Dear Prof. Kirk-Davidoff,

Thank you for carefully reading the revised manuscript and for your helpful suggestions in view of further improving the manuscript. After thorough revision, I believe I have successfully addressed both reviewers' comments, as I will reflect on below.

The newly revised manuscript has mostly been built on the advice about clarifying the central point of the paper. As you suggested, the point is to demonstrate the usefulness of pulse response in the role of Green's function for diagnosing deviations from the carbon budget approach. The other two themes that Reviewer 2 candidates as possible central themes are much less salient, with the climate economics part (1st point) being only briefly mentioned, and the third point, which is generalizing the method of using pulse response for carbon budget deviations, brought in the paper as potential use, left for future work.

As per suggestion, Sect 3. and Sect 4. have now replaced the order of appearance with an optimization program in the role of validating the concepts introduced in Sect 3. However, I have kept the dual role of the optimization program, with the second role (besides Green's function validation) being the generation of maximally possible scenario-dependent deviations under given user-defined constraints. Although Reviewer 2 is not wrong that the scenario-independency stemming from plausible future emission scenarios has been tested, that is not the point of the optimization program. The optimization program tests the whole emission scenario space that could be considered possible. Even though it is not a large breakthrough compared to previous literature, it adds value to the literature by confirming scenario independence to a higher level. Additionally, as I suggested at the end of the discussion, the optimization program could be tested in other, more complex models for verification of previous results.

I have kept the one-box model pulse response representation because it explicates and confirms the pulse response view of carbon budget deviations, giving a counter-example to FaIR as a model whose pulse response suggests large and persistent deviations. Nevertheless, I have been more explicit this time in pointing out that the two models are not on par, and a lot of material in regard to one-box has been removed.

Finally, I have followed the reviewer's 2 advice in lines of cutting the text out / putting it into supplementary. The subsection "net-zero case" that was in Sect 3.2.1 (previous version) is now fully removed, while the section that introduces temperature leftover has been moved to the appendix. Supplementary material has been boosted with the optimization run detailed setup description.

I will refer to the rest in the point-by-point response, followed by this letter.

Once again, thank you for giving me an opportunity to revise the article again and for helpful guidance on how to do it the best way. I hope that the reviewers will find the revision in a good light, as I did my best to follow the given suggestions. The list of changes can be found after the point-by-point response.

Best regards,
Vito Avakumović

Pont-by-point response

Referee 1

Overall Assessment:
I would like to thank the reviewer for the very kind and encouraging words in regard to the revision of the original manuscript. I hope the new version meets the same level of quality.

General comments:

- The font size is increased in all figures
- Units have been changed as suggested. Also, GtC is now presented as PgC.

Specific comments:

- The specific comments have been addressed in the text and the figures have been fixed
- Answer to the comment "*Figure 5: Would be clearer if used same y-axis range for all subplot in top row*".
    - While it is a valid point in general, I would disagree in this specific context. I chose a different y-axis range such that the central value (the magnitude of the absolute temperature increase) would always be approximately in the middle of the y-axis. Since the magnitudes are different for every graph, the y-axis changes. If I chose the same y-axis range for all the graphs, the difference in generated temperatures between FaIR and Green's approach for each case of F_tot would not be in the foreground, which is the point of these graphs.

Referee 2

General comments:

I would like to extend my gratitude to the second reviewer for a thorough examination of the second manuscript and for very detailed advice on possible ways to improve it. I believe the addressed comments in the second review phase made the manuscript a better version of what it was. The newly revised manuscript has been designed to address the major and minor concerns, and I sincerely hope that the latest version is clearer to read and more on point.

Major concerns:

Key point of the paper

The ambiguity about the key point has been hopefully resolved in the newest revision. The main point of the paper is indeed the second point, i.e., showing how to understand both state-dependent and scenario-dependent deviations of the relationship between cumulative CO2 emissions and

temperature (the carbon budget approach) from the perspective of the pulse response representation (in the role of Green's function). Hence, as suggested by the reviewer, the sections' order of appearance has been rearranged. In the revised version, the paper first shows how to understand deviations through the lens of pulse response in Sect. 3, followed by the validation of the theory and quantification of the deviations in Sect 4.
Furthermore, as suggested, most of the climate economics (point one) had been cut out of the text, with some brief referencing in the context of the pulse response representation and climate models that are used in climate economic assessments.
The last, third point was not intended to be a central point of the paper and is less emphasized in the revised version. The 'robust conclusions' are reduced, and generalizing Green's approach to models of higher complexity is given as a suggestion for future research, instead of as a novelty claim of the paper.
Lastly, the issue of "*the paper is still quite slow*" was addressed. A lot of the text has been cut out (for example, the whole section that was 3.2.1 in the previous version does not appear in the new version anymore)

State-dependence of TCRE

The functional form TCRE = - a T + b was not chosen but derived empirically from the interpolating the points of the state-dependent pulse response (approximated as state-dependent TCRE), as shown in Fig 2b (revised ms). I have addressed the reviewer's concern in the main text, where I put the limits of the equation backed by the emission runs under which the equation had been tested. Specifically, "*It seems an odd idea to me to suggest that TCRE could to zero (or even negative)…*", this is not the case with this equation, or at least not within any reasonable values of temperature. Checking the values of the coefficients a and b, one can see that TCRE reaches 0 with the temperature around 15 K, and goes to negative for larger values.

Minor concerns:

Overblown conclusions

"*A key example is in the abstract (line 18-20)*". The critique is valid, and it has been properly addressed in the revised manuscript. The discussion addresses potentially using the Green's approach with more complex models but with caveats that the validity of the approach cannot be tested in the ESMs since they will never be able to be run in the optimization program, because of their size. However, as an outlook at the end of the discussion section, I added the potential of using it as a more complex than FaIR, but still a climate model of reduced complexity in the optimization run and checking its pulse response under different parameterizations. Using the more complex climate model, but still a relatively simple one would be a first step towards verifying the findings of this paper further.

Exploration of scenario space

"*The author says that their use of optimisation means they explore a greater amount of the scenario space than other papers. That's probably technically true, but I think it is a bit of a stretch to say that this is a really novel aspect. Nicholls et al. (2020) used all the SR1.5 CO2 pathways, which cover an already wide range of different rates of mitigation considered plausible*."
While I understand the reviewer's concern about overblown conclusion about the exploration of scenario space, I must point out that the reviewer himself states that it is technically true that the

optimization program gives means of exploring a greater amount of scenario space than other papers, meaning that it is to some extent novelty. The itself is that a bigger set of emission scenarios have been tested (in fact it is full emission space under given constraints), and moreover, the paper suggests a method for future research and different models to test the scenario-dependent deviations in a form of the optimization run, which could be useful for the community. Furthermore, I never argued in the paper that the diagnosed scenario-dependent deviations stem from plausible emission scenarios, but only that the optimization program tests the extreme possible cases under the given user-defined constraints. The fact that the optimization program tests possible, not necessarily plausible emission scenarios, is now explicitly stated in the revised manuscript.

Moreover,
"I think it should be noted that the author also applies (arguably arbitrary) contraints on emissions in their optimisation. I think this undermines lines 95-96 "Through the optimization scheme, the full portfolio of emission pathways is tested.". "
Again, the constraints are arguably arbitrary and attempted to be justified in the section that discusses the boundary conditions, which is now moved to supplementary material. To emphasize this point, I have emphasized throughout the text that the portfolio of emission pathways is constrained by user-defined constraints.

"*I think it is important to keep [the optimisation], since it is the key point that differs how the scenario-dependent effects are examined in the manuscript, compared to the previous literature". As I've said above, I don't think this element is that novel or key in terms of making this 3manuscript stand out (and as I've said further above, it wasn't obvious to me that this was the key difference/point of this paper*)."
I can only reiterate what I tried to convey in the points above (and in the letter to editor), and also the reply that I given in the first round of the discussions. However, the whole paper's focus is shifted away from the optimization program, and the optimization is now used foremost as a validation tool in a revised manuscript, so I hope that the compromise is found. I could technically remove it completely, but I still think it is a valuable result, as a tool to test maximally possible (not plausible) scenario-dependent carbon budget deviations under the given user-defined constraints.

Freely Evolving case

Thank you for pointing out this weak explanation; I was not fully aware of it myself. Indeed, the rest of the boundary conditions stay the same as only the boundary condition in the optimization year changes. This, however, drastically affects how the deviations behave since, because of the slope restrictions, the pathway in the net-zero approach must start declining ahead of time to reach 0, regardless of whether we minimize or maximize. This makes the minimization and maximization pathway more similar in net-zero, than the transient budget case.
Nevertheless, the comparison between net-zero and transient budget case have a much smaller role in the revised paper, as it focuses on the investigation of the pulse response instead. Hence, the distinction between net-zero and transient budget case is described in more detail in the supplement. In the main text, they are distinguished in one sentence with the one characteristic that differs between them, i.e., the condition of no emission in the optimization year for the net-zero case.

T_left

Once again, I can only commend and thank the reviewer for careful scrutiny of the paper. As the article is no longer centered around the optimization scheme, the whole section that was describing the optimization scheme (including a misplaced T_left discussion) has now been removed from the main text. As such, T_left found its place in the appendix.

Dependence on optimization year

Both F and t* are varied in the revised manuscript.

Technical corrections:

- All of the suggestions in this section have been incorporated in the text. Some of the points are already covered in the discussion above.
- "*I will just note that it is possible to run FaIR concentration-driven in Python, you just have to do a bit of digging to find the configuration*."
    - This is probably true since I never asked the authors directly for a FaIR concentration-driven
    - I guess the reviewer meant the version that can be found on the following link - https://github.com/OMSNetZero/FAIR/blob/master/src/fair/gas_cycle/inverse.py
    - However, the anecdotal reason why I haven't used it is the following:
        - The code generated at the link above was uploaded at the end of 2022, I started playing with the initial (in the meantime scratched) idea of this paper before at the beginning of 2022
        - Hence, I did it in GAMS, because it deals with changing the role of a parameter to a variable and vice versa rather easily as opposed to Python
- "*line 61-63…*" fixed! I emphasize in the text that the non-linearities are small in comparison to other uncertainties.
- "*line 73-74…*" By logarithm, I just meant a logarithmic functional form, which is the assumed functional form in Nicholls et al. (2020). I added a statement in the text that underlines that there is a multiplying factor that makes sure that the equation can take both concave and convex form (since stating that it is logarithmic can be indeed misleading).
- "*line 91-94*" blended.
- "*line 102-104*" discussed above
- "*lines 113-114*" This is not a main point of the paper, but it is of concern to a large part of the community that uses simple climate models in the climate economic field. Hence, I do not make it more obvious as a point of the paper (because the point of the paper is the pulse response discussed above), but I put it in a discussion section where I took the liberty to discuss the findings of the paper in a broader context. I admit that it is a subjective assessment to bring it up in the discussion section. I could easily remove this part, but I still consider it a very valid concern for climate economic discipline, worthy of mentioning.
- "*line 148-150*" Initially, I did not cite the authors since I used the parameters from the FaIRv2.0.0 paper (Leach et al. 2021), where I had trouble finding the citations. Nevertheless, I hope this is now fixed as I dug in and looked for the model description papers.
- "*Line 182*"/"*Line 185*" fixed
- TCREv2 is removed
- *"lines 192-193"* The procedure of acquiring the pulse response in the article follows the same procedure as the preceding literature that inspects pulse responses (albeit not to the same level of detail and not varying the climatic conditions as done in this paper), i.e., Joos et al. (2013) and Millar et al. (2017)
- "*line 279*" in the previous (second) version of manuscript, by this passage I meant that net-zero budget deviation is independent of ZEC because the ZEC effects subtract each other when subtracting T_max from T_min – the same way that scenario-dependent deviations (T_d's) are independent of T_left, as the reviewer already pointed out in his review
- "*line 352*" varied for both in the revised version

- "*Red line in ACCESS disappears*" fixed
- "*line 558*" Here, I meant that the pulse response experiments under different climatic conditions (referred to in the paper as the pulse representation, shown in Figs 1 and 3) were not (to my knowledge) done in the ESMs. So, it would be interesting to see to which extent the pulse response representation of FaIR corresponds to the ESMs pulse response to which it was calibrated.
  In other words, yes, FaIR was tested to reproduce CMIP6 behaviour in prescribed emission scenarios in Leach et al. (2021), but there is no explicit comparison between pulse responses under different climatic conditions.
- "*lines 617-619*" I removed these lines in the latest revision, as they referred to the lines from the introduction that talk about the carbon budget in the climate economic context (removed as well). A brief explanation of these lines: Namely, it refers to the decision-making framework used in climate economic calculations called "Cost-effectiveness analysis", where meeting a temperature target is ingrained in the optimization program in a sense that the climate-economic model finds the cheapest way to adhere to the climate target. As a consequence of mathematic formulation, the model will then try to stay as close to two degrees because sees it as a boundary condition. See Figure 5 in the supplementary material of (Neubersch et al. 2014)

List of changes

- All of the changes (except changing the y-axis in Fig 5, upper panels) suggested by the referees
- Changed the units from °C to K
- Adjusted the focus of the paper towards inspecting the pulse response dynamics and its implications on the deviations of the carbon budget approach
- Removed the carbon budget implications on and referring to climate economics from the introduction
- Changed the sections' order of appearance (Sect. 3 and 4. switched and modified)
- Moved the detailed description setup of the optimization run and the choice of boundary conditions in the supplement
- Removed the "net-zero" optimization scheme results section, Sect. 3.2.1.
    - Used net-zero results in the last section of the paper that discusses the transitory nature of the scenario-dependent deviations
- Moved the temperature leftover modification on the Green's approach section to the supplementary
- Added more figures (varying F_tot and t*) showing the optimization year independence in the supplementary
- Introduced the domain of applicability of TCRE(T) relationship
- Changed the Conclusion, so it does not refer to sections anymore, but to overall topic of the paper
- Rephrased the claim in the introduction about Nicholls et al. (2020) showing that linear equation leads to unrealistically low budgets and fixed the claim about their logarithmic relationship
- Fixed the "overblown conclusions" issue, while keeping the optimization scheme in a twofold role: primarily as a validation for the Green's approach, and secondary as a novel tool of inspecting the possible (not plausible) range of maximal possible scenario-dependent deviations under user-defined constraints
- Increased the font size on all of the figures

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