General comments

Firstly, apologies for the delay in receiving my review.

In this paper, Avakumovic examines the impacts of assuming that there is a strictly linear relationship between cumulative emissions of CO2 and warming in climate economics. They show that this assumption can lead to errors in assumed warming. They continue on to derive a new equation that represents the link between cumulative emissions of CO2 and warming in a way that also includes state-dependence. They note that neither Green's formalism nor their new equation can capture scenario-dependence and leave a formulation that captures this dependence for future work.

I think the paper is interesting, identifies an issue for climate economics and then provides a way to solve it. I believe the author can revise it to make it a useful contribution to the literature. However, I am recommending major revisions because I think it currently misses key literature and is not presented in a way that will have maximum impact.

Major concerns

Missing key literature

The author claims that "While [state-dependence of the relationship between cumulative CO2 emissions and warming] was previously numerically detected, here we offer a way to quantify it explicitly in the form of a new, state-dependent carbon budget equation". This isn't actually true, Nicholls et al. (2020) derived an equation that provides a way to capture non-linearities between cumulative CO2 emissions and warming (i.e. non-linearities in the carbon budget). Arguably, the Nicholls et al. formulation improves on the formulation presented in equation 6 here in two key ways: a) it provides a clean connection with the TCR and TCRE parameters assessed by the IPCC (via its T_2x and C_2x parameters), something the author leaves for future work (see their comments in the discussion, "the analysis ought to be redone so that the derived equation also includes the climate sensitivity or transient climate response parameter") and b) it can represent dominance of both the temperature response saturation and carbon cycle weakening terms i.e. it can show a concave or convex relationship between T and cumulative CO2 emissions whereas the author's proposal is always concave.

Nicholls et al. also investigate deviations of the relationship between cumulative CO2 emissions and warming from linear. The author's claim, "…we assert that the extreme cases of maximally possible scenario-dependent carbon budget deviations are yet to be investigated and scrutinized", may be true because Nicholls et al. did not optimise to maximise deviations, but it is a much less broad claim than is currently presented in light of the work done by Nicholls et al.

Missing this paper (unfortunately) undermines the author's claims of complete novelty. Nonetheless, Nicholls et al. did not consider the application to climate economics or include the Green's formalism discussion so I think there is still space for this paper to present something new. However, it will likely require a significant restructure to focus more on the new parts and either compare their proposal to the results and equation of Nicholls et al. or simply use the Nicholls et al. parameterisation as their translation tool (removing their equation 6).

Writing

The paper reads like it hasn't been proof-read for basic errors (typos, missing words etc.) or overall structure and fluency. This is unfortunate, because it makes the paper's key points much harder work to understand than they could be. I appreciate that the author has already put a lot of work into this paper, but such changes are relatively simple to make and significantly improve the experience of the reader. A thorough proof and re-write to remove jarring language is needed.

The other big problem with the writing is that it is extremely slow in some patches. This makes the experience of reading the paper drag, which is unfortunate because there are lots of interesting insights so the experience should rather be one of learning and excitement. There are two obvious things that contribute to the slowness of the writing (one easier to fix, the other harder). In general though, I would ask the authors to consider how to make the writing punchier by removing areas of repetition and areas where they circle around the point they are making rather than getting straight to it.

The easy fix is making the language more active. Describing is slow. If you instead change your text so focus on what the point is, then things are generally easier to read. This can be achieved by making the sentence focus on the point, pointing to evidence in brackets e.g. "Scenario-dependence in FaIR is around 0.1C (line description, Figure X)." As another example, "That implies that the pulse response introduced in the previous subsection should also be a constant function. In Fig. 4, it is plotted in a dashed black line: the temperature response to an emission pulse has an immediate jump following the emissions, and it does not change in time, as the "perfect budget" implies." could be re-written as "That implies that the pulse response introduced in the previous subsection should also be a constant function (dashed black line in Figure 4)." In this section, there is also quite a lot of text spent on showing that the perfect budget can be represented by a heaviside function, which is a fairly trivial observation and not worthy of so much text I don't think (move it to supplementary if you really want to keep it). In general, I would reconsider whether the more trivial observations would instead be better in a supplementary or simply removed (for example the discussion of different ways one can reduce emissions and still have the same cumulative emissions in section 3.1.1).

The harder fix is how much background is included in the paper. This is tricky because the author is (arguably) writing for two audiences: climate economists and climate scientists. The problem is that what is boring, trivial background for one audience may be vital background for the other. As a result, it may be inevitable that some sections do feel slow to some parts of the audience. I'm not sure if there is a perfect solution to this, but I would invite the authors to reconsider which parts need to be in the main text. For example, as a climate scientist I suspect much of the discussion of FaIR's structure could be moved to the appendix (or even to a supplementary) because repeating the underlying paper's isn't needed. Another example is the intro, which includes a very slow historical account of the use of the carbon budget by the IPCC (yet still misses the use of the carbon budget in SR1.5, which was arguably a key point too). I don't think this historical account is really needed at the very start of the paper, and it could be significantly shortened in my opinion.

Key insights get lost

The above point is a shame because some really nice key points get lost. The analysis in Section 4.1 is very nice and provides a very nice insight into how FaIR (and perhaps the climate system) behave and what this means for approximations based on the simple linear carbon budget equation. However, this point can be lost because it does not stand out from everything else that is going on in the paper. I thought Section 4 was very nicely done and it should, arguably, be a much more key part of the paper. If other sections need to be shortened to achieve this, I don't think that would be a bad thing.

Journal fit

I hesitate to bring this up as I know how hard it is to find a journal that fits interdisciplinary papers like this one. However, the authors state their audience is climate economists. However, ESD is not a primarily climate economics journal so I do wonder whether they need to broaden their audience, re-write to fit ESD's primarily climate science audience or re-consider whether they are in the right spot. Discussion with the editor will likely clarify this (I may also have not well-understood the journal's intended audience).

Minor concerns

Clarity of methods presentation

The methods aren't always clearly presented. The references to the optimisation in the abstract are very hard to understand without having first read the paper. I would suggest removing discussion of the optimisation in the abstract as this is a methodological detail whereas the abstract here could simply focus on identifying the problem and the solution.

On this point, the open budget isn't clearly presented. A (supplementary) figure demonstrating the methodology would be very helpful. In particular, it wasn't clear to me whether you enforce that $E(t > t^*) = 0$ in the open budget cause or whether there can be emissions after t^* (which would seem odd, as then the cumulative emissions that the system sees are now changing from the specified limit up to t^*). Clarifying this may also help clarify the extent to which the author is blending the TCRE and ZEC concepts or not.

Introduction

The introduction is a bit slow and doesn't really lay out what the paper does clearly. Could it be re-formulated to a much simpler structure, e.g.

- the carbon budget concept is widely used
- for climate economics, a cheap tool that goes beyond simple linear would be very helpful to avoid some clear errors that come with the simple linear approximation
- climate economists can't always use simple climate models because of ...
- here I derive such a tool/use the equation of Nicholls et al. to see what the implications are/do both

Lack of exploration of climate uncertainty

The paper explores scenario- and state-dependence of the relationship between cumulative CO2 emissions and warming. However, the paper only runs FaIR in a default configuration so doesn't explore uncertainty in the climate response at all. This seems to be a shame: given that FaIR is a cheap model, the author could likely repeat the analysis to capture at least some of this uncertainty. That could be nicely explained through your impulse response view as changing the shape (both in magnitude of response and time-evolution) of the pulse response, helping the reader understand these differing climate system responses in a single framework. Such analysis could also lead to a very nice 'final product', specifically a set of parameters that could be fed into an updated carbon budget equation (whether the author's equation 6 or equation 4 of Nicholls et al. or both). Different values of these parameters would represent the range of climate sensitivities and provide a simple look-up table for climate economists who wish to include an updated carbon equation in their own models and also explore the range of climate responses consistent with the IPCC's latest assessment.

I think addressing this point would also address reviewer 1's major concern as some of FaIR's parameter combinations will exhibit a much more linear response (although even reviewer 1's own figures arguably still show the weak non-linearity and Nicholls et al. discuss this conclusion in detail).

Validation of model and code

I note that there is no section on code availability or validation of the GAMS implementation of FaIR. Both of these seem pretty key if reviewers are going to be able to assess the validity and reproducibility of the results (particularly so given that a known software package of FaIR is not used).

Claims about FaIR being 'best'

There are many places in the paper where claims that FaIR is the best model for climate economics are made (whether by the author or quoting Dietz et al.). I was surprised by these for two reasons. Firstly, when I looked at Dietz et al. I could not find the conclusion that 'FaIR was best for climate economics' (if the authors can point me to this that would be great). Secondly, using Dietz et al. is an odd choice of reference given that the IPCC included an assessment of different simple climate models in their latest IPCC report. This assessment comes in Cross-Chapter Box 7.1 of AR6 WG1 and I would recommend the author reads that and then updates their assessment of simple climate models based on it. Alternately, the author could simply remove all assessment of which model is best because it isn't relevant to their paper. What matters for the author is whether the model they use can capture the scenario- and state-dependence they are interested in. Cross-Chapter Box 7.1 of IPCC AR6 WG1 shows that to be the case, as does the author's own analysis, so all that needs be said is "FaIR 2.0 is good enough for the analysis we are doing here and is easy to implement in GAMS, so we use it" and then move on.

On this point, I think Cross-Chapter Box 7.1 of AR6 WG1 shows that FaIR is 'good enough' for negative emissions at the level of analysis you're doing here. There are lots of uncertainties when it comes to negative emissions, but for the level of precision you're going for it would be fine to think about negative emissions too and just note that there is (like everything) uncertainty, but this provides a step forward for economic assessment that is better than nothing.

Full-fledged

The authors keep referring to FaIR as 'full-fledged' FaIR. Unless their is a 'half-fledged' version of FaIR floating around, the author can remove the phrase 'full-fledged' everywhere and simply refer to 'FaIR'. The phrase 'full-fledged' is almost damaging because it gives the impression that FaIR is on the level of an Earth System Model, which it most definitely isn't (FaIR is an emulator or simple climate model).

Lower cumulative emissions level

At the moment, the lowest cumulative emissions level (including history) is 1000 GtC. Would it be worth adding a lower level, more in line with 1.5C of warming in total (I assume adding such a level is fairly trivial and this provides an extra, arguably policy-relevant, level).

Leftover temperature

It would be good to see a plot of T_left. Is having to prescribe T_left a problem? It seems an odd extra exogenous input to require. Could you just run the Green's model over the historical period too? This may have the benefits of adding historical data as a validation point and making it easier to understand exactly how the changing TCRE affects model behaviour?

Claims about time evolution of T_d

One of the claims made is that T_d declines to zero. However, I am not convinced that this is a general thing rather than just being a model feature of FaIR, which appears to always reaches the same equilibrium for the same cumulative emms? I don't think you need to expand your analysis to use other simple climate models (you are of course welcome to) but I think you need to be more careful with the conclusions you make about this time dependence as this may just be a model feature rather than a general property of the climate system.

In the same vein, the following comment at the end of the paper should be considered more carefully, "Furthermore, what is genuinely surprising is that we have managed to capture the whole model with seven compartments into a single equation without losing too much precision.". Given that the whole point of FaIR is centred on impulse response (the "IR" in the name stands for impulse response) it isn't that surprising that its sum behaviour (particularly its default response) can be captured reasonably well by a single well-tuned impulse function. I would caveat this statement more carefully and consider testing the fit under a scenario where emissions decrease sharply (currently you've only tested your simplified formula under 3 scenarios of increasing or stable emissions), acknowledge that you've only tested it with CO2 forcing (so you avoid any non-CO2, particularly aerosol-related, headaches) and consider testing the fit under different parameter values that might not be so amenable to being captured by a single impulse response function. The AR6 distribution of FaIR 1.6 could be (fairly simply) translated to FaIR 2 I believe (because their carbon cycle and temperature response are basically the same) if you want a sensible range of parameter values to explore. The probabilistic distribution of FaIR 2 from RCMIP Phase 2 would also work I suspect.

Claims about impact of optimisation year

Concluding that optimisation year doesn't matter from a sample size of two doesn't seem super robust to me. I think if you're going to make this claim, a more systematic exploration of the sensitivity of results to the chosen optimisation year is needed.

Why can't you just use a simple climate model

One thing that wasn't clear to me was why climate economists can't just use a simple climate model. Are there cases where even the simple climate model is too expensive to run or hard to implement? If yes, it would be great to have just one or two sentences in the introduction that help the reader understand why a new (or almost new) equation is needed and why existing solutions don't work.

Figures

Figure 6 is a nice illustration. Could you make a plot where the y-axis is relative deviation from FaIR (even as a supplementary plot)? This would help assess the fit more easily (a 0.1C deviation in a scenario with 4C of warming isn't the same as a 0.1C deviation in a scenario with 1.7C of warming).

Technical corrections

'We' should become 'I' throughout given you are a single author

The figures are not ordered in the order in which they appear in the text. This isn't necessarily a problem, but it is very unusual and quite distracting for the reader.

FAIR → FaIR throughout (most unfortunately, FAIR is actually a different model)

page 1, line 8: 'scenario choice under the fixed cumulative' → 'scenario choice under fixed cumulative'

page 2, line 38: footnote 2 should be brought into the main text. The text is misleading without this caveat being in the main text

page 3, line 76: 'the optimization program' → 'an optimization program'

page 3, line 84: 'Furthermore, Using' → 'Furthermore, using'

page 3, line 90: 'the state-dependent carbon budget equation' → 'a state-dependent carbon budget equation' or do you believe you have find the one and only way of doing this?

page 4, line 115: "correctly capture the temperature response following one carbon emission pulse", this seems to miss the point and forces you to define 'correct' (which you currently don't). Surely the point is that FaIR is used elsewhere and has an adequately complex temperature response to explore the scenario and state-dependence of interest (which it does). You don't need to claim 'correctness' (which is arguably impossible to validate).

page 6, line 154: 'To use Eq. (2), must opt', missing word

page 6, line 170: 'Thus, pulse' → 'Thus, the pulse'

page 6, line 172: missing bracket after 'Green1'

page 6, line 178: 'that that' → 'that the'

page 10, line 292: 'the associated historical cumulative emissions counting 584 GtC' → 'the associated historical cumulative emissions being 584 GtC'

page 10, line 296: 'Conversely, this is not a problem for the full-fledged model since that "leftover" response is fed into the initial conditions of the run.' Repetitive of previous paragraph, can be deleted page 11, line 308: ‘due excluding’ → ‘due to excluding’

page 12, line 350: ‘from from’ → ‘from’

page 13, line 360: ‘dependency.irstly’, something wrong

page 18, line 470: “one RCP runs’” → “one RCP run’s”

Figure 1 caption: ‘Max(T) in S1-G represents the minimal’ → ‘Max(T) in S1-G represents the maximum’

Figure 5 caption: “linear extrapolations” → “linear regressions” or do I misunderstand what I am looking at?

Heading sections: The headings seem to have random capitalisation: “Pulse Response as a Deviation Source” looks super weird, is this the journal’s style?

References

Nicholls et al. (2020): Z R J Nicholls et al 2020, Implications of non-linearities between cumulative CO2 emissions and CO2-induced warming for assessing the remaining carbon budget, Environ. Res. Lett. 15 074017. https://iopscience.iop.org/article/10.1088/1748-9326/ab83af

IPCC AR6 WG1 Chapter 7: Forster, P., T. Storelvmo, K. Armour, W. Collins, J.-L. Dufresne, D. Frame, D.J. Lunt, T. Mauritsen, M.D. Palmer, M. Watanabe, M. Wild, and H. Zhang, 2021: The Earth’s Energy Budget, Climate Feedbacks, and Climate Sensitivity. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 923–1054, doi:10.1017/9781009157896.009. https://www.ipcc.ch/report/ar6/wg1/downloads/report/IPCC_AR6_WGI_Chapter07.pdf