover changes (i.e. deforestation). Both simulated results, albedo and RF, are then compared with surface albedo and RF derived from satellite based observations (MODIS). Additionally, a simple regression

model for surface albedo is introduced to further rate the performance of the albedo schemes.

The main scientific question in this context is, how much the albedo of snow covered surfaces is reduced by the presence of forests, i.e. the snow masking effect, and which albedo scheme is reproducing it best. This is an important task with respect to climate modeling as it determines the strength of the snow-albedo feedback in the taiga zone. I think that the approach taken in this study to assess the quality of albedo schemes by driving them with local data and comparing the result with observed albedo has a large potential to contribute to this task. Usually, climate models are evaluated by looking at the differences between their results and observation. That means that in the presence of snow a bias in the simulated albedo may be due to deviations in the modeled snow cover or an inaccurate representation of forest cover in the climate model or a deficient albedo scheme. It is hard to disentangle the single contributions to the overall error. So, it is hardly possible to rate an albedo scheme by this approach. By contrast, in this study the albedo schemes are not used embedded in the climate models but isolated from them and driven directly by observations, so that it is straight forward to rate their performance.

My main criticism to the current manuscript is that the approach of the study is not coherently explained. Also the advantage of this approach is not sufficiently pointed out. This should be done at the end of the introduction. To be honest, I read most of the main text thinking, that surface albedo values of climate model simulations have been evaluated. So, it should be clearly stated that only the albedo schemes employed in climate models are driven by observations to calculate the analysed albedo values. It should be also mentioned in the main text that the albedo schemes have been slightly modified for technical reasons and that they are not identical with the albedo computations done in the climate models (with a reference to the supplement for the details).

After clarification of this essential aspect of the manuscript I would appreciate the publication of this study.

specific comments:

(1) I would alter the title to: "Radiative forcing bias of simulated surface albedo changes linked to forest cover modifications at northern latitudes". Albedo is simulated in this study, not the forest cover changes.

(2) The second line of the abstract contributes to the confusion I already criticised in the general comments. Not whole land surface schemes are used in this study, but only isolated albedo schemes (commonly employed in climate models) have been used for the calculations.

(3) There should be a description added in section 2, how surface albedo was calculated technically with the six albedo schemes. For example, I guess, that surface albedo was calculated only once per day at noon for each scheme and that the daily cycle is not considered.

(4) I would appreciate a few comments, if other sites with similar measurements exist that would allow to extend the analysis to other regions. A few sites with summergreen forest or a few sites with a much drier and colder climate would be interesting.

(5) Concerning the RF presented in Fig. 2, I would only quote local RF values. The area average RF values scale with the cropland fraction (I think), which is of no interest to the reader (I guess). I would also change the area average RF value mentioned in the abstract to the local RF value.

technical corrections:

(1) I would add two additional subsection numbers in section 2. One for the paragraph about MODIS albedo (2.1) and one for the station/forest data (2.2). Just for a bit more consistent structure of the subsection numbering.

(2) p. 17340 l. 19 change "an order or" to "an order of"

(3) p. 17341 l. 6 change "covered in snow" to "covered with snow"

(4) p. 17341 l. 18 change "intermodal" instead "intermodel"

(5) p. 17341 l. 19 change "suspect it to be behind the differences" to "suspect it to be the reason for the differences"

(6) p. 17342 l. 7 change "discussion surrounding" to "discussion about"

(7) p. 17346 l. 4 change "sits" to "sites"

(8) p. 17352 l. 26 change "efforts to improved parameterizations" either to "efforts to improve parameterizations" or "efforts towards improved parameterizations"

(9) supplement, S.3.4, change "from temperature and wind in are adapted" to "from temperature and wind are adapted"

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