Response to referee comments on "*Monte Carlo Drift Correction – Quantifying the Drift Uncertainty of Global Climate Models*"

Benjamin S. Grandey et al.

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Introduction

We thank the two referees for their constructive critique of our manuscript. In this document, we offer a response to each comment (quoted in *green italics*). The comments have enabled us to reconsider our assumptions, modify our methodology, and revise our analysis, enhancing the manuscript. Major methodological changes include (i) fitting each statistical model of drift to the entire control time series (not 150-yr segments), (ii) accounting for autocorrelation in the residuals, (iii) using branch-time metadata, and (iv) applying a third approach, "agnostic-method MCDC", which corresponds to an assumption that linear, quadratic, and cubic models of drift are equally plausible. Following these major methodological changes, we have substantially revised large sections of the manuscript, especially Sect. 3 (methods) and Sect. 4 (results).

Referee #1 (Damien Irving)

General comments

In general, I think this manuscript makes a valuable contribution to the literature. It introduces a concept - internal drift uncertainty arising from internal climate variability within model control simulations - that is typically overlooked by authors working with climate model variables that are prone to drift (i.e. those influenced by the deep ocean). The main result - that drift uncertainty can be relatively large in comparison to forced trends in historical simulations - is important and the authors put forward a useful method (Monte Carlo Drift Correction) for quantifying/checking the size of drift uncertainty.

Some other minor results are also interesting and well worth documenting:

1. The results the authors present regarding the how the fraction of excess energy absorbed by the ocean and expansion efficiency of heat behaves in control simulations before and after drift correction also adds a little to the existing literature on energy conservation in CMIP models (Hobbs et al 2016; Irving et al 2021).

2. The authors point out that it is preferable to integrate fluxes before correcting the trend (as opposed to correcting the bias before integrating the flux) which is something other papers do (e.g. Irving et al 2019; https://doi.org/10.1029/2019GL082015) but don't necessarily explain why they make that methodological choice.

We thank Dr Irving for his assessment and helpful comments, which have helped to enhance the manuscript. Our revised manuscript adopts an improved Monte Carlo Drift Correction methodology and also retains the two secondary results highlighted by Dr Irving (including interaction with the previous work of Hobbs et al., 2016; Irving et al., 2021, 2019). Regarding the relevance of Irving et al. (2019) to the order of integration and drift correction, we now write, “Researchers should clarify whether they correct drift before or after integration. Although we primarily refer to cumulative integration across time here, such clarity should also extend to other calculations: for example, Irving et al. (2019) clarify that they calculate spatial integrals before correcting drift” (Sect. 5.2 paragraph 2).

Specific comments

The authors acknowledge in the manuscript (line 415 and elsewhere) that a limitation of their study is that they ignore branch time metadata, which could be used to reduce uncertainty by allowing for higher order polynomials to be fitted. I agree with the authors that in some cases the branch time metadata is either not available or incorrect, but more often than not branch time metadata is available/correct and where it isn't it can usually be estimated. For instance, Irving et al (2019; https://doi.org/10.1029/2019GL082015) analyse an ensemble of CMIP models and say the following: "We obtained a drift estimate by fitting a cubic polynomial to the full control time series... The time period in the control simulation that parallels the forced simulation was then identified using the branch time information provided in the file metadata, so that the correct segment of the cubic polynomial could be subtracted from the forced simulation. For models with erroneous metadata, the branch time was estimated via visual inspection of the globally integrated OHC timeseries."

I strongly encourage the authors to follow the lead of Irving et al (2019) by attempting to verify model branch times by plotting a variable such as globally integrated OHC. Since essentially all models have a fairly large drift in globally integrated OHC, if you plot the control and forced experiment time series (using branch time information to line up the respective time axes) it's usually pretty easy to see if the first value of the forced experiment does in fact branch off the control experiment at the time the metadata says it does. If it doesn't, it's usually pretty easy to approximately esimate where the branch point actually is. Following this procedure I'd be surprised if there were many models for which the branch time couldn't be verified as correct or sufficiently estimated. This would allow the authors to overcome some of the main limitations of their study.

We accept Dr Irving's argument that we can use branch-time metadata to improve our approach. In our revised manuscript, we use the CMIP6 branch-time metadata, which are available for all the models we analyse. We also mention that Irving et al. (2019) proposed an alternative method to identify the branch time (Sect. 3.2 final paragraph). We now fit each statistical model of drift to the entire control time series, rather than 150-yr segments. Using the branch-time metadata, we also extend our MCDC methodology and analysis to non-linear models of drift. We apply a new “agnostic-method MCDC” approach, which corresponds to the assumption that linear, quadratic, and cubic models of drift are equally valid. Following these changes, our revised quantification of drift uncertainty is now more directly applicable to the linear, quadratic, and cubic drift correction approaches adopted in recent studies. Details of the revised methodology are provided in Sect. 3.2 of the revised manuscript.

Technical corrections

Irving et al "2020" is quoted throughout the paper but the actual publication year of that paper is 2021: https://doi.org/10.1175/JCLI-D-20-0281.1

We apologise for this error. We have now corrected the reference to Irving et al. (2021) throughout the manuscript. We have also checked the other references.

Referee #2 (Anonymous)

General comments

The authors build on previous papers that considered model drift in coupled climate models, and in particular the potential role of internal variability in affecting drift corrections. To that end they propose a Monte Carlo method of drift estimation, and compare drift estimates using time derivative/flux vs time-integrals/state variables. The paper is quite clearly written (although I found the paragraph structure - with almost each sentence being a separate paragraph - to be quite jarring), and the figures are mostly clear. However, there are some significant technical/methodological issues that the authors will need to either address or justify before i could recommend publication.

We thank the anonymous referee for their assessment and constructive critique, which has contributed to an improved manuscript. We note that the referee disliked the frequent occurrence of shorter paragraphs. When revising the manuscript, we have sought to consolidate the shorter paragraphs into longer paragraphs, whilst also seeking to develop one main idea per paragraph. We have addressed the methodological issues raised by the referee, as outlined below.

I have attached a marked-up PDF, but my significant comments are;

1) terminology - the authors need to be a little careful claiming that the climate models are not energy conserving, which isn't really true. There are errors in the models' energy budgets - which previous papers have termed 'leaks' - but because the models ARE energy-conserving these leaks are generally absorbed by the ocean.

We now use the term "energy leakage" instead of "energy non-conservation" throughout the manuscript (eleven occurrences).

2) Method - this is my biggest issue. The authors split a model control into 150-year segments and calculate the spurious trends (ie. drift) for each segment. That's an entirely appropriate way fo looking at century0scale internal variability. However, for thier Monte Carlo method they then create a parametric Gaussian distribution of the drift, based on the statistical error of each 150-year segment. That uncertainty is then included in their estimates of uncertainty in the overall drift, since they extrapolating that 150-year estimate (with statistical error) to the full 1100-year control run. This spuriously magnifies the actual uncertainty, because the statistical error in a trend calculated from the full 1100-year series would be much less than that from a 150-year series. Almost any reasonable analysis would use the whole control run, and not a 10% subset, for drift correct.

The referee correctly identifies that our previous use of 150-year segments "spuriously magnifies the uncertainty", assuming that drift is linear and/or the branch time is known. Hence our previous analysis should be interpreted as a worst-case scenario, corresponding to a situation in which drift is non-linear and the branch time is unknown. We now accept that reliable branch-time metadata are available. Hence, we have substantially revised our analysis. We no longer use 150-year segments. Instead, we fit each statistical model of drift to the entire control time series (Sect. 3.2 paragraph 1), as recommended by the referee.

3) Justification of Method - the use of 150-year estimates rather than the full control is not adequately justified. The only real justification that's given is related to issues with an unknown branch time in forced model experiments. That argument isn't relevent here where there's an a priori assumption that the drift is a constant, and it's also untrue that branch time is inherently unknowable, It is correct that in CMIP5 some published branch times were incorrect, but the correct ones were able to be inferred and indeed were publicly posted on the CMIP5 errata. I'm not aware of any such issue in CMIP6.

We thank the referee for constructively critiquing the assumptions underlying our original methodology. We accept the referee's argument that reliable branch-time metadata are available. We now use these metadata in our revised analysis (Sect. 3.2 final paragraph).

4) as a counterpoint to using time subsets, the authors could still use a Monte Carlo method of dealing with internal variability by calculating the trend across the full control run, and considering the standard error from that estimate.

As recommended by the referee, we now fit each statistical model of drift to the entire control time series (Sect. 3.2 paragraph 1).

5) One the authors' main conclusions is that drift estimated from time-intergrals has less uncertainty than from a time-derivative, becuase time-derivatives are inherently more noisy and so have highr standard error. This is certainly true, but since the use of subset periods spuriously elevates the uncertainty, this effect is magnified. Figures 3b and f show that there really isn't any difference when you use the entire control run (beyond that fact that one is estimated using an average, and the other using least squares optimisation).

In our revised manuscript, which uses the entire control time series, we clarify that "the linear-method drift uncertainty is smaller than the integrated-bias-method drift uncertainty because integrating cumulatively effectively averages over the substantial inter-annual variability, resulting in a smaller standard error" (Sect. 5.2 paragraph 2). We no longer imply that the two approaches produce different means.

In summary, I think the authors need to make a much clearer argument for using temporal subsets rather than the full series, and also need to consider very carefully how uncertainty from a subset should be extrapolated to the whole series.

Rather than defending our previous approach, we have chosen to substantially revise our analysis in response to both referees' helpful comments. Our revised analysis uses the entire control time series and also uses branch-time metadata, allowing us to consider higher degree polynomials (Sect. 3.2). Our revised quantification of drift uncertainty now corresponds more closely to the linear, quadratic, and cubic drift correction approaches adopted in recent studies.

Additional comments (PDF annotations)

L23: *odd/jarring use of paragraphs, with every sentence being a new paragraph.*

We have now combined many of the shorter paragraphs into longer paragraphs.

L37: *careful with terminology.*

We now use the term "energy leakage" throughout.

L38: *of a state variable (e.g. temperature) rather than a flux.*

We have added "of a state variable" (Sect. 1 paragraph 5).

L92: *maybe worth explaining how this is different from thermal expansion coefficient of seawater (alpha), and what other factors feed into it (other than equn of state).*

In Appendix A, we write, "The thermal expansion of water varies between locations and depths, increasing with temperature, salinity, and pressure (Russell et al., 2000; Piecuch and Ponte, 2014). Remarkably, the thermal expansion coefficient is an order of magnitude larger in the warm low-latitude ocean than it is in the cold high-latitude ocean (Griffies and Greatbatch, 2012). Nevertheless, in practice, we can use a globally-representative coefficient: ϵ, the "expansion efficiency of heat" (Russell et al., 2000)" (Appendix A final paragraph).

L93: *I'd prefer a variable name other than Beta in this context - you're considering steric sea level so this could be confused with the haline contraction coefficient, which is typically referred to as beta.*

Instead of β, we now use η throughout the manuscript.

L124: *why? seems a bit weird to have a paragraph-long discussion and then not state how you arrive at this decision.*

We now extend our analysis beyond the linear approach. Our new "agnostic-method MCDC" includes linear, quadratic, and cubic models of drift (Sect. 3.2 paragraph 5).

L140 and L143: *fundamentally this is inflating the actual* and *This is fundamentally inflating the 'internal variability' in the drift correction. Internal variability will been that any random 150-year draw will be different from any other. BUT you're are also allowing for the statistical error in trend calculation for that 150-year period. BUT the statistical error if calculated over the whole (e.g. 500 year) control run would be much less.*

We accept that our previous use of 150-yr segments tends to inflate the drift uncertainty if the branch time is known. Instead of sampling 150-yr segments, we now use the entire control time series (Sect. 3.2 paragraph 1).

L142: *This is from monthly data - is there any correction applied to sample size to account for temporal autocorrelation in the time series?*

This is very helpful question. We previously overlooked the importance of autocorrelation in the residuals. To account for autocorrelation in the residuals, we now use a heteroskedasticity and autocorrelation consistent covariance matrix (Newey and West, 1987) (Sect. 3.2 paragraph 1). We also clarify that we use annual-mean data (not monthly means) (Sect. 2 paragraph 2).

L156: *That's not a justified assumption. It's true that there were some metadata errors for CMIP5 but even them the branch times could be calculated offline and were made publicly available by Dr Jonathon Gregory. I've not seen any evidence of the same problem in CMIP6; there may be a reference somewhere that I'm not aware of, but if so the authors should cite that.*

We no longer assume this. We now accept that the CMIP6 branch-time metadata can be used (Sect. 3.2 final paragraph).

L173: *why is this relevant to the MCDC approach?*

A sufficient number of samples must be drawn in order to quantify drift uncertainty. We now simply state that "we draw 1500 drift samples, sufficient to quantify drift uncertainty using the 2nd–98th inter-percentile range" (Sect. 3.2 paragraph 1).

L184: *Isn't this just a repeat of section 3.2?*

Previously, we described each alternative MCDC method in different sections, seeking to be clear at the risk of being overly verbose. We have now condensed the description into a single section (Sect. 3.2). Our revised description is considerably more concise.

L185: *you use the integral sign interchangeably between the flux terms above and here.*

We recognise that our previous use of the "$\int$" sign in variable names may have been confusing. To improve clarity, we have now modified our notation throughout the manuscript. Excess system energy is now ΔE (not $\int R$) and excess ocean heat is now ΔH (not $\int H$). The use of "Δ" is consistent with our notation for the change in thermosteric sea-level, which remains ΔZ. The top-of-atmosphere radiative flux is now E' (not R) and the sea-surface heat flux is now H' (not H).

L244: *they really don't look very different at all; given that they were calculated by different numerical methods (averaging vs least squares) they're basically the same.*

Our revised analysis no longer includes this comparison.

L252: *this is correct, the time-derivative will always be noisier that the time integral. But the statement is only methodologically-relevant if expanding the 150-year statistical error to the whole series is valid.*

Our revised analysis now uses the entire control time series (not 150-yr segments). The revised text now reads, "We ascribe the difference between the two methods to the size of the standard error: even if autocorrelation is accounted for, the standard error of the trend of ΔE is smaller than the standard error of the mean of E', because integrating cumulatively effectively averages over the substantial inter-annual variability in E'" (Sect. 4.2 paragraph 2).

L276: *I don't agree that the difference of Beta between simulations is an uncertainty - it may well be a physical response. E.g. under stronger forcing scenarios we expect a more stratified ocean that reduces ocean heat uptake efficiency.*

We agree that the differences in η (previously β) are likely due to a physical response. We now add, "The differences arise from the response of the climate system to different forcing scenarios" (Sect. 4.5 paragraph 3).

L395: *If the forced response is small compared to uncertainty in the method of drift correction is significant, doesn't this imply that the forced response is so weak as to be null?*

This is an interesting observation. In our revised discussion of the ΔZ results, we write, "For two CMIP6 models, the agnostic-method drift uncertainty is large enough to obscure the positive ΔZ signal during the historical period: the 2nd–98th percentile range includes negative values of ΔZ (Fig. S2a and n)" (Sect. 4.4 final sentence).

L378: *that's a bit of an overstatement.*

0.1 W m^{-2} is approximately an order-of-magnitude smaller than 0.9 W m^{-2}. Nevertheless, we now write "much smaller" rather than "an order-of-magnitude smaller" (Appendix A paragraph 4).

Fig. 1: *I find this choice of unit needlessly confusing - it's ambiguous whether it's a flux or an energy integral so for example panel d) is hard to interpret; is the negative drift amplifying over time or is the trend due to the cumulative effect of the drift? It would be easier if state variables (e.g. H) were quoted as J and fluxes (e.g. dH/dt) quoted as W.*

We have adopted the referee's suggestion: we now use YJ for ΔE and ΔH throughout the manuscript.

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