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Interactive comment on "Constraints on long term warming in a climate mitigation scenario" *by* Benjamin Sanderson

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This paper introduces an interesting concept of how to account for the long-term effects (such as changing climate feedback parameter) in the carbon budgets framework. However, I found it challenging to understand this study in the context of applications to carbon budgets, and how these findings should be interpreted. I would recommend revising the framing of this paper to make it more relevant and easier to follow for readers familiar with the carbon budgets literature.

Many thanks to the reviewer for the extensive review - and I recognise (from both reviews) that the initial submission required better context in the existing carbon budgets

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literature. I have endeavoured to incorporate the suggestions and better frame the paper in the revised version.

The paper could also benefit from clarifications, and consistency with the most recent literature on carbon budgets and TCRE. In particular, the role of non-CO2 forcing on hysteresis of the effective TCRE curves discussed in the paper should be clearly separated from making claims on hysteresis in TCRE alone (which applies to CO2-induced warming only). I included severalsuggestions that potentially could help to clarify the points of confusion. Also, I would suggest putting the findings of this paper in the context of the overall uncertainties in carbon budgets (see IPCC Special Report, Table 2.2, Chapter 2, for a summary of different uncertainties), which I suppose are much larger than the uncertainties in carbon budgets due to changing climate feedbacks.

Point well taken - I have worked to make clear in the revised version that there is potential for hysteresis from a number of factors, of which this study only deeply considers one - with better reference to the SR15 conclusions.

Furthermore, I would suggest discussing the uncertainties in the observational datasets explicitly in the main text, and how they affect the results of constraining the simple model.

Agreed, I have structured the methods to be inline with the document - including the observational uncertainties.

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Major points

1. Model description

It is unclear what model is used in this study -is it a version of the FaIR model with additional components that would account for changing long-term feedbacks, or is it a simpler impulse-response model that contains less processes than FaIR? (the references are a bit vague).

Sorry about this - I've made the current version more clear. It's a Green's function solution of the core carbon-climate equations in FAIR - not the whole model (i.e. there's no complex chemistry - just a single bulk forcing term for aerosols.)

I would suggest to include basic description of the model in the main text (e.g. on lines 70-75) in the context of recent climate model emulators (or how it differs component-wise from FaIR) for the readers to have a brief idea of how the climate response is determined without the need of referring to the appendix.

The methods are now inline with the document, with an expanded description of the model

Lines 180-185: It is unclear how the emulator used here differs from the FaIR emulator? Is this an extension of FaIR that accounts for the possibility of changing feedbacks, or is it a simplified version of it.

This should be clearer now - it's a green's function implementation of the core dynamical assumptions which is fast enough to run MCMC calculations (I started out using FAIR itself, but the solver was too slow to run the number of iterations needed to calculate posterior distributions).

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2. Observational constraints from the historical period

It is unclear how the observational uncertainty both in the observed warming and in the estimates of cumulative CO2 emissions from the global carbon project affects the results.

Thanks for this point. I've made efforts to expand the discussion of observational uncertainty - and have updated the analysis to better consider these aspects.

The approach of Millar and Friedlingstein 2018 (MF18) is not possible here - given the MCMC optimization is computationally demanding so repeating the probabilistic assessment for each member of Cowtan and Way (CW hereon) observational ensemble would not be practical, nor would the results be particularly meaningful (an ensemble of posterior distributions).

Given this - I've attempted to ensure that observational uncertainties are appropriately considered in the model's parameter space. For the case of climate sensitivity parameters, showing that the range of 20th C warming is consistent with the CW spread. For emissions uncertainty - I've introduced a new parameter which introduces uncertainty into the emissions in a given year to account for land use emissions uncertainty. A prior on this parameter is now chosen such that distribution of cumulative emissions in 2016 is consistent with MF18.

I would suggest either illustrating it on Figure 1 or at least discussing the following points in the main text:

Figure 1: 'observed' cumulative CO2 emissions – Please include references to the observational datasets in the perhaps in the figure caption, and specify if they include the total CO2 emissions (from fossil fuels and land use change?)

Done - now total land use and fossil emissions from GCP 2019.

If so, the uncertainty on estimated CO2 land use change emissions in the historical period is quite large (even up to +/-50 percent for the annual E luc emissions), and it should be indicated on the figure or at least mentioned in the text and the figure caption. (e.g. see Table 5 from the recent Global Carbon Project 2019).

Thanks for this point. The revised manuscript allows for uncertainty in historical cumulative emissions by introducing a new parameter in the model, for which the prior is manually adjusted to replicate (very well) the distribution of historical emissions in MF18, see new Figure S3.

Figure 1: observed warming from HadCRUT4 – is it adjusted for the blending-masking effects? If not, it is not like-for-like comparison with the global (and complete coverage) climate models' output. In such case, at least a caveat in the figure caption and a short mention of this point would be useful. (e.g. see Cowtan et al. 2015; Richardson et al. 2016, 2018).

I've shifted to using the Cowtan/Way 2015 ensemble median (for claibration target) and ensemble to assess the sigma T parameter which conveys the degree to which we trust that data in the MCMC calibration

Uncertainties in the other observation-based quantities (heat content, paleo and RWF) should be discussed, as some of those inputs/constraints have narrower uncertainties, while others are a lot larger.

I've made clear for other constraints (heat, Paleo-ECS and RWF) that they are idealized - they illustrate what the effect would be on our confidence in the event that we knew that data.

Lines 70-75: Please discuss the uncertainty in the observational parameters that are used to constrain the model output. Also, perhaps include a figure showing the observation-based priors used.

The confidence in temperatures is now covered in the context of the discussion of the selection of sigma_T, and informed by the range of observed warming seen in the Cowtan-Way ensemble.

Do historical emissions include emissions from land use change? If so, the uncertainty on cumulative emissions is much larger than the uncertainty resulting from observed temperature.

Confidence in cumulative emissions is replicated from Millar/Friedlingstein 2018 - itself informed by uncertainty estimates in GCP2016. This is represented in the model with a scaling parameter on emissions, which is calibrated to represent this uncertainty.

Also, the discussion regarding constraints from the historical record could use the following reference and a short discussion:

Millar, R. J. Friedlingstein, P. The utility of the historical record for assessing the transient climate response to cumulative emissions.Phil. Trans. R. Soc. A 376, 20160449 (2018).

Well noted -this is now discussed in the introduction

References:

Cowtan, K. et al. Robust comparison of climate models with observations using blended land air and ocean sea surface temperatures. Geophysical Research Letters 42, 6526–6534 (2015).

Richardson, M., Cowtan, K., Hawkins, E. Stolpe, M. B. Reconciled climate response estimates from climate models and the energy budget of Earth. Nature Climate Change 6, 931 (2016).

Richardson, M., Cowtan, K. Millar, R. J. Global temperature definition affects achievement of long-term climate goals. Environ. Res. Lett. 13, 054004 (2018).

3. TCRE definition, non-CO2 forcing and the effective TCRE hysteresis

Please note that the definition of TCRE should be applied to CO2-induced warming alone. If calculating carbon budgets directly from RCP scenarios that are subject to CO2 and non-CO2 forcing, please refer to the Effective TCRE (Matthews et al. 2016). The current version of the manuscript confuses these two concepts, referring to TCRE even if non-CO2 forcing is present, making the arguments difficult to follow, since the effective TCRE, per definition, is not necessarily linear, due to the non-linearities arising from non-CO2 forcing. This should be clarified throughout the text.

Point well taken, and apologies for this confusion. I have made efforts to clarify the definitions throughout.

Based on earlier studies (e.g. MacDougall et al. 2015; Tokarska et al. 2019), I would expect that the apparent hysteresis behaviour depends on non-CO2 forcing scenario, and I am not convinced that observational constraints address this non-linearity.

I have now noted these papers - but my paper is exploring uncertainties which are not present in these studies. MacDougal 2015 considers only a single feedback timescale (i.e. constant sensitivity parameter) - therefore is omitting the major development considered here. Tokarska 2019 uses only a single model configuration in which thermal effects and carbon cycle nonlinearities cancel to produce a near constant TCRE - but the results are not generalised to all possible configurations of the model, Neither study is addressing the key issue here - whether historical temperatures can constrain the free parameters of model which allows for feedbacks on multiple timescales.

Furthermore, if considering TCRE to CO2-emissions alone (with no non-CO2 influence), TCRE would likely be fully reversible (no hysteresis)- e.g. see Figure 2a in MacDougall et al. 2015.

Figure S3 shows the response to CO2 emissions alone for each posterior parameter distribution. The effective TCRE is clearly different to the TCRE, as would be expected, but the hysteresis behaviour is not strongly influenced by the non-CO2 forcing - arising primarily from unresolved uncertainty on fraction of warming to date which is explained by slow timescale and fast timescale feedbacks.

Thus, regarding Figure 1, I would suggest discussing the effect on CO2-only response separately, as I suppose most of these non-linearities arises due to the specific nonCO2 emission scenarios, and is not necessarily an inherent property of TCRE alone. One way to address this issue would be to repeat the analysis using CO2-only simulations (according to RCP 2.6 scenarios), to illustrate if such hysteresis also arises in the absence of non-CO2 forcing.

Thanks for this suggestion - I have conducted the sensitivity study as suggested, which illustrates that although non-CO2 forcing assumptions do scale inferred TCRE, they do not play a strong role in hysteresis on a multi-century timescale (illustrated in supplemental figure S4). Scenarios with non-CO2 forcers set to zero show different apparent TCRE in RCP8.5 (as expected), but the hysteresis behaviour in RCP2.6 remains primarily a function of the choice of prior assumptions on the model behavior.

References:

MacDougall, A. H., Zickfeld, K., Knutti, R. Matthews, H. D. Sensitivity of carbon budgets to permafrost carbon feedbacks and non-CO2 forcings. Environ. Res. Lett. 10, 125003 (2015).

Tokarska, K. B., Zickfeld, K. Rogelj, J. Path independence of carbon budgets when meeting a stringent global mean temperature target after an overshoot. Earth's Future (2019).

Specific comments:

Lines 15-25: Please note that TCRE refers to CO2-only induced warming (originally defined in simulations where atmospheric CO2 concentrations increase at a rate of 1

Corrected as suggested

Lines 20-25: I found this sentence confusing and inaccurate: 'the range of TCRE values observed in Earth System Models (ESMs) can be used to infer model-based carbon budgets which are compatible with 1.5 and 2 degree Celsius targets of the Paris Agreement. . ." Is this referring to model-based TCRE that is used then in conjunction with other quantities (such as estimates of observed warming and future warming from

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non-CO_2 forcing, as in Rogelj et al. 2019a framework) to infer remaining carbon budgets? Or is this sentence referring to carbon budgets at 1.5 C and 2.0C directly inferred from ESM output, as in AR5, for example? (in that case, those budgets already account for CO_2 and non-CO_2 warming in RCP scenarios, for example), but those budgets are not calculated directly from TCRE.

Paragraph completely rewritten in light of the reviewer's comment.

Lines 115-125: Since CO_2 emissions follow different trajectories, but non-CO_2 forcing follows the RCP 2.6 trajectory for each section, it is unclear how is the role of non-CO_2 forcing distinct from CO_2-induced changes? TCRE is pathway independent for CO_2 emissions (both positive and negative), but if non-CO_2 emissions are evolving in time (according to the RCP 2.6 scenario), the effective TCRE (to CO_2 and non-CO_2 forcing) is scenario-dependent, and heavily depends on the chosen non-CO_2 scenario.

e.g. Reference: Mengis, N., Partanen, A.-I., Jalbert, J. Matthews, H. D. 1.5 âŮę C carbon budget dependent on carbon cycle uncertainty and future non-CO_2 forcing. Sci Rep 8, 5831 (2018).

I've added a paragraph at the end of section 2.1 to discuss non-CO_2 forcers. But the CO_2-only experiments in Figure S3 show fairly convincingly that this is not the major factor in explaining the possibility for hysteresis without the RWF prior.

4. Robustness of TCRE under negative emissions

Please note that there are several recent studies using climate models of different complexity, including comprehensive ESMs and EMICs, that should be cited on lines 30-40. Currently, the paper gives an impression that this topic has not been studied in depth, while quite the opposite is true. Some discussion of these more recent studies *would also be helpful on lines 25-40.*

Regarding Earth system response to negative emissions in ESMs and EMICs:

Ehlert, D. Zickfeld, K. Irreversible ocean thermal expansion under carbon dioxide removal. Earth System Dynamics 9, 197–210 (2018).

Tokarska, K. B. Zickfeld, K. The effectiveness of net negative carbon dioxide emissions in reversing anthropogenic climate change. Environ. Res. Lett. 10, 094013 (2015).

Thanks - these are added in a new dedicated paragraph in the introduction on EMICs

Jones, C. D. et al. Simulating the Earth system response to negative emissions. Environmental Research Letters 11, 095012 (2016).

Mentioned in a dedicated paragraph on ESMs

Regarding TCRE behaviour under negative emissions:

Zickfeld, K., MacDougall, A. H. Matthews, H. D. On the proportionality between global temperature change and cumulative CO2 emissions during periods of net negative CO 2 emissions. Environ. Res. Lett. 11, 055006 (2016).

Noted in the EMIC discussion

Tokarska, K. B., Zickfeld, K. Rogelj, J. Path independence of carbon budgets when meeting a stringent global mean temperature target after an overshoot. Earth's Future (2019).

Noted in the EMIC discussion

MacDougall, A. H., Zickfeld, K., Knutti, R. Matthews, H. D. Sensitivity of carbon budgets to permafrost carbon feedbacks and non-CO2 forcings. Environ. Res. Lett. 10, 125003 (2015).

Noted in the EMIC discussion

Lines 30-35: '[TCRE] robustness in complex models under large negative emissions is relatively unexplored' – There are at least several recent studies that look at ESM model responses under different amounts of negative emission scenarios, and reversibility of TCRE after an overshoot (see several examples above).

Noted in the ESM discussion

Lines 80-85 claim that the TCRE relationship is not robust under negative emissions. However, it is unclear what fraction of this hysteresis behaviour is due to non-CO2 forcing. In intermediate-complexity model (UVic ESM) TCRE is reversible under negative CO2 emissions. At least a discussion of this claim in the context of these two following studies would be helpful here.

This paragraph has been highly restructured, with a more extensive literature review. I now note in the EMIC paragraph in the introduction that the net negative emission cumulative emission behavior is well tested in the U.Vic model. It is also noteworthy that the vast majority of the literature on the matter is conditional on the structural assumptions in a single EMIC - with very few studies formally sampling uncertain parameters of the model in tests of reversibility.

Lines 105-110: Figure 1b is not discussed in the previous section. I find it unconvincing why the TCRE framework would not hold under negative emissions even if model output is constrained by temperature and cumulative CO2 emissions (see major point above regarding reversibility). Please explain more your claim, possibly process-wise). Also, this hysteresis in the effective TCRE shown here may arise due to time-dependent non-CO2 forcing. Please see my comments below and in the above section 3 regarding the separation of CO2 and non-CO2 effects on the reversibility of TCRE.

As noted in the previous section, in this setup, the non-CO2 forcers are not a significant factor in the potential for hysteresis (the dominant factor being whether a prior is assumed for RWF or long term ECS). The model with the RWF prior does show more hysteresis in the all-forcing RCP2.6 compared with the CO2-only RCP2.6 (Figure S4(c)), but the choice of prior is by far the dominant constraint on hysteresis-like behavior in the model (compare Figure S4(a) with S4(c))

Line 80: This paragraph suggests that TCRE relationship is not robust under negative emissions. However, TCRE (due to CO2 emissions alone, as originally defined) has shown to be reversible in overshoot scenarios with negative emissions, including RCP scenarios (see major point 3 above).

I would argue it has not been shown to be true in a general sense - it has been demonstrated, for the most part, to hold in the UVic model, with limited evidence on shorter timescales in ESMs, and for simple climate models making strong structural assumptions on feedback timescales.

The non-linearity probably arises due to time-varying non-CO2 forcing. This should be clarified here,

As for the point above - supplemental plot S3 shows that the potential for hysteresis arises from the timescale dynamics of the thermal response, not from non-CO2 forcing assumptions.

and please refer to effective TCRE, if non-CO2 forcing is included.

corrected

Also, this behaviour depends on non-CO2 forcing scenario, and I am not convinced that observational constraints address this

See response to major point 3 and figure S3

Non-linearity.

Lines 85 to 90: I would suggest discussing the effect on CO2-only response separately, as I suppose most of these non-linearities arises due to the specific non-CO2 emission scenarios, and is not necessarily an inherent property of TCRE alone.

See response to major point 3 and figure S3

Line 107: 'cumulative emissions framework is not guaranteed to hold under negative emissions' – This is a strong statement, which I am not convinced about. I would expect non-CO2 forcing in RCP 2.6 to be responsible for this hysteresis, and if considering RCP 2.6 CO2-only simulation, this hysteresis effect would be a lot smaller, if at all Present?

See response to major point 3 and figure S3

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5. Committed warming (after emissions reach net-zero):

The paper refers to peak warming occurring after the emissions reach net-zero. However, there is no discussion with the literature on the committed warming occurring after emissions are stopped, which is directly relevant to the carbon budgets framework. For example, a short mention in the introduction (e.g. lines 25-40) and a discussion of how this paper fits within earlier studies would be valuable.

Ehlert, D. Zickfeld, K. What determines the warming commitment after cessation of CO 2 emissions? Environ. Res. Lett. 12, 015002 (2017).

MacDougall, A. H. et al. Z. M. MacDougall, A.H, Frölicher, T.L., Jones, C.D., Rogelj,J., Matthews, H.D., Zickfeld K., Arora, V.K., Barrett, N.J., Brovkin, V., Burger, F.A., Eby, M., Eliseev, A.V., Mokhov, I.I., Hajima, T., Holden, P.B., Jeltsch-Thömmes,A., Séférian, R., Michou, M., Shaffer, G., Sokolov, A., Wiltshire, A., Ziehnand, T., Menviel, L. How much warming remains in the pipeline? A multi-model analysis of the CO2 zero emission commitment. (discussion paper/ in review). https://www.biogeosciencesdiscuss.net/bg-2019-492/

Thanks for these suggestions - the papers are now discussed in the introduction.

Also, regarding the parameter choices and how they influence TCRE – perhaps it would be valuable to discuss the results of this study in the context of earlier studies, for example: MacDougall, A. H., Swart, N. C. Knutti, R. The Uncertainty in the Transient Climate Response to Cumulative CO2 Emissions Arising from the Uncertainty in Physical Climate Parameters. J. Climate 30, 813–827 (2016).

Thanks - paper noted in introduction.

Lines 170-175: I would suggest also discussing the zero-emission commitment (see examples above), which suggests, that on average, ZEC is close to zero for CO2 emission pathways (in ESMs).

Noted - for decadal timescales - but the focus of this study is century timescales. The MacDougal paper itself shows a large diversity of response on century timescales (Figure 3a, https://www.biogeosciences-discuss.net/bg-2019-492/) , with only a subset of models having performed integrations long enough to assess the long term response.

Also, part of the difference between the threshold exceedance and avoidance budgets may be non-CO2 forcing, which is not part of the TCRE relationship (it would be part of the effective TCRE, which, however, is not expected to be linear due to non-CO2 influence). Please see above major comments regarding framing.

I've now noted this point in the discussion.

Line 60: Please note that recent studies suggest that the peak warming after emissions are zeroed is likely to be close to zero (see examples above)

Note that the focus of this study is on century timescales - where the MacDougal 2019 review shows a large diversity of sign and magnitude of post-cessation warming.

6. Long-term uncertainties in the context of overall carbon budget uncertainties

Lines 215-220: It would be good to put it in the context of other uncertainties on carbon budgets (see IPCC SR Ch2, Table 2.2). I would expect that other uncertainties such as permafrost carbon cycle feedbacks, model response to non-CO2 forcings, and nonCO2 forcing scenario uncertainty are still the dominant sources of uncertainness in the remaining carbon budgets.

Thanks for this suggestion. I've quantified the impact of the prior assumption uncertainty on 2100 budgets in the results section, added a new figure S12 to illustrate budgets as a function of time in each experiment. I've also added a discussion paragraph to consider these uncertainties in the context of other factors, as you suggest, tabulated in SR1.5 Table 2.2

Minor suggestions:

Title: I would suggest for the title to reflect more that the scope of this paper is also focusing on carbon budgets and TCRE framework

Fair point.

Title is now: The role of prior assumptions in carbon budget calculations

Abstract: The abstract gives the impression that the main source of uncertainties for near-term policy decision is future negative emissions capacity and the long-term response to climate forcers as the main sources of uncertainty in for the near-term policy decisions. However, carbon budgets and related net-zero emissions targets are subject to much larger transient uncertainty from the future non-CO2 forcing (at the time of 1.5C or target warming level), and climate models' response to non-CO2 forcing, which varies largely among models, contributing to a large spread in the remaining carbon budgets. (See IPCC Special Report, Chapter 2, Table 2.2. therein for quantification of different sources of uncertainties). While non-CO2 forcing is not the main scope of this paper, I would suggest revising this framing to avoid the misconception about the key sources of uncertainties in the remaining carbon budgets and near-term emission Targets.

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Thanks for this - and I agree, non-CO2 forcing uncertainty should be discussed in the abstract.

Abstract: 'definite cumulative emissions budget' -I would suggest following terminology from Rogelj et al. 2019a framework, for consistency with other studies. Please specify if that is referring to the total or remaining budget?

Done - abstract rewritten to support the Rogelj model (a paper which came out during the late stages of writing)

Lines 90-100: While it is an interesting discussion, it is unclear how it relates to the transient timescales shown on Figure 1. (For example, it would be interesting to see the emulator behaviour until year 3000, for example, to assess the effect of non-linearities discussed in this study).

Added Supplemental Figure 15 to show evolution up to year 3000 for all simulations.

Figure 2 c. I found this plot confusing, and it is unclear what the baselines are. Following the SR1.5 and Rogelj et al 2019b recommendations, I would suggest plotting only the warming since 2006-2016, and offset it (as in SR 1.5 Table 2.2), so that the 1.5C and 2.0C target levels are clearly readable,

Done. x- and y-axis now anomalies from 2010.

Are cumulative emissions since 2010 or since 2020? (the figure caption and x-axis labels are inconsistent or confusing).

Now 2010 throughout.

Similarly, I suggest using the present-day warming baseline (as in SR 1.5), for consistency, in the whisker plots.

Done.

Perhaps, to clarify the point of this figure, it would be also useful to show whisker plots for the remaining carbon budgets at the time when 1.5C and 2.0C target is reached for the first time (before the overshoot), which would help to illustrate the difference in the transient and long-term budgets.

Thanks for this suggestion. Done.

However, they are not expected to be the same due to the ongoing non-CO2 forcing contribution. This point would need to be clarified as well.

As noted in response to major point 3 - this is a factor, but a secondary one to the choice of prior on thermal response parameters.

Lines 130-135: I found this paragraph unclear and confusing. It seems to be comparing carbon budgets calculated from scenarios that non-CO2 forcing is constantly evolving over time, with carbon budget estimates directly inferred from TCRE, but how that latter estimate accounts for the future contribution from non-CO2 forcing?

I've removed the TCRE estimate comparison from the text and Figure 2d, now just citing the SR1.5.

Lines 135-155 and Figure 3: It is unclear how the budgets can be compared for the different time periods, given that they entail different levels of non-CO2 forcing that is *evolving in time in the simulations considered in this study. (i.e. since those budgets depend on the future non-CO2 forcing levels that differ, how can they be compared in a like-for-like manner?)*

The non-CO2 emissions are fixed here at RCP2.6 emission levels, but the forcing is allowed to vary as an uncertain parameter in the model configuration - and that uncertainty is represented in the vertical spread of the distribution of points in Figure 3. I've made efforts to make this clearer in the text.

Lines 200-205: Perhaps a brief discussion in the context of more recent literature would be interesting (e.g. see Rogelj et al. 2019)

Thanks - restructured such that the end of the discussion explicitly supports the Rogelj framing.

Lines 205-215: Please note that carbon budgets should be calculated from anthropogenic warming estimate (Rogelj et al. 2019b. Haustein et al. 2017), which is not subject to internal variability. Reference: Haustein, K. et al. A real-time Global Warming Index. Scientific Reports 7, 15417 (2017).

I disagree that the Haustein estimate is not subject to internal variability, for reasons I laid out in this realclimate piece:

http://www.realclimate.org/index.php/archives/2017/10/1-5oc-geophysically-impossible-or-not/

The thesis of which was that the anthropogenic warming estimate is itself particularly sensitive to temperatures in the last few years of the timeseries, and repeating the approach in a large ensemble produces a distribution of forced warming estimates varying by over 0.2K. As such, I'm reluctant to recommend a regression approach as the preferred means of assessing forced warming trends for carbon budgeting.

That said - in the context of the present paper, I agree that the regression reconstruction would be more skillful by mid-century - and have cited these references in this context.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2019-82, 2020.

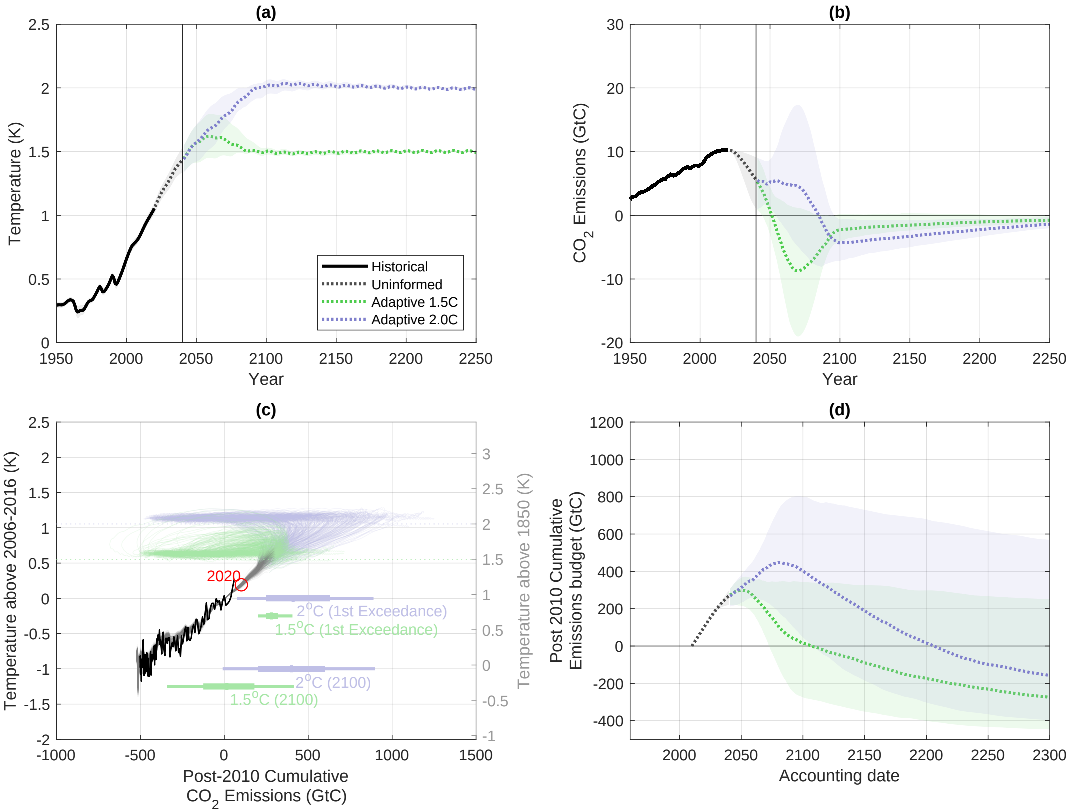

Fig. 1. Figure 2 Revised

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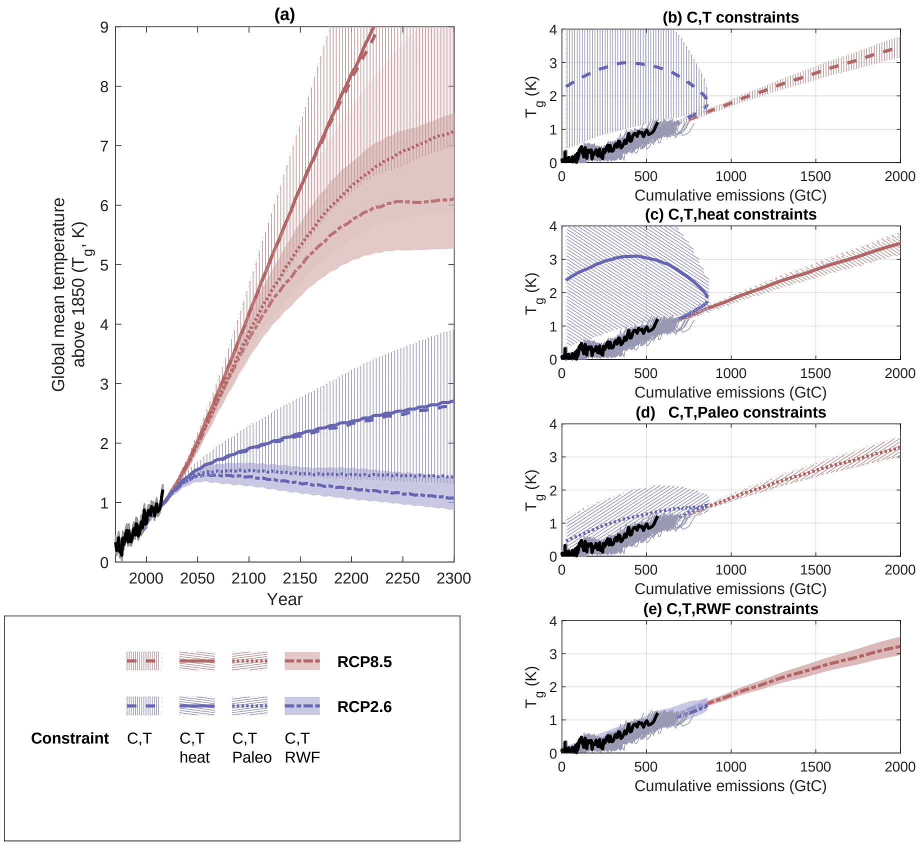

Fig. 2. Revised Figure 1

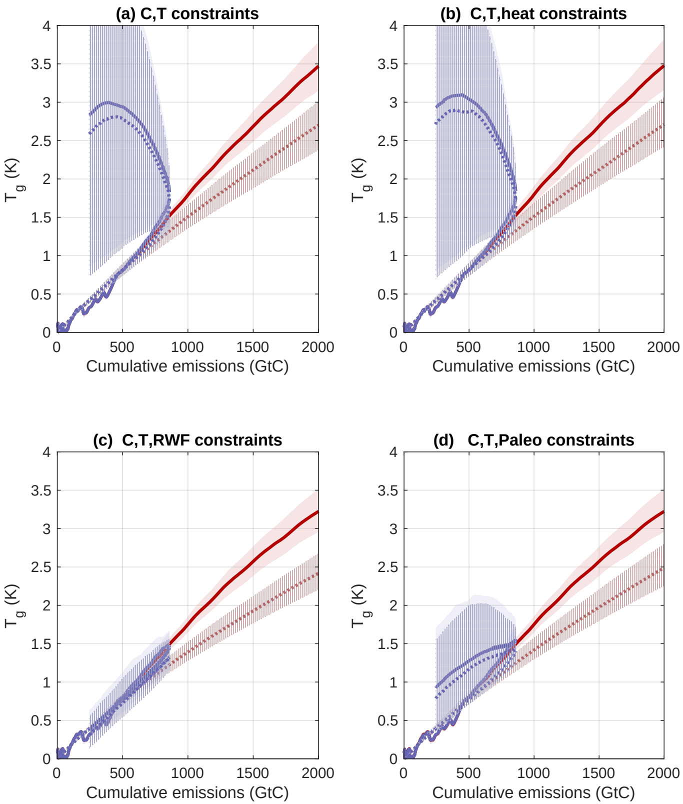

Fig. 3. Figure S3 - As for figure 1 b-e: dotted lines CO2 only, solid lines all forcing for each prior type.

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