Responses to Richard Essery

In discussing differences in surface albedo between forests and snow in open areas, Bright et al. present some interesting results on an important source of uncertainty for radiative forcings in climate models. The work will be worth publishing, but some improvements are required in the structure of the paper. This is an extremely compact paper with an enormous amount of supplementary material that is useful but does not compensate for deficiencies in the paper. Most of the problems can already be identified from statements in the abstract: - results "predicted by land surface schemes of six leading climate models" are not presented; the albedo parametrizations from these schemes are used in isolation, and more information is required on how these parametrizations are run without including them in full land surface models - the emphasis of the paper is unclear. The statements that "the magnitude and sign of the albedo biases varied considerably for forests" and "RF bias was considerably small across models" are contradictory; the models cannot all have small biases if there is a considerable range in their biases. - no justification is given for the statement that "model improvement efforts of recent years are leading to enhanced LULCC climate predictions"

The meteorological data available (page 17342) do not include all of the variables or temporal resolution that would be required for running the full land surface models. How the albedo parametrizations from these models are run with the available data is not adequately explained in either the paper or the supplement. What is meant by "forest structure" in terms of model parameters needs to be explained in the paper, not just the supplement.

We appreciate the constructive comments and critiques of Richard Essery. Many similar yet important comments regarding structure and clarity were also raised by Anonymous Reviewers 1 & 2 and have been addressed in our revision.

Our statements (in Abstract) that: '"the magnitude and sign of the albedo biases varied considerably for forests" and "RF bias was considerably small across models"' are, however, not necessarily contradictory since RF is a metric based on the difference between two albedos, which we had made explicit in both the Results and Discussion sections (for example, positive albedo biases of GISS were approximately equal in magnitude for both Forests and Open areas, which went undetected when taking the difference (Open – Forest) for the RF calculations.

Regarding the point that "the models cannot all have small biases if there is a considerable range in their biases", what we meant is that a large range was present across models for some sub-regions and time periods, yet this bias was not visible when averaging the results across all sub-regions and time periods. We have clarified this in our revised Abstract.

We delete the statement that "model improvement efforts of recent years are leading to enhanced LULCC climate predictions" in the Abstract as we agree that it is not justified.

The meteorological variables presented on page 17342 of our discussion paper are indeed not enough to run the full land models but are, in most cases, enough to execute the albedo parameterizations. In the limited cases in which they are insufficient we had noted this in the Supporting Information. However, we agree that this information is too important not to place in the main paper and have now make it clearly visible in the main paper (new section 2.2) that additional meteorological input variables are sometimes required, with new text elaborating how they have been obtained (computed) using the existing observational dataset and to which schemes the belong. Forest structural variables are now better described in the main paper .

Minor corrections:

page 17340, line 19 “an order of magnitude spread”

Corrected.

17340, 24 Insert Boisier et al. (2012) reference here for LUCID

OK.

17341, 18 “intermodal spread”

Corrected.

17345, 21 “r = 1” here looks like it refers to a correlation, but the correlation of what is not clear.

Changed to “1:1 line”

17345, 26 “positive biases occurred for the VIS band”

Revised as suggested.

17346, 4 “at Open sites”

Corrected.

17349, 14 Table S4?

Corrected.

17350, 29 If “CC%” is referred to in the paper it needs to be explained in the paper, not just in the supplement.

We have now defined it in the paper.

17351, 18 “on the underlying datasets”

Corrected.

17352, 25 Replace the hyphen with a comma.

OK.

Figure 1 caption The data shown in (a) – (d) are observed and modelled albedos, not correlations between them.

Corrected.

Figure 2 caption (a) shows albedo differences, not albedo changes. The two rank

scales and four NME scales on (b) are not explained in the caption.

Corrected.