Dear Reviewer,

First of all, let me express my gratitude for the examination and assessment of the manuscript. The points given are very well received and I am happy to reflect on those in the following text. Some of the points I will gladly incorporate in the revised manuscript (in case be given the chance), while for some I think extra clarification is needed in order to provide context, and aid the reader in understanding the concepts provided in the manuscript. This way, I contest the prospect of paper rejection argued in the review. In this response, I first give a general context of the research and I reply to what I detected as the main criticisms of the manuscript afterwards.

As apparently, in part, the goal of the paper was misunderstood, at first I summarize the key innovations as I see them:

(1) Deviations from a linear relationship between cumulative carbon dioxide emissions and global mean temperature are separated into path dependencies and nonlinearities and hence discussed separately in a more explicit manner. From the perspective of climate economics, this is relevant, as the relation between different decision-analytic frameworks is governed only by the former effect. Hence, the accuracy of approximation must only be expressed through the former effect.
(2) When analyzing path dependency, I do not rely on a limited number of stylized scenarios but systematically determine the maximum possible deviations. For this, FAIR only serves as a demonstration case. In spite of its nonlinearity, the Greens function approach explains most of the found deviations.
(3) I present a formula as one way of dealing with nonlinearity. It might substitute models like FAIR in regimes where FAIR should not be operated – either for being too computationally costly (endogenous learning under uncertainty) or for exaggerating nonlinearity. For the latter, it should be calibrated from ESMs. Again, FAIR only serves as a demonstration case.
(4) I demonstrate that path dependency even further reduces over time by an order of magnitude after emissions have been stopped, forming a bridge between TCRE and ZEC.

Referring to the paragraph „Overall Assessment“:
The first sentence is correctly stating that the paper examines the deviations from linearity and path-dependence in the cumulative CO_2 – global temperature relationship with the FAIR model, with a caveat that the analytical approximation is one of the results, not a predefined tool.
Going into the second sentence, the goal of the paper is not to find a way to incorporate these effects into simple climate models used in economic assessments.
The goal of this paper is as follows. Firstly, we distinguish two different sources of deviations from the carbon budget as mentioned in the intro sentence, the (1) emission scenario (or path) dependence and the (2) climate state dependence manifested as a deviation from the above-mentioned linearity; both sources of deviations are treated independently. Accordingly, the goal of the paper is to quantify both deviations using FAIR.

Regarding the FAIR model & the climate economics perspective:
The usage of FAIR was critiqued in the review for various reasons. While I agree that the FAIR model is a simplified version of reality and lacks many natural processes, one should be aware that in climate economics the compromise between the ability to properly emulate the climate conditions (more precisely the GMT) in response to emissions on the side, and the computational costs on the other. Namely, the optimization procedures done in economic assessments require around 1 000 to 100 000 runs to find an optimal result, so the model needs to be as simple as possible, otherwise, the run would be never finished. In that sense, even MAGICC (Meinshausen et al. 2011) is considered computationally expensive and

FAIR has been suggested as the best compromise (Dietz et al. 2021). I will come back to FAIR when addressing the specific comments later in this document.
Furthermore, to contextualize the research, the research was partially titled „Climate Economics Perspective". Hence, I do not claim that the findings here reflect the real state of climate (i.e. the missing carbonate chemistry) but the best estimate in view of climate economics. In the context of climate economics and simple climate models, I argue that by inspecting the temperature response following an emission pulse (in the paper used as Green's function), one can concur to what extent a simple climate model adheres to a path-independent carbon budget. This gives extra weight to the paper, in addition to the two main goals of the paper.

With this stated, I would like to move to the main point of the response, referring to the general points raised in the review; some of the specific points I am glad to leave for the revision phase.

The first general comment argues that the deviation from the linearity is only observed in simple climate models and EMICs, while at the same time, the CMIP5 models in Tokarska et al. 2016 (Figure 3) show the linearity. To this, I have two arguments.
In Tokarska et al. 2016, the CMIP5 models were forced with both 1% per year atmospheric concentration increase forcing (pure carbon dioxide emission forcing) and the extended RCP8.5 scenario (aerosols and other non-CO_2 gases). The review argues that Tokarska et al. 2016 clearly show the linearity between cumulative emissions and the temperature increase with solid lines. However, those are the results using RCP8.5 scenarios so they have other non-CO_2 effects included, whereas FAIR was forced only with CO_2 in my work. Additionally, it is questionable if we have a linear relationship in a solid line for BCC-CSM 1.1 (green) in the relevant domain (~2000 GtC). On the other hand, it is not clear from the figure if the relationship is linear (or non-concave) for the purely CO_2 experiment (dotted lines). To round up, I quote from the paper „*Figure 3a shows that the warming in the RCP 8.5-Ext simulations scaled by the ratio of CO2 to total forcing, for a given magnitude of cumulative emissions, is slightly higher than for the 1PCTCO2 simulations. One possible reason for this is the warming from non-CO2 greenhouse gases, which reduces the diagnosed cumulative emissions in the RCP 8.5-Ext simulations associated with the carbon-climate feedback*." A similar conclusion can be made by looking at the first figure that I attach below, which I refer to in my second argument. With that in mind, I come to my second argument.
Looking at the graph given in the summary for policymakers AR5 WG1 (IPCC, 2013) in the first figure below, one can detect a slightly concave relationship between cumulative emissions and temperature increase, for RCP scenarios and the stylized 1% per year concentration increase scenario, also conducted in CMIP5 models. The figure can be found below. I drew the green and white lines to visually emphasize the deviation from linearity. In my research, only CO_2 emissions are taken into account, so <u>the grey plume</u> in Figure SPM.10 is imperative in this case. I use the result from the old report since in the newest SPM WG1 (IPCC, 2021), the relationship between cumulative emissions and the temperature increase is shown only for SSP scenarios (Figure SPM 10.).

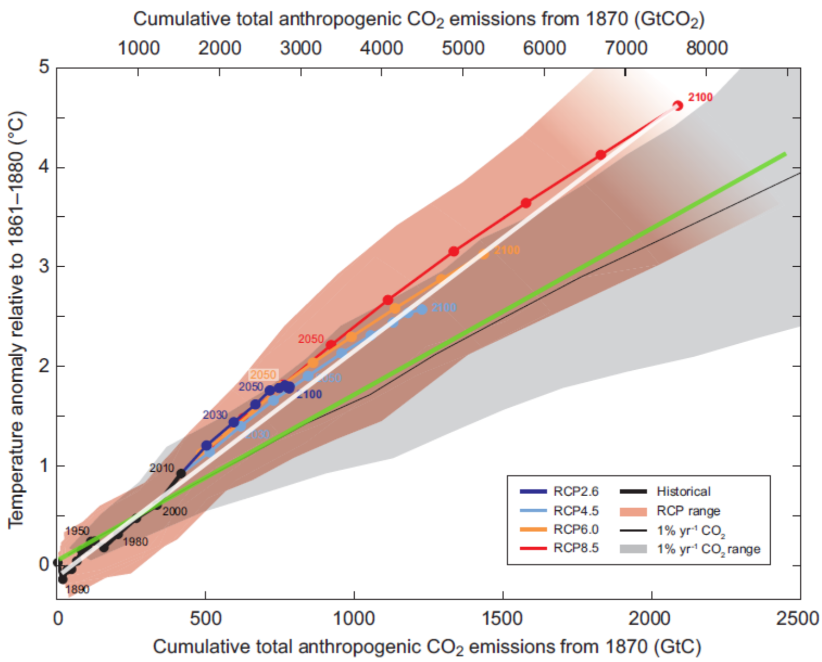

Figure SPM.10 | Global mean surface temperature increase as a function of cumulative total global CO_2 emissions from various lines of evidence. Multi-model results from a hierarchy of climate-carbon cycle models for each RCP until 2100 are shown with coloured lines and decadal means (dots). Some decadal means are labeled for clarity (e.g., 2050 indicating the decade 2040−2049). Model results over the historical period (1860 to 2010) are indicated in black. The coloured plume illustrates the multi-model spread over the four RCP scenarios and fades with the decreasing number of available models in RCP8.5. The multi-model mean and range simulated by CMIP5 models, forced by a CO_2 increase of 1% per year (1% yr^{-1} CO_2 simulations), is given by the thin black line and grey area. For a specific amount of cumulative CO_2 emissions, the 1% per year CO_2 simulations exhibit lower warming than those driven by RCPs, which include additional non-CO_2 forcings. Temperature values are given relative to the 1861−1880 base period, emissions relative to 1870. Decadal averages are connected by straight lines. For further technical details see the Technical Summary Supplementary Material. {Figure 12.45; TS TFE.8, Figure 1}

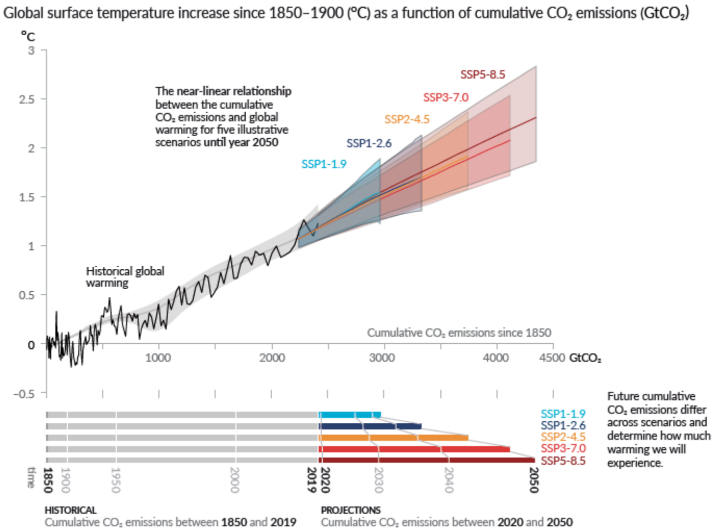

Figure SPM.10 | Near-linear relationship between cumulative CO_2 emissions and the increase in global surface temperature

Top panel: Historical data (thin black line) shows observed global surface temperature increase in °C since 1850–1900 as a function of historical cumulative carbon dioxide (CO_2) emissions in $GtCO_2$ from 1850 to 2019. The grey range with its central line shows a corresponding estimate of the historical human-caused surface warming (see Figure SPM.2). Coloured areas show the assessed *very likely* range of global surface temperature projections, and thick coloured central lines show the median estimate as a function of cumulative CO_2 emissions from 2020 until year 2050 for the set of illustrative scenarios (SSP1-1.9, SSP1-2.6, SSP2-4.5, SSP3-7.0, and SSP5-8.5; see Figure SPM.4). Projections use the cumulative CO_2 emissions of each respective scenario, and the projected global warming includes the contribution from all anthropogenic forcers. The relationship is illustrated over the domain of cumulative CO_2 emissions for which there is *high confidence* that the transient climate response to cumulative CO_2 emissions (TCRE) remains constant, and for the time period from 1850 to 2050 over which global CO_2 emissions remain net positive under all illustrative scenarios, as there is *limited evidence* supporting the quantitative application of TCRE to estimate temperature evolution under net negative CO_2 emissions.

Bottom panel: Historical and projected cumulative CO_2 emissions in $GtCO_2$ for the respective scenarios.

Additionally, the exponential relationship between cumulative emissions and temperature derived in the paper only slightly deviates from linearity in the cumulative emissions regime that is inspected. This was not shown in the paper not to overflow the reader with plots and information, but it can be tested with the values provided in the text.
Albeit the relationship is nearly linear, the small and persistent difference in temperature output can make a large difference in economic assessments.
Lastly, even if the FAIR model does produce a higher deviation from linearity than the ESMs, one of the points of deriving the new equation (Manuscript, Eq. 6) was to show that one can fairly well approximate a FAIR climate model with a single equation if the state-dependent TCRE is considered (derived from FAIR). This is a result independent of the linearity discussion.

On the second point, it was never argued that Green's function is an approximation of the real climate system, but rather an approximation of a model; in this case a FAIR model.
Furthermore, the point of the paper was not to qualitatively detect the source of deviation from the real-world (natural) system, but to quantify what are the maximally possible deviations given the FAIR model, at the same time testing whether FAIR (and its Green's approximation) can accurately capture path-independence. Hereby, I refer to the comments questioning both models' ability to capture the path independence. In my paper, the path dependence was tested by the optimization program given by Eq. 4. By using the optimization procedure, the full portfolio of all the possible emission pathways (under the given constraints) was tested, and not just „a simple series of idealized experiments using different rates of emissions“, as suggested in the review. Hence, using the optimisation program the pathways that generate the minimal and maximal temperature in a given year (y*) under the same cumulative emissions are generated. By subtracting those two, the maximal path-dependent deviation of the carbon budget is generated. Seeing it is small in magnitude, the conclusion is that the FAIR adheres to scenario independency of the carbon budget approach. Coming back to the Green's function that is derived from the FAIR model and running it through the same optimization program, it provides more or less same path-dependent deviations, confirming that it can accurately capture path-independence at least in context of FAIR model. Therefore, we use the shape of Green's function to provide the intuition behind the path-independency and ground the argumentation that it can be used to assess other simple climate models' adherence to carbon budget (explained above).

Regarding the third point, I am happy to show the validation of GAMS in the revision in case permitted.

Finally, I would like to comment on ZEC. While I was aware of ZEC, I did not emphasize in my paper that TCRE is instantaneous, while ZEC is a measure for the temperature response after cessation of emissions. This is mostly visible by the not-so-well-managed term „net-zero“ budget, which is equivalent to ZEC if we look at the temperature evolution in time further from the optimization year (Figure 2). The confusion and not precise usage of terms here come about due to the fact that this is an interdisciplinary paper, and to the best of my knowledge, ZEC is not yet established in the climate economics community. That does not, however, stop me from introducing it now and I would be very glad to have a chance to fix this in revision.

As a last comment, I would like to emphasize again that this work is an attempt to analyze the natural science of TCRE from a climate economics-optimized angle and also qualitatively aggregate and condense the findings for climate economic purposes, overall putting the work into a basket of interdisciplinary research.

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