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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 #1

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This article provides a review of the literature on climate metrics to evaluate land use change as CO2-equivalent emissions. There are 6 main methods developed over the last 20 years to evaluate the CO2-equivalence of land use changes, of which GWP and sigma-TDEE are the two most recommended by the authors (though both have their own drawbacks). The majority if not all national emissions inventories under the Paris Agreement only take into account CO2-equivalent emissions for a basket of WMGHGs, and clearly there is an omission in the non-GHG forcing. AR5 reviewed metrics for SLCFs in section 8.7 (in any case they are not widely used) and this article highlights the lack of consistency for metrics for surface albedo changes (which are also seldom used in pracitce). In any case, metrics can not yet describe with acceptable accuracy

the resulting climate impact of a particular future socioeconomic scenario (i.e. from an integrated assessment model; see Dension et al. 2019).

Main comment and recommendation

I am personally sceptical of the utility of metrics beyond well-mixed GHGs (and even then, with many problems) and I am not convinced that applying CO2-equilvance to land use changes is useful and robust. However, the authors are not necessarily claiming that it is, so the stated purpose of this paper - a review article on land-use metrics - is achieved. Additionally, practitioners in policymaking and life-cycle assessment applications, prone to using metrics without a full appreciation of where the numbers come from, are likely to continue to do so for the foreseeable future, so it is important that methods are critically reviewed and limitations communicated.

I would recommend more discussion as to the limitations of metrics generally and land-use change specifically. Conceptually, the problem for land use is similar to assigning metrics to SLCFs; like land use change, radiative forcing from SLCFs is spatially heterogenous and theoretically (if not practically) reversible over short time scales, and like land use change many SLCFs co-vary with CO2 emissions. Several groups are actively trying to improve metrics, e.g. GWP* (Allen et al 2018) and CGWP and CGTP (Collins et al. 2020). These adjusted metrics have a better link to final climate impacts (in terms of global mean surface temperature changes) and could include the effects of non-GHG forcers (through the CO2-forcing equilvant measure in Allen et al. 2018), though at the risk and expense of introducing additional complexity. Another way in which land use metrics are difficult to apply is that RF from land use change is small and uncertain (the authors do discuss this). Metrics for GHGs work well enough because the emissions to concentrations to radiative forcing (to temperature, in GTP-style metrics) relationships are well-defined, invertible (at least up until Etminan) and known within acceptable uncertainty.

The alternative to metrics is to use a simple climate model to determine impacts. The two most recommended methods in this article, sigma-TDEE and GWP, need estimates of the radiative forcing of land use change. Therefore, why not cut out the middle man: calculate RF from land use change, calculate or estimate the change in land-use related CO2 sources or sinks for afforestation/deforestation (if that is the intervention of interest, for a geoengineering-style experiment, the land-use CO2 does not need to be perturbed), and run these scenarios in a simple climate model that evaluates the emissions - concentrations - radiative forcing - temperature process, and determine the temperature difference between this and a reference scenario. Several candidate simple climate models are discussed in Nicholls et al. (2020a, 2020b). The required RF estimate can be obtained from the kernel method that the authors describe, or from something like Jones et al. (2015).

Line-by-line comments

- 13: equivalent
- 24: Earth (capital E)
- 26: the 13
- 30: "large scale carbon dioxide removal" - mention afforestation specifically here, and why
- 38: "backward looking measure" - explain what is meant here. RF is not necessarily backward looking, and several future projections exist (including for radiative forcing from land use changes)
- 45: UNFCCC

- 47: "debate about GWP as the metric of choice" - perhaps add a citation here. Almost anything from Myles Allen's group 2018-2020 would fit the bill, or Denison et al 2019
- 58: AR4
- 69-70: True up until AR5, where metrics were only done in rigorousness for WMGHGs and there was no evidence to suggest that ERF differed from RF. I would think that ERF would be preferred for metric calculations now, if it differed from RF. Of course we wait for AR6 for this to be defined, though might be worth a mention to future-proof this paper a little.
- 77-85: This paragraph implies why ERF is a better idea than RF; by including tropospheric and land surface adjustments, temperature responses track ERF more closely than RF, and reduces (perhaps elimiates) the need for efficacy scalings.
- 88: is it definitely regression?
- 90: this is outdated: use Etminan et al. 2016 or the re-fitted Etminan relationship in Meinshausen et al. 2020.
- 94-96: the dependency on background state of the reference gas highlights a weakness of metrics generally; more should be made of this.
- 99-101: the Etminan relationship has even modified the RF from 2xCO2 (3.80 W/m2 versus 3.71 in Myhre et al. 1998) but the point here is that a 1ppm change around present day doesn't affect the radiative efficiency of CO2 much, I guess.
- 107-108: g mol-1 is a simpler unit?
- 116: y(t) could be generalised and could represent individual carbon cycle models, as well as the multi-model mean as was used in Myhre et al. 2013

- 121-123: you could explain why this is important for metrics and why metric calculations based on AR5 values might be incorrect under different future assumptions (including for 1.5C targets, where the impulse response function between pre-industrial and present-day states are substantially different as shown by Millar et al. 2017)
- 139: Block and Mauritsen reference should be 2013, not 2015
- 153-154: To use eq. 5 don't you need climate model output? I suppose you could do this based on observations but it won't be globally complete and will still need a fair amount of data crunching. Maybe I've misunderstood something. table 3: Sciusco et al: should it be "on" rather than "no"?
- 177-179: here is another demonstrable weakness of attempting to use a CO2-eq metric for land use change. While AF can be assumed to be = 0.47 in the recent past, there's no guarantee this would hold in the future (Millar et al. (2017), and Jones et al (2013) on AF in RCPs).
- Figure 1: could now be updated to 2019 using the latest GCP update
- 191-192: "approximate time frame..." this half of the sentence doesn't explain what's going on here - does it mean that 47
- 238: is the minus sign meant to be there after TH? Section 3 subsectioning: it's rather top-heavy (most of the text under 3, with comparitavely little under 3.1, 3.2 and 3.3).
- Figure 3: subplot labelling needs a bit more care
- 271: a single rooftop? The associated RF change in fig. 3b suggests this is a very large perturbation. On second reading I understand this is a localised and not global mean RF, it would be good to just confirm this in the text somewhere.

- 287: again can now update to 2020
- 296: 100 years (add unit)
- 333-336: I think this example nicely highlights the inherent danger of using metrics for policymaking without due understanding of the methods and assocaited uncertainties behind boiling everything down to one CO2-eq number.
- 452: aff/def: spell out
- 456: Smith et al 2020 published version now has 17 models
- Section 7: as section 3 (top heavy)

References in this response not in manuscript already

- Allen et al. 2018 https://www.nature.com/articles/s41612-018-0026-8
- Collins et al. 2020 https://iopscience.iop.org/article/10.1088/1748-9326/ab6039
- Jones et al. 2015 https://link.springer.com/article/10.1007/s10584-015-1411-5
- Nicholls et al. 2020a https://gmd.copernicus.org/articles/13/5175/2020/gmd-13-5175-2020.htm
- Nicholls et al. 2020b https://www.essoar.org/doi/10.1002/essoar.10504793.1
- Denison et al. 2019 https://iopscience.iop.org/article/10.1088/1748-9326/ab4df4
- Meinshausen et al. 2020 https://gmd.copernicus.org/articles/13/3571/2020/
- Jones et al. 2013 https://journals.ametsoc.org/view/journals/clim/26/13/jcli-d-12-00554.1.xml

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