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Interactive comment on "The Climate Benefit of Carbon Sequestration" *by* Carlos A. Sierra et al.

Anonymous Referee #2

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Overall: I think this is an interesting paper, and lays a simple and elegant formalism for understanding the importance of changes to ecosystem carbon flows from the perspective of atmospheric radiative balance. However, the applications of the method are more confusing than helpful, and don't really sell the utility of the method for answering policy-relevant questions. So my overall suggestions here are major revisions along the lines of: (1) take some time in section 2 to explain a bit more what each of the terms here mean, (2) reformulate the subsequent examples to use more real-world numbers that describe specific sequestration activities: afforestation, deforestation, changed agricultural practices, etc, where the comparison is made between a perturbed and unperturbed ecosystem; (3) concentrate less on the long-term dynamics and more on the comparison of unperturbed vs perturbed ecosystem changes over discrete and policy-relevant time horizons.

Section 2.3. I think more detail needs to be given here for how this method works when

the linear and/or equilibrium assumptions are not justified.

Sections 3-4. I think some detail is needed on what exactly the proposed sequestration that is being modeled here is. It seems like the sequestration proposed here is to create a new ecosystem where none existed previously, so that x(t=0) = 0 and u is being changed from zero to some global-mean vlaue. But a typical sequestration plan is to afforest a given patch of ground, i.e. converting from grassland or crops to forest. How would such a transition, where the change is to both the u vector and B matrix when x(t=0) != 0, be calculated?

Line 245: I disagree with the authors here and think that if they are going to go this route, then they need to justify their decision much more than they do. Of course carbon does return to geologic reservoirs, but the timescale of this is much longer than the 10-1000 year timescale discussed here. So I think, if anything, the Joos et al curves should be used and the Lashof & Aruha ones removed. I'm honestly confused about why the authors would suggest the opposite.

Line 265: I disagree with the idea that changing u will, in general, not lead to a change in B. I think there is quite a bit of evidence (forest self-thinning, soil carbon saturation) that B is highly sensitive to changes in u in real ecosystems This comes up again a few paragraphs later. While it is mathematically convenient to separate these two things, I think in general it is not really possible to change without the other (nor a priori to assert what sign that change to u or B will necessarily be).

Line 293: This is a fairly obvious result and so I'm not sure why this formalism is needed to make that point?

Section 5. I am not sure I understand the point of this example, and I also think there is a conceptual error being made here when the method is applied to large (relative to the total biospheric fluxes) sequestration perturbations: as I understand the notation used here, the function h_a(t) represents the remaining pulse (positive or negative) of CO2 into the atmosphere. But much (∼50%) of the loss of that atmospheric concentration pulse is due to the beta effect of land carbon responding to the carbon that has been emitted. So it seems like you are double counting this biospheric response, as it appears in both h_a(t) and in r(t)? I suspect this whole approach only works for small perturbations to the biosphere, where h_a(t) and r(t) are approximately non-overlapping, and thus excludes this example here.

Table 1. Where are these numbers coming from? It seems like the authors are just sort of making them up as heuristic examples. Is that the case, and if so, might it be more useful to use numbers based on real-world, even if highly simplified, examples? Similarly, the u and B numbers from Emanuel et al (1980) are for a globally-averaged ecosystem? If so, I think this wouldn't make sense for this example and you would have to use examples for a specific forest ecosystem instead. I understand the intention here is to be heuristic but more realistic numbers shouldn't be too hard to track down and it'd be infomrative to try to do something that corresponds more to reality when talking about concrete examples such as this.

Line 395. Can they? I see how albedo could, but other surface energy terms or surface roughness imply a tradeoff of one or another type of energy, or redistributions of energy between the land and atmosphere, and so can't really be compared to this metric.

Section 7: I'm not totally sure how comparing the CBS metric of two extremely vintage ecosystem models (one of which is a global-mean number and the other is a sort of reference-temperature number, so not really comparable even) is really of any importance to the argument being made in this manuscript, or anything else really. If the point is just that soils store a lot of carbon for a long time, don't we already know that? Suggest substantially revising or deleting this section.

Line 476-484. The other (much larger, really) problem with GWP is that its utility completely depends on what time interval the metric is integrated over; hence the unending debates about how much policy should focus on CH4 as compared to CO2. This problem applies equally to the CBS, but is completely skipped over in this paper. How

would the CBS be used in a policy-relevant context where we care about limiting peak temperature at some time period? Are there sequestration methods that are positive at a 50-year time horizon but negative on shorter or longer timescales? Is there a CBS analogue for GWP-star? Exploring these question would seem to be central to how this metric would actually be used in practice, but isn't actually touched on at all in this manuscript. I think this is a mistake and a major shortcoming of the current manuscript.

Lines 501-509. These are really not trivial problems, and substantially degrade the utility of this metric. The criticism of the Joos et al model strikes me as wildly off base; the irreversibility of global warming on shorter than multi-millenial timescales is a core feature of the problem and so asserting it away as something that can be ignored is not a good idea. In principle, the uncertainty in the impulse response function would be the same if used for two separate treatments, i.e. a baseline and a perturbed ecosystem, thus it seems like the more useful application of this method would be as an analog to GWP (not AGWP): calculate CBS of both a directly-perturbed ecosystem and an unperturbed ecosystem (or relatively unperturbed, i.e. not logged or afforested or whatever the treatment is, but still subject to CO2 fertilization, changes in climate-driven mortality, etc), recognizing that, in a globally changed world, neither will likely be at steady state, and calculate a relative CBS as the ratio of the two absolute CBS.

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