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

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We appreciate the thoughtful comments from the Reviewer and the critical questions she/he poses. Based on this review, we decided to make a major restructuring of the manuscript. First, we decided to give more emphasis to the theoretical part of the manuscript and decrease the emphasis on the importance of the examples. Therefore, we show now very simple examples on the computation of the CBS, still using a simple model for clarity and transparency, and discussing potential applications in the Discussion section. Second, we use now a different model, slightly updated, focused on the ecosystem level and not on the biospheric level. It is still a very simple model, which has the advantage that it is tractable and the computations can be reproduced in a transparent way.

Below, we elaborate more on the proposed changes, and provide specific answers to

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the reviewer's comments (in *italics*).

Answers to general comments

- *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.*

  This is an important comment and we took it seriously. We acknowledge that the examples we provided are not necessarily very useful in explaining the main CBS concept and showing its relevance for policy-related questions. In the new revised version, the examples were completely changed, they now illustrate how these quantities are computed and what are the meaning of the obtained results. Direct applications to policy are addressed in the Discussion section. We realized that our original examples can be topics of importance to be addressed in separate studies, but the use of our simple model creates an imbalance of addressing an important topic with a very simple model. Therefore, we think those particular questions can be addressed in subsequent manuscripts that use more appropriate models for the questions being asked.

- *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*

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discrete and policy-relevant time horizons.

We made major revisions following these recommendations. In particular, 1) we expanded section 2 to better explain the terms of the equations and their potential use. Also, we made an effort to better explain the special cases of the framework and how they can be used for particular problems. 2) We changed considerably the examples to avoid misunderstandings in that we do not provide definite answers to particular questions. Rather, we use examples that show how the framework is used for particular *computations* that could be helpful for some particular problems addressed in the Discussion section.

Answers to specific comments

- *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*

  More details are giving here and also in section 2.4 on how to apply the concepts to the non-equilibrium case.

- *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 value. 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?*

  The analyses presented in sections 3 and 4 are steady-state analyses and not an afforestation case. The idea here is to show what is the climate benefit of an amount of carbon taken up by an ecosystem that is already in equilibrium. For example, it can be used to calculate carbon sequestration as an ecosystem

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service, say an old-growth tropical forest and how much warming is avoided over the long-term. We give now more details about this and explain cases in which these analyses might be justified.

- *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.*

  We agree with the reviewer and we are aware that this manuscript is not the best place to challenge the IRFs of Joos et al. (2013). However, it is still problematic to use this IRF for long-term and steady-state analyses. The decision of Joos et al. (2013) to introduce an intercept term with an infinity timescale in their IRFs is not well addressed in their manuscript. This is something not relevant for many analyses focused on policy-relevant timescales, but it is relevant for steady-state analyses as we showed in the previous version of the manuscript. More recently, Millar et al. (2017) addressed this issue by simply assigning a timescale of 1 million years to the proportion of carbon that in the IRFs of Joos et al. (2013) had an infinity timescale. We decided to follow the same approach as Millar et al. (2017), still using the same timescales of Joos et al. (2013), but avoiding entering in a discussion of appropriate timescales for geologically stabilized carbon. The IRF of Lashof and Ahuja (1990) was removed from the manuscript as suggested by the reviewer.

- *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*

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change without the other (nor a priori to assert what sign that change to u or B will necessarily be).

This is basically the difference between a linear and a nonlinear system, and this is why we make the distinction in our manuscript. In a linear system u and $\mathbf{B}$ are independent by the same definition of linearity. However, in many systems they are not independent as the reviewer points out, and this is a strong indication for nonlinear behavior. However, our intention in this section is to show how CS and CBS behave in the linear case, so when someone makes this assumption is aware of the consequences. We added text in this section making this point clear. We try to show that these results only apply to linear systems in equilibrium, but for more realistic systems this is not the case. We also want to point out that even though the assumptions may be unrealistic, they are still made in many different analyses and for this reason it is important to know what are the consequences of the linearity assumption.

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

  Yes, this is an obvious result, but there are policy relevant cases in which this is not so obvious. For example, in discussions of the 4 per mil initiative it is commonly assumed that inputs of carbon to soils can be achieved by increasing C inputs by a proportion of 0.004 of the current soil carbon stocks. However, there is no distinction between increasing C sequestration by management inputs versus management rates, or a combination of both. Therefore, we believe it is still important to clearly show that carbon sequestration can be maximized by managing both. We do not show here any formal optimization analysis, but the idea is that this framework can be used to better pose the maximization problem on formal mathematical grounds. We elaborate now better on this idea in the new version.

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- *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 (âĹij50%) 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.*

  The idea behind this example was simply to show that the CBS and the CS metrics can be computed for a time-dependent situation. The reviewer is correct in that for a large perturbation, there is potential for double counting because the atmospheric response already includes biospheric effects. For a correct computation of the time-dependent response of the atmosphere to large biospheric perturbations, a time-dependent response function $h_a(t_0, t - t_0)$ obtained directly from the particular simulation should be used. This function should exclude the effects of the biosphere and only include carbon removal from ocean sinks. However, since our aim is simply to show how to compute CBS for the time-dependent case, we decided to remove this example. We use now a different model that works at the ecosystem level and not at the biosphere level. With this model we show now how to compute the proposed metrics for systems out of equilibrium within the limit of a small perturbation, so we can still use a constant atmospheric response function.

- *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 sim-*

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plified, 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.

These are indeed heuristic values, only for showing the consequences of different types of biospheric carbon management. However, for the reasons mentioned earlier, we decided to remove this example.

- *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.*

  They cannot be compared directly, and we only claim that they are in 'units more comparable to those used to assess the overall effect of forest on climate'. The point is that values of CBS in units of W m^{-2} yr may be easier to relate to energy balance terms than GWP values, which are reported in CO_2-equivalents.

- *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.*

  This section/example was removed as recommended by the reviewer.

- *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*

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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.

Thanks for this suggestion. There has been a lot of debate on the time horizon for integration in GWPs, and this is indeed problematic for the case of emissions. But for the case of sequestration, it is an advantage to consider a finite time period for assessing sequestration. This is basically the problem of Permanence in the carbon accounting literature, where it is clear that sequestrations of carbon cannot be considered as permanent. To address this topic we decided to add an example with differences in integration time to show that different conclusions could be obtained by comparing systems at different integration times. We also added a section in the Discussion on this topic.

- *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*

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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.

Again, we reconsidered this point and believe this manuscript is not the right venue to challenge the IRFs of Joos et al. (2013), so we removed this text from the manuscript. Instead, we take the same simple approach of replacing the infinity time scale in Joos' IRF and replaced it by a 1 million year timescale as in Millar et al. (2017). This removes the mathematical problem of finding a limit to our integrals and has no practical consequence for policy relevant timescales. We do appreciate the suggestion of the Reviewer about computing a relative metric to compare CBS for two cases, e.g. a perturbed and unperturbed system. We did something very similar in the forestry examples in the previous version of the manuscript, but instead of computing a ratio we computed a difference between the two CBS values. In the new version of the manuscript, we added now a computation of the ratio of CBS between two systems, and added a discussion about how one could use this ratio for problems similar as in the use of GWPs.

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