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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "CO_2-equivalence metrics for surface albedo change based on the radiative forcing concept: A critical review" *by* Ryan M. Bright and Marianne T. Lund

Anonymous Referee #2

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I found this review informative, and is a useful evaluation of options for CO2-equivalence methods for albedo change. Overall, as a review paper it naturally contains a fair amount of information. On the whole, it presents the information clearly, with a good structure and it is therefore straightforward to follow. I think it will be a useful paper to inform any potential future developments in how albedo change is evaluated in comparison to other changes to radiative forcing.

The paper highlights that there are difficulties with producing an equivalence between two fundamentally different forcings (a CO2 emission and a change to albedo), but that methods do exist, albeit with various drawbacks. It appears to me that scientific

accuracy is sacrificed for simplicity in the use of scalar metrics. The question for me is under what circumstances that renders the scalar metric useless. An example is shown which does this (e.g. around line 330, the choice of AF can lead you to different conclusions. Then the example in fig 6 also shows EESF is inappropriate.). Does this mean that EESF is inappropriate to use more generally, as the scientific representation is generally inaccurate? It seems to me that would be the case, and the only real benefit is because of simplicity/the status quo. I think a clear summary statement of whether you assess the scalar metrics to have any scientific benefit (when there are no technical hurdles to overcome, as there are for use by non-specialists). Or, the alternative is that the only benefit of a scalar metric is that it's easy to use for a non-specialist (which would imply to me that it's probably even easier to misuse. . .)

In addition to the methods you assess, I think GWP* would be worth assessing (or at least mentioning, if it's really not possible to assess). Lee et al (2021) evaluate the short-lived effects of aircraft, calculating CO2 equivalence to changes in RF using GWP*. The equation used in Lee et al is based on equations in Cain et al (2019) and Allen et al (2018). I think this would be highly relevant to your review paper, and I think it would be useful to evaluate where this method sits compared to the other methods, although I recognise it has only recently been published with respect to a radiative forcing. I think it ought to certainly be mentioned, and ideally discussed on its merits. Eg I think it should overcome the time dependency issues you have identified in other metrics.

CO2-forcing equivalent emissions (eg shown in Allen et al 2018, Jenkins et al 2018, Wigley 1998) are also another way to compare different RFs with one another, which may be worth considering/mentioning (although as this 'metric' uses a model, it's perhaps not strictly a metric).

I think the finding that the equivalence calculated using EESF is highly sensitive to AF is important, eg around line 330 you make this point with regard to policy/decision making, and your comments around line 500. That could be brought out in the abstract as it highlights a key challenge for using this metric for albedo change as standard practice. This suggests to me that the other metrics are theoretically better suited for more general use (if not practically).

Specific comments

Line 50: worth pointing out that CO2 persists in the climate system for 100s to 1000s of years. If it was short-lived then the relationship would be different so I think it's important to mention this here, as it's why the CO2 emission is not reversible.

Line 78: 'The climate may respond differently to different perturbation types despite similar RF magnitudes, as feedbacks are not independent of the perturbation type' – do the two clauses in this sentence mean the same thing? If there is, remove the repetition. If there is a subtle difference, please make it explicit.

Fig 3a: alpha old line is too hard to see. The B) doesn't appear on panel B and D) is misplaced too.

Line 294: Can you explicitly explain why?

Fig 6 is very hard to follow. Can you make the explanation clearer and legend clearer? I am not sure if you discuss the red lines in fig 6a? Are the red lines simply the cumulative of the blue lines? If so, why is the red dashed line always +ve when the blue dashed line starts of -ve?

The example starting in line 400: you say that GWP is most appropriate here. However, if GWP works very well for GHGs like nitrous oxide, but not very well for albedo change and is subject to discrepancies that vary over time for albedo, then is it really appropriate? GWP works well for long lives gases as an equivalence metric for CO2. However where the impact varies over a shorter time period than CO2 (eg an albedo change scenario) then although the use of GWP could in some ways be seen as consistent, it is in some ways simply applying a metric that works for long lived gases to other forcings which are poorly suited. I can see GWP is useful because people already

use it. It doesn't really mean it's scientifically suitable. In summary, I'd suggest that for ease of use, GWP might be suitable here, but I believe that scientifically it will still be less suitable than TDEE. If that is correct, then I think it would be a useful distinction to make here.

Line 410-12: I think that the GWP* approach in Lee et al mentioned above could be mentioned here as well as the discussion, if you are unable to bring it in to the metrics analysed in the main part of the paper.

Line 503: Does the requirement of the use of a scalar metric defeat the purpose of using a metric for comparison for policy making /decision making? If you would make a different decision using a scalar and a vector metric, why even use the scalar metric at all, when the scalar metric pushes you into a different decision? (I am not sure how often the scalar metric would push you into a different decision – perhaps something for future work)

Line 549: This implies that using a model is more uncertain than using a metric. As the metrics are based on models, I do not see how this can be the case. Suggest wording this more carefully so as not to imply metrics are free of the model uncertainty, whereas they are based on those same models with their inherent uncertainty.

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