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Interactive comment on "Developing a monthly albedo change radiative forcing kernel from satellite climatologies of Earth's shortwave radiation budget: CACK v1.0" *by* Ryan M. Bright and Thomas L. O'Halloran

Anonymous Referee #2

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- General comments

The manuscript presented by Bright and O'Halloran suggests the use of a new kernel (CACKv1.0) to derive radiative forcing at the top of the atmosphere from surface albedo changes. This kernel is derived by applying a machine learning technique to identify a formula which can best reproduce the results from kernels derived from Global Circulation Models, once it is applied to CERES satellite-derived data. The authors argue that compared to GCM-derived kernels, this new formula would 1) enable a more transparent derivation of radiative forcing from surface albedo changes, and 2) rely on data from

several years. Their analysis shows that the new formula performs better at mimicking the results from GCM-derived kernels compared to previously suggested formulations. They suggest the use of their results by the scientists studying the impacts of land-use and land-cover changes (LULCC) on climate to improve their calculations of radiative forcing from surface albedo changes.

Having an easily applicable kernel that reproduces the results from GCMs can indeed be useful for the LULCC community, and it that sense the authors' initiative is welcome and scientifically significant. Having said that, there are a couple of issues with the authors' approach, while the methodology could be better described to ensure reproducibility of the results. Overall, substantial work also needs to be done on the writing to improve understandability of the manuscript. These issues are not insurmountable, but I recommend that they are addressed before the manuscript is accepted.

- Specific comments

The real added value of CACK compared to previously suggested simple formulations can only be assessed in light of the uncertainties between GCM kernels. These thus need to be included at least in Figure 1 and discussed in the manuscript, so that the readers can assess for themselves how much of a difference using CACK rather than a simple isotropic kernel (for example) makes. The authors also mention that the GCM-derived kernels are based on single years of forcing data. This renders them uncertain and thus less appropriate as a benchmark, therefore the authors choose to use the multi-GCM mean kernel as a reference to partly alleviate the lack of consideration of interannual variability when they were derived. This seems reasonable but only partly alleviates the issue. In addition to being explicitly shown and discussed, the uncertainties about GCM-derived kernels (both related to model spread and interannual variability) need to be acknowledged in the Discussion.

Even in the current state, more conclusions could be drawn from Figure 1 by describing for example which kernels perform worst against the GCM-derived ones and potentially advancing reasons why this is the case.

The methodology should be more detailed to be able to understand how Equation 16 is derived. Which optimal structures and coefficients are considered during the symbolic regression? What should make the reader think that this approach doesn't miss potentially relevant formulas? And which "boundary fluxes (or system parameters derived from these fluxes) that minimized the sum of squared residuals..." were considered? This information should at least be provided in the Supplementary Material.

Given that the GCMs and the CERES data are not available at the same resolution, some kind of regridding must have been conducted. Some regridding methods make more sense than others, therefore it would be useful to have some more information on this aspect.

It is also not so clear from the current manuscript why certain choices were made regarding the GCM and kernel selections. Why are four GCM kernels included in the study, are these the only ones available? Is there some information existing on the quality of these kernels that guided the selection? Could the authors justify why they "emulated" the kernels of just two GCMs in a second step? It seems like only the 3 kernels performing best against the GCM-derived ones were retained for further analysis, but this is also not explicitly mentioned.

The structure of the manuscript could be improved to facilitate understandability. For example, why not mentioning the isotropic and anisotropic kernels, as well as the kernel from Qu and Hall in Section 2 already. Currently, at first it may read like they have been derived by the authors. The names of the studies that introduced other types of statistical kernels could also be added in the subsection titles to help the reader follow. The description of the CERES dataset also seems misplaced in Section 2. Additionally, in some occurrences the subsection numbering is wrong and the placeholders for Figures or Tables misplaced.

Last but not least, the CACK dataset is only mentioned in the conclusion, although from

the title it sounds like an important output of the study. If this is the case, it would need to be introduced in the abstract and the introduction of the manuscript. But ultimately, one may wonder whether describing CACK as a dataset is appropriate. Could the authors maybe develop on what makes it more than just applying Eq. 16 to CERES data, for example in terms of pre-processing or perspectives for updates, etc.?

- Technical comments

l. 68: "An additional downside is the that". Check typo

l. 157: to facilitate understandability it could be good to repeat the downsides of GCM-derived kernels here

l. 211 and 230: isn't the subscript "SFC" missing in the left-hand terms?

l. 425: "course" should read "coarse"

l. 704-705: can the authors make clearer what is meant by "100X100 sample grid"?

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