

A traceable global warming record and clarity for the 1.5°C and well-below-2°C goals

Corresponding Author: Professor Gottfried Kirchengast

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Professor Kirchengast,

Your manuscript titled "A traceable global warming record and clarity for the 1.5°C and well-below-2°C goals" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. Specifically, we ask you to:

1. Provide more clarity on how uncertainties in datasets and scaling factors affect the results as well as fully assess the sensitivity of findings to different methods.
2. Enhance the method for quantifying Paris goals by better accounting for climate complexity and natural variability.
3. Include more comparisons with recent research and reconsider the reliance on model estimates over observational data.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look

forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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and also in our style and formatting guide [Communications Earth & Environment formatting guide](https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at <http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf>.

In particular, the Data availability statement should include:

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- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

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Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript presents a new global surface temperature timeseries and proposes a method for quantifying the Paris climate goals. The topic is highly relevant and timely, and the study has the potential to make a significant contribution to the field of climate research.

The study addresses a highly relevant and timely topic. Global warming and the Paris climate goals are of great importance. The authors' new global surface temperature timeseries and tracking method are valuable. The methodology, like using multiple datasets and the "ClimTrace record" with improvements, is innovative and enhances data reliability. The multidisciplinary approach, linking science with policy and law, is also notable.

Major Comments:

1. Data Processing and Uncertainty: While the data sources and processing are described, more details on uncertainty propagation are needed. How do dataset and scaling factor uncertainties affect results? What about the sensitivity to dataset choices and processing methods?
2. Paris Goals Quantification: The proposed method is good but should better handle climate complexity. How does it account for natural variability? Can it be refined as climate knowledge grows? What are its long-term implications?
3. Comparison with Other Studies: More comparisons with recent studies are required. How do this study's findings compare in temperature trends, projections, and goal assessment? This would help position it in the literature.

Reviewer #2 (Remarks to the Author):

Major comments

-Thank you to the authors for their article tackling a highly important topic. The authors appear to have overlooked the recent Betts et al (2023) paper, which is highly relevant: <https://www.nature.com/articles/d41586-023-03775-z>. The article under review combines a number of methods to address issues raised in Betts et al. It presents alternative methods to those in Betts et al and addresses issues raised by Betts et al., falling under a class of methods for measuring current global warming levels being representative of the current time (not an earlier centre point of a 10-year average as in IPCC AR6).

-The paper provides a set of choices for a number of steps toward a current warming estimate. Many of these steps have been trodden before in previous work, e.g. the combination of time series follows similar approaches to that of the IPCC, Forster et al 2023, 2024 papers and WMO State of the Climate reports. Non-linear trend based estimation has been done before for global temperatures (e.g. see IPCC AR5 WG1 Box 2.2 or more recently use of LOWESS smoothed series in Betts et al., 2023, which also combines recent averages with model projections to assess current warming). The trend method used is essentially a local scatter plot smoother (similar to LOWESS but less robust to outliers) with 7 different window sizes and a subsequent box car smoothing, although the consideration of end effects is helpful. In my opinion, it is the combination of methods towards the aim of estimating current warming levels is the main contribution of the paper in my view, along with results that indicate the difference in solution obtainable having made different assumptions to those of other studies.

-The section titled "Estimation of the GMST-to-GSAT scaling factor" presents a broadly similar set of evidence as that underpinning the IPCC AR5 GMST to GSAT conversion factor but comes to a different conclusion of the value of conversion factor to use. In that respect, a large contribution to the difference from IPCC results appears to arise from a difference of opinion based on the same data. Of factors in the opinion, the disregard for observational evidence in favour of physical model estimates for marine air temperature trends is somewhat concerning. Is this the reason that the confidence interval for the GMST to GSAT conversion factor's confidence interval does not include the IPCC best estimate value?

-Extension of the series to the end of 2024 through use of a forecast seems reasonable. The authors may wish to compare their results with end of year observed values. Regardless of the full year now being observed, I would not recommend swapping to the observed values as the method now that they are available. The authors may wish to use full year values as a comparison to motivate validity of the method.

Detailed comments

Line 5 – slight misquote of the Paris agreement text ("limit this temperature increase" should be "limit the temperature increase").

"Holding the increase in the global average temperature to well below 2 °C above preindustrial levels and pursuing efforts to limit the temperature increase to 1.5 °C above pre-industrial levels, recognizing that this would significantly reduce the risks and impacts of climate change"

Page 2 Line 6 – The authors could consider including the full statement rather than splitting the clause of the CoP text. The exact wording of the agreement at CoP matters, very much including the words "recognises" and "resolves" that have been pulled out of the quotation.

Page 2 Line 31 – This statement of choice of 1850-1900 baseline and the formal nature of the relationship between the Paris Agreement and the IPCC may need evidence. In practice the IPCC has adopted 1850-1900, is that sufficient to support the statement? The Paris Agreement does not refer to the IPCC as a monitoring mechanism for its temperature goals but there is a link via UNFCCC.

Page 3 line 8: Can a reference be provided for the statement of physical modelling being needed.

Page 3 line 12: Is “by scaling” here referring to the scale factor of 1 applied in IPCC AR6?

Page 3 line 13: There is currently a relevant preprint on a non-reanalysis GSAT estimate, although currently under open review: <https://essd.copernicus.org/preprints/essd-2024-500/>

Page 3 line 14: The JRA-3Q reanalysis is considered of adequate quality for the World Meteorological Organization to include it as a line of evidence for its State of the Climate assessments <https://library.wmo.int/records/item/69075-state-of-the-climate-2024> . JRA-3Q is mentioned in the methods section – it seems a difference of choice has been made.

Page 5 line 15: please more clearly expand the meaning of these classes in the text. Definitions are currently provided in the figure caption of Fig 2, but I believe moving these to the text would be beneficial for readability.

It may be beneficial to highlight that these are interpretations of the Paris Agreement text that the authors consider to be reasonable. Paris does not define “well below”.

Page 5 line 21: how is “Paris-effort-goal compliance” defined here? Is this “pursuing efforts to limit the temperature increase to 1.5 °C”?

Page 5 line 24: It would be better to clearly define the class before discussing results. It seems RB2C is define as median crossing 1.7C but this would be better defined up front on the first introduction of the classes.

Page 5 line 29: Not sure what is meant by “strongly reasoned warning from overshoot”

References: Please provide a citation for the NOAA GlobalTemp data set and for the GHG/CO2 data sets used in Fig 1. Update – I see the reference is provided in the second reference section following the methods. I think anything included in the main text's figures should appear in the first reference section, although I think both would be merged in the final article for this journal(?)

Fig 1. Please provide an indication of which line is which for the SSP and GHG scenario extension lines.

Fig 2. As previously mentioned, I would suggest moving the threshold definitions into the text. Scenarios are not labelled.

Page 14 line 23-28: It is not clear to me what new evidence is provided in references 36-38 beyond that that informed the IPCC AR6's assessment. Could the authors please elaborate on this?

I also recommend consideration of Richardson 2023 with regards to physical explanations: <https://journals.ametsoc.org/view/journals/clim/36/9/JCLI-D-22-0215.1.xml>

As mentioned in the key points, the difference in conversion factor from IPCC appears to result from a difference in interpretation of broadly the same evidence as used in IPCC, leading to the IPCC's central estimate of GMST to GSAT conversion factor falling outside the confidence range provided here.

Page 15 line 4-15: Evidence provided for marine air temperature records does not differ from the evidence underpinning the IPCC AR6 conclusion of 1.0 ± 0.01 relationship.

Page 15 line 26-32: Is this paragraph not a trivial result of the multiplicative $f = 1.06$ scaling or am I missing something?

Page 18 line 18 – As stated here “the uncertainties mainly express the strength of the interannual (natural) variability in the GST”, i.e. these are not uncertainty estimates for current warming level but rather the expected variability in annual averages about the warming level.

Page 10 line 3-6: I'm failing to follow the rationale for how the intercept theorem applies here to provide a proof that the statement of tending towards a constant derivative holds. Can a reference be provided to support the statement here? The statement also seems somewhat redundant as line 11-12 states that a separate extension approach is used beyond a marginal year.

Page 19 – Algorithmic steps: This section is rather verbose and hard to follow in the absence of a more precise mathematical description. I would have appreciated equations of access to the mentioned code, that is stated to be provided with the paper but is not available at the time of review.

Page 22 line 6: Technically this is not a robust regression method in normal statistical parlance, which refers to a particular class of regression method. I assume the authors are stating that the results are insensitive to some factor. What is the text stating that the regression is insensitive to? I do not see a test of this.

Following text “Careful sensitivity tests verified that the regression results and subsequent regression-corrected trend rate uncertainty results are robust and do not improperly depend on technical details of the regression implementation.” This is rather vague. What technical details is it insensitive to?

Page 22 line 20: "Unadjusted" maybe rather than "uncorrected" or it implies something was incorrect before rather than a processing choice to remove effects.

Page 24 line 14: It is not quite clear to me from the description how the projections were appended to the observed series. The text indicates that a 7% reduction in projection trend was required. Does this mean the projected warming rate beyond 2019 is scaled down to match the warming rate at 2019 in the observed smooth series? Is that justifiable given that the warming rate at 2019 is rather uncertain?

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Version 1:

Decision Letter:

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Dear Professor Kirchengast,

Your revised manuscript titled "A traceable global warming record and clarity for the 1.5°C and well-below-2°C goals" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewer 2. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have handled my comments well and I recommend acceptance.

Reviewer #2 (Remarks to the Author):

A traceable global warming record and clarity for the 1.5 °C and well-below-2 °C goals
Authors: G. Kirchengast and M. Pichler

I thank for authors for their consideration of the previous round of review comments and extensive work to revise the manuscript in response. The expansion to a regular article format, with the space constraints of the Brief Communications format lifted, has allowed substantial improvements to the structure of the paper and improved clarity.

I appreciate the expansion of discussion on the scaling factor in the methods (although see the one comment below relating to a brief discussion of this in main body paper) and clear discussion of reference levels and use of IPCC terminology to describe likelihood of having passed them.

I consider the manuscript to be very close to ready for acceptance, but I ask the authors to first consider my one main comment below.

Main points

1. I appreciate the effort invested to more fully describe the scaling factor range adopted and the inclusion of the new Figure 3. My one criticism is the absence of discussion of the difference in scale factor from that in IPCC AR6 within the main body of the manuscript.

This difference from IPCC AR6 provides a 1.06 ± 0.04 scaling for a policy relevant metric and excludes the central estimate of the scaling factor in IPCC AR6 of 1.0 ± 0.1 , hence it deserves discussion in the main text. Currently the 1.06 value is stated on Page 4 Line 25 without discussion of the reasoning, which is later in the methods. There is no discussion of the summary of the reason for this change of scale factor or its impacts in the main body of the manuscript either here or in the Results and Discussion.

Therefore, my one request to the authors is to briefly comment on why the scale factor differs from that in IPCC AR6 in the main text. This should briefly highlight the key lines of evidence that point to an exclusion of the IPCC central estimate of $f = 1.0$, scaling policy relevant warming estimates of the IPCC or other studies such as Bett's et al by 6%. I am not asking for a substantial addition to the main text in this request, only a brief discussion of the evidence base to signpost readers to the key reasons for this difference from IPCC AR6.

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Author's Response to Reviewers

Manuscript-ID: COMMSENV-24-4036-T (Journal *Communications Earth & Environment*)

Manuscript:

A traceable global warming record and clarity for the 1.5 °C and well-below-2 °C goals

Authors: G. Kirchengast and M. Pichler

We very much thank the reviewers for their thoughtful and constructive assessment and for the many helpful comments for further improvement. We carefully considered all points raised, substantially revised and improved the manuscript along these lines, and provide point-by-point answers to the comments below. Comments are quoted in *italic with gray background*, with the answers inserted below each comment.

Reference to specific parts of the Revised Manuscript (RM) is typically made in the style “...see RM Page N Lines n-m”. A Tracked-Changes Manuscript (TCM) version is provided as well, where the reviewers can see all changes relative to the original manuscript in redlined form; this is typically referred to in the style “...see TCM redlines on Page N”.

Author's Response to Reviewer #1 Comments

1) The manuscript presents a new global surface temperature timeseries and proposes a method for quantifying the Paris climate goals. The topic is highly relevant and timely, and the study has the potential to make a significant contribution to the field of climate research.

The study addresses a highly relevant and timely topic. Global warming and the Paris climate goals are of great importance. The authors' new global surface temperature timeseries and tracking method are valuable. The methodology, like using multiple datasets and the "ClimTrace record" with improvements, is innovative and enhances data reliability. The multidisciplinary approach, linking science with policy and law, is also notable.

Thank you for this very positive basic assessment of our manuscript. We hope you will agree that the substantial additional updates in the revised manuscript (RM) provide significant further improvement. Given that we could expand beyond the strictly limited “Brief Communications” format of the original manuscript (which was transferred from an initial submission to *Nature Climate Change*), we could reasonably deepen with respect to all the aspects that you raise below; browsing the tracked-changes manuscript (TCM) shows the substantial updates throughout. In addition, based on a suggestion by the other reviewer, the description of the core algorithm for constructing the mean GST records and related trend rates (“Moving-window EOT smoothing algorithm”) was substantially clarified; the new Supplementary Information (SI) file contains its description in form of Supplementary Methods.

2) Major Comments: 1. Data Processing and Uncertainty: While the data sources and processing are described, more details on uncertainty propagation are needed. How do dataset and scaling factor uncertainties affect results? What about the sensitivity to dataset choices and processing methods?

We agree that the description of the data processing and uncertainty was somewhat slim in the original manuscript. We have hence significantly expanded and improved on it both in the main text and in the Methods, plus now include dedicated Supplementary Methods (in a separate SI file) to provide a detailed description of the ClimTrace core processing algorithm (the “MW-EOT smoothing algorithm” mentioned in the previous answer). Relevant major updates in the main text (several new paragraphs) are found in the RM in the section Exceeding 1.5°C within ten years on Pages 4-6 (TCM redlines on Pages 6-7), and in the Methods part in the section Input datasets (TCM redlines on Page 24), the section

Estimation of the GMST-to-GSAT scaling factor (TCM redlines on Pages 27-28), and the section Computing the 20-year-mean GST records and trend rates (TCM redlines on Pages 31-33, complemented by the new SI). Aspects of sensitivity are now addressed in various parts; for example, the scaling-factor description is also complemented by an entirely new Figure, Fig. 3 (RM Page 13).

3) Major Comments: 2. Paris Goals Quantification: *The proposed method is good but should better handle climate complexity. How does it account for natural variability? Can it be refined as climate knowledge grows? What are its long-term implications?*

We agree that also this part of the description was able to gain considerably from updates & expansion in the directions you suggest. We have hence also substantially improved the manuscript along these lines, in both the main text and the Methods part. Relevant major updates and extensions (several new paragraphs) are found in the RM in the section Clarity for the Paris climate goals on Pages 7-9 (TCM redlines on Pages 8-11), including also an entirely new Figure, Fig. 5 (RM Page 15), that now also helps to better explain the proposed method and to discuss various implications. Also a dedicated closing section on “Steps towards standardization” is included now (RM Page 10, TCM redlines Page 12).

4) Major Comments: 3. Comparison with Other Studies: *More comparisons with recent studies are required. How do this study's findings compare in temperature trends, projections, and goal assessment? This would help position it in the literature.*

Thank you; yes, definitely, it is very helpful if more such comparisons and discussions are included. We hence also substantially improved the manuscript in this direction; for example the number of Refs has now increased by twenty (from 43 to 63 references total; see TCM redlines on Pages 41-44), due to the expansion of this type of discussion throughout the “Results and discussion”. This includes new entry paragraphs in the section Clarity for the Paris climate goals (TCM redlines on Page 8 bottom and Page 9 top) and more detailed comparisons to/reference to other studies also in the ClimTrace GST record discussion (e.g., TCM redlines on Page 6, lower half, and first two new paragraphs on Page 7).

Author's Response to Reviewer #2 Comments

Major comments:

1) Major comment #1: *-Thank you to the authors for their article tackling a highly important topic. The authors appear to have overlooked the recent Betts et al (2023) paper, which is highly relevant: <https://www.nature.com/articles/d41586-023-03775-z> . The article under review combines a number of methods to address issues raised in Betts et al. It presents alternative methods to those in Betts et al and addresses issues raised by Betts et al., falling under a class of methods for measuring current global warming levels being representative of the current time (not an earlier centre point of a 10-year average as in IPCC AR6).*

Thank you for this very positive basic rating of the value of our manuscript. We hope that you will agree that the substantial additional updates in the revised manuscript (RM) provide significant further improvement. Given that we could expand from the strictly limited “Brief Communications” format of the original manuscript (which was transferred from an initial submission to *Nature Climate Change*), we could reasonably deepen the treatment related to all the aspects that you raise below; browsing the tracked-changes manuscript (TCM) version shows the substantial updates throughout. In addition,

the description of the core algorithm for constructing the GST records and related trend rates (MW-EOT smoothing algorithm) was substantially clarified as you recommended; the new Supplementary Information (SI) file contains this description in form of Supplementary Methods.

Turning back to the important Betts et al. (2023) paper that you mention (now cited as Ref. 5, signaling the prominent role it received): we had in fact come over it soon after the original paper submission; before receiving this review back. Nice and very valuable commentary article in our view. And sure, we immediately realized that our paper should properly relate to it, and we are glad that the revision now allowed us to do so. We already highlight it as part of the introduction, pointing to the importance of better quantifying and monitoring the Paris goals (RM Page 2 Lines 19-20), and in the section Clarity for the Paris climate goals it has become the core of the new entry paragraphs; including that we aligned to the Betts et al. GWL terminology and particularly highlight the utility of the CGWL notion as a starting point of the generalized method we propose (see TCM redlines on Page 8 bottom and Pages 9-11). We also included an entirely new Figure 5 (RM Page 15) that clearly helps to better understand the concept (relates to a suggestion included also in some of the specific comments below).

2) Major comment #2: *-The paper provides a set of choices for a number of steps toward a current warming estimate. Many of these steps have been trodden before in previous work, e.g. the combination of time series follows similar approaches to that of the IPCC, Forster et al 2023, 2024 papers and WMO State of the Climate reports. Non-linear trend based estimation has been done before for global temperatures (e.g. see IPCC AR5 WG1 Box 2.2 or more recently use of LOWESS smoothed series in Betts et al., 2023, which also combines recent averages with model projections to assess current warming). The trend method used is essentially a local scatter plot smoother (similar to LOWESS but less robust to outliers) with 7 different window sizes and a subsequent box car smoothing, although the consideration of end effects is helpful. In my opinion, it is the combination of methods towards the aim of estimating current warming levels is the main contribution of the paper in my view, along with results that indicate the difference in solution obtainable having made different assumptions to those of other studies.*

Thank you for your positive rating of our work also in the direction of how we created innovation by aiming to combine and further advance various methods and tools, and key input datasets, to best reach our goal of a benchmark-quality GST record and its use for better quantifying the Paris goals and then a better tracking of goal compliance. In the RM we have substantially expanded and improved in all respects of this description. In particular, we have significantly improved the description of the core algorithm (MW-EOT smoothing algorithm) in making the relevant section in the Methods part itself more concise (see RM Page 23-24), while now providing a detailed description, including Equations, in the Supplementary Methods (SI file); plus providing a link to the Python code and documentation file (see a link to Gitlab repo in the answer to a related Detailed comment below).

3) Major comment #3: *-The section titled “Estimation of the GMST-to-GSAT scaling factor” presents a broadly similar set of evidence as that underpinning the IPCC AR5 GMST to GSAT conversion factor but comes to a different conclusion of the value of conversion factor to use. In that respect, a large contribution to the difference from IPCC results appears to arise from a difference of opinion based on the same data. Of factors in the opinion, the disregard for observational evidence in favour of physical model estimates for marine air temperature trends is somewhat concerning. Is this the reason that the*

confidence interval for the GMST to GSAT conversion factor's confidence interval does not include the IPCC best estimate value?

Thank you indeed; while having not been entirely surprised that this might be a point of possible debate, we have learned from this comment that it is clearly useful to elaborate on this GMST-to-GSAT scaling factor much more thoroughly; the change from a “Brief Communications” format to a regular article for CommEE fortunately allowed us to do so. We have completely revised and expanded this section on “Estimation of the GMST-to-GSAT scaling factor” in the Methods part (RM Pages 19-21; see also the TCM relines on Pages 26-28). Moreover, we included an entirely new Figure 3 to this end (RM Page 13), which strongly underpins this discussion. Please see more specific pieces of answer also in connection with the relevant specific comments on this matter below.

4) Major comment #4: - *Extension of the series to the end of 2024 through use of a forecast seems reasonable. The authors may wish to compare their results with end of year observed values. Regardless of the full year now being observed, I would not recommend swapping to the observed values as the method now that they are available. The authors may wish to use full year values as a comparison to motivate validity of the method.*

Thank you; we are glad that you like this “prediction feature”, which we also think is really useful. Your recommendation is fully in line with how we liked to treat this now while we have already entered the year 2025. We have hence updated exactly in this direction: Figure 1 now highlights both, the predicted and actual values for 2024, and the related text (RM Page 5 Lines 10-16) allows us to discuss this in the spirit of this comment. We do firmly plan, by the way, to produce the first such prediction of the annual-mean GST for 2025 (and of the underlying CGWL) by August 2025, in line with how we summarize the prediction's utility now (RM Page 5 Lines 15-16).

Detailed comments:

Line 5 – slight misquote of the Paris agreement text (“limit this temperature increase” should be “limit the temperature increase”.

“Holding the increase in the global average temperature to well below 2 °C above preindustrial levels and pursuing efforts to limit the temperature increase to 1.5 °C above pre-industrial levels, recognizing that this would significantly reduce the risks and impacts of climate change”

Thank you – corrected.

Line 6 – The authors could consider including the full statement rather than splitting the clause of the CoP text. The exact wording of the agreement at CoP matters, very much including the words “recognises” and “resolves” that have been pulled out of the quotation.

Thank you – done in this way (see RM Page 2 Lines 5-9).

Page 2 Line 31 – This statement of choice of 1850-1900 baseline and the formal nature of the relationship between the Paris Agreement and the IPCC may need evidence. In practice the IPCC has adopted 1850-1900, is that sufficient to support the statement? The Paris Agreement does not refer to the IPCC as a monitoring mechanism for its temperature goals but there is a link via UNFCCC.

Thank you – improved in the spirit you suggest (see RM Page 3 Lines 7-14).

Page 3 line 8: Can a reference be provided for the statement of physical modelling being needed.

Ok; we have significantly revised and improved this whole part of introducing to GMST and GSAT (see RM Page 3 Lines 18-30).

Page 3 line 12: Is “by scaling” here referring to the scale factor of 1 applied in IPCC AR6?

Yes, we have further clarified also in this paragraph; and explicitly use the term “scaling factor” now.

Page 3 line 13: There is currently a relevant preprint on a non-reanalysis GSAT estimate, although currently under open review: <https://essd.copernicus.org/preprints/essd-2024-500/>

Thank you, this additional reference was very helpful. We have included its citation at various places, in particular related to the discussion of the GMST-to-GSAT scaling factor (relevant Methods subsection is in the RM now on Pages 19-21).

Page 3 line 14: The JRA-3Q reanalysis is considered of adequate quality for the World Meteorological Organization to include it as a line of evidence for its State of the Climate assessments <https://library.wmo.int/records/item/69075-state-of-the-climate-2024> . JRA-3Q is mentioned in the methods section – it seems a difference of choice has been made.

Yes, regarding JRA-3Q we had a careful assessment over the full range 1950-2023, which made us not to use it as a long-term “benchmark record” (we also had direct contact with Yuki Kosaka, the first author of the JRA-3Q 2024 paper, as part of the preparation of this decision). However, as you can see, we do employ JRA-3Q now for the purposes of the new Figure 3 (RM Page 13); it is one of the datasets supporting the related description in the Methods section on the scaling factor (RM Pages 19-20). This new use is now also addressed in the Input datasets subsection (RM Page 16-bottom to 17-top).

Page 5 line 15: please more clearly expand the meaning of these classes in the text. Definitions are currently provided in the figure caption of Fig 2, but I believe moving these to the text would be beneficial for readability.

It may be beneficial to highlight that these are interpretations of the Paris Agreement text that the authors consider to be reasonable. Paris does not define “well below”.

Page 5 line 21: how is “Paris-effort-goal compliance” defined here? Is this “pursuing efforts to limit the temperature increase to 1.5 °C”?

Page 5 line 21: how is “Paris-effort-goal compliance” defined here? Is this “pursuing efforts to limit the temperature increase to 1.5 °C”?

Page 5 line 29: Not sure what is meant by “strongly reasoned warning from overshoot”

Page 5 line 24: It would be better to clearly define the class before discussing results. It seems RB2C is define as median crossing 1.7C but this would be better defined up front on the first introduction of the classes.

Thank you for these valuable comments related to the Paris goals quantification. We fully agree that this type of extensions and various improvements are needed to improve this part in the interest of a clear introduction of the proposed quantification approach; including also the linking to the Betts et al. (2023) article. Now that the strict limits of the “Brief Communications” format did no longer apply, we have completely overhauled this subsection Clarity for the Paris climate goals (see RM Pages 5-7), including introduction of a new Fig. 5 (RM Page 15) that additionally helps facilitate the understanding. We have also substantially improved the discussion of the context and implications, and included also several more references.

Fig 1. Please provide an indication of which line is which for the SSP and GHG scenario extension lines.

Ok – done – see updated caption of Fig. 1 (RM Page 11).

Fig 2. As previously mentioned, I would suggest moving the threshold definitions into the text. Scenarios are not labelled.

Ok – done – see updated caption of Fig. 4 that now includes the previous Fig. 2 (RM Page 14). And please see the answer above to your various comments related to Page 5 of the original manuscript; the description of the Paris quantification approach has been completely overhauled.

Page 14 line 23-28: It is not clear to me what new evidence is provided in references 36-38 beyond that that informed the IPCC AR6’s assessment. Could the authors please elaborate on this?

I also recommend consideration of Richardson 2023 with regards to physical explanations: <https://journals.ametsoc.org/view/journals/clim/36/9/JCLI-D-22-0215.1.xml>

As mentioned in the key points, the difference in conversion factor from IPCC appears to result from a difference in interpretation of broadly the same evidence as used in IPCC, leading to the IPCC’s central estimate of GMST to GSAT conversion factor falling outside the confidence range provided here.

Page 15 line 4-15: Evidence provided for marine air temperature records does not differ from the evidence underpinning the IPCC AR6 conclusion of 1.0 ± 0.01 relationship.

Thank you also for these valuable comments related to the way how we had introduced our update of the GMST-to-GSAT scaling factor. In particular, thank you also for the pointer to Richardson’s 2023 JCLim paper – it was really valuable to prominently co-cite it now, since it provided some further substance specifically to the evidence-points (1) and (2) that we had covered very briefly only in the original manuscript. As summarized in our answer to your “Major comment #3” above, we have completely overhauled also this Methods section Estimation of the GMST-to-GSAT scaling factor (RM Pages 19-21; and see also the new Fig. 3 on RM Page 13), as part of which we also addressed these four comments. We hope you can agree that the description and reasoning is now considerably clearer, and considerably better substantiated, as to the update in this study vs. the previous AR6 estimate.

Page 15 line 26-32: Is this paragraph not a trivial result of the multiplicative $f = 1.06$ scaling or am I missing something?

Actually no, it is not trivial, since this paragraph describes the uncertainty propagation from GMST to GSAT, where the scaling-factor uncertainty comes in as an additional uncertainty component that expands the uncertainty corridor of GSAT vs GMST in a (non-trivial) time-dependent manner. We hence refer also to the light-gray “outer portions” of the GSAT uncertainty range (Fig. 2b) on top of the dark-gray “inner portion” that derives from the GMST uncertainty range (Fig. 2a).

Page 18 line 18 – As stated here “the uncertainties mainly express the strength of the interannual (natural) variability in the GST”, i.e. these are not uncertainty estimates for current warming level but rather the expected variability in annual averages about the warming level.

Page 10 line 3-6: I’m failing to follow the rationale for how the intercept theorem applies here to provide a proof that the statement of tending towards a constant derivative holds. Can a reference be provided to support the statement here? The statement also seems somewhat redundant as line 11-12 states that a separate extension approach is used beyond a marginal year.

Page 19 – Algorithmic steps: This section is rather verbose and hard to follow in the absence of a more precise mathematical description. I would have appreciated equations of access to the mentioned code, that is stated to be provided with the paper but is not available at the time of review.

Thank you for commenting on these various aspects related to our “MW-EOT smoothing algorithm”, optimized for the purpose of interest here. As summarized in our answer to your “Major comment #2” above, we have completely overhauled also the Methods section Computing the 20-year-mean GST records and trend rates (RM Pages 23-24; and Supplementary Methods SI file), as part of which we also addressed these specific comments and improved the related texts. We hope you will agree that the revised description is now clearly more informative. And please find here, at this point in time for your personal information as a reviewer, also the provisional link to a Gitlab Repo that provides access to the Python code and algorithm documentation file: <https://wegcgilab.uni-graz.at/mop/mw-eot>. As mentioned in the “Code availability section”, a final Repo is scheduled to be available at the time of publication, which in addition will contain further paper-related code scripts and datasets.

Page 22 line 6: Technically this is not a robust regression method in normal statistical parlance, which refers to a particular class of regression method. I assume the authors are stating that the results are insensitive to some factor. What is the text stating that the regression is insensitive to? I do not see a test of this.

Following text “Careful sensitivity tests verified that the regression results and subsequent regression-corrected trend rate uncertainty results are robust and do not improperly depend on technical details of the regression implementation.” This is rather vague. What technical details is it insensitive to?

Page 22 line 20: “Unadjusted” maybe rather than “uncorrected” or it implies something was incorrect before rather than a processing choice to remove effects.

Ok, we agree that these have been ‘suboptimal’ formulations that had remained in the description of the regression method that we use for optionally regressing-out ENSO and volcanic signals. We have improved the formulation accordingly in all three of these points; please see in the RM on Page 25 and Page 26-top, or alternatively the TCM redlines on Page 36.

Page 24 line 14: It is not quite clear to me from the description how the projections were appended to the observed series. The text indicates that a 7% reduction in projection trend was required. Does this mean the projected warming rate beyond 2019 is scaled down to match the warming rate at 2019 in the observed smooth series? Is that justifiable given that the warming rate at 2019 is rather uncertain?

Good question, ok. The slightly different trend rate of the TLM model timeseries and the observational timeseries around 2021 is to some degree visible over about the two decades before 2021 so that it likely will have some bearing also over a similar timescale into the future. However, we agree that our simple handling in the original manuscript was suboptimal, i.e., our choice of adjusting the trend rate in an unlimited manner into the future. We hence rectified this and now apply the trend rate adjustment only with a linearly decreasing rate from 2021 onwards, to let it reach zero-rate 20 years after (by 2041). We have also changed the text accordingly; see the TCM redlines on Page 38.

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Author's Response to Reviewers (revision 2)

Manuscript-ID: COMMSENV-24-4036A (Journal *Communications Earth & Environment*)

Manuscript:

A traceable global warming record and clarity for the 1.5 °C and well-below-2 °C goals

Authors: G. Kirchengast and M. Pichler

We very much thank the reviewers for their second assessment and the related (final) comments for further improvement. We carefully considered the remaining comments raised (by reviewer #2) and revised and improved the manuscript accordingly; a point-by-point answer follows below. Comments are quoted in *italic with gray background*, with the answers inserted below each comment.

Reference to changes in the revised manuscript (RM) is made by referring to the page number and relevant paragraph on the page. A marked-up version of the RM is also uploaded to the system (as requested by the editor), showing the changes and (final) improvements vs the previous version.

Author's Response to Reviewer #1 Comments

The authors have handled my comments well and I recommend acceptance.

Thank you for this final positive assessment of our paper and the recommendation for acceptance. We are glad that you have been satisfied with the substantial further improvements in our revised version.

Author's Response to Reviewer #2 Comments

I thank [the] authors for their consideration of the previous round of review comments and extensive work to revise the manuscript in response. The expansion to a regular article format, with the space constraints of the Brief Communications format lifted, has allowed substantial improvements to the structure of the paper and improved clarity.

I appreciate the expansion of discussion on the scaling factor in the methods (although see the one comment below relating to a brief discussion of this in main body paper) and clear discussion of reference levels and use of IPCC terminology to describe likelihood of having passed them.

I consider the manuscript to be very close to ready for acceptance, but I ask the authors to first consider my one main comment below.

Thank you for this very positive assessment of our improved manuscript; we are glad that you found our revision to be substantially improved and that you now see the manuscript very close to ready for acceptance. We carefully considered your one remaining main comment and related suggestions; please see our specific response next below.

Main comment: I appreciate the effort invested to more fully describe the scaling factor range adopted and the inclusion of the new Figure 3. My one criticism is the absence of discussion of the difference in scale factor from that in IPCC AR6 within the main body of the manuscript.

This difference from IPCC AR6 provides a 1.06 ± 0.04 scaling for a policy relevant metric and excludes the central estimate of the scaling factor in IPCC AR6 of 1.0 ± 0.1 , hence it deserves discussion in the main text. Currently the 1.06 value is stated on Page 4 Line 25 without discussion of the reasoning, which is

later in the methods. There is no discussion of the summary of the reason for this change of scale factor or its impacts in the main body of the manuscript either here or in the Results and Discussion. Therefore, my one request to the authors is to briefly comment on why the scale factor differs from that in IPCC AR6 in the main text. This should briefly highlight the key lines of evidence that point to an exclusion of the IPCC central estimate of $f = 1.0$, scaling policy relevant warming estimates of the IPCC or other studies such as Bett's et al by 6%. I am not asking for a substantial addition to the main text in this request, only a brief discussion of the evidence base to signpost readers to the key reasons for this difference from IPCC AR6.

Ok, thank you for suggesting this specific point. We agree that it is preferable, related to this important point of scaling factor update, that we go a bit beyond the very brief factual introduction of this point as it currently appears in the main text. As suggested, we have hence now added a brief discussion to signpost the readers to the key reasons for this difference versus IPCC AR6. We did so at a suitable (prominent) place in the main-text subsection “Exceeding 1.5 °C within ten years”, right after the introductory paragraph that lists the several key innovations included in the new ClimTrace record (see RM Page 5, first paragraph in the upper-half of the page). In addition to this expanded paragraph in the main text, we also improved a related paragraph in the Methods subsection “Estimation of the GMST-to-GSAT scaling factor” in that we simplified a longer sentence into two shorter ones, while at the same time making the content more informative (see RM Page 16, bottom paragraph).

As to your mentioning of the key article Betts et al. (our ref. 5) we note here, as a sideinfo, that we also used this (final) revision opportunity to improve at two other main-text locations. We included as an additional very fitting reference the WMO-No. 1368 report section “Monitoring global temperature for the Paris Agreement” (published very recently in March 2025), authored as well by a Betts et al. team and neatly complementing ref. 5. And along with this, one further improvement was also possible based on another very recent study (Lamboll & Rogelj 2025), related to the rationale for proposing 1.7 °C as well-below-2 °C limit (new small paragraph at top of RM Page 9). As mentioned on introduction above, the marked-up version of the RM shows all these final improvements.

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