nature portfolio

Peer Review File

Time-variations of the climate feedback parameter λ are associated with the Pacific Decadal Oscillation

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

5th Sep 22

Dear Dr Meyssignac,

Please allow us to apologise for the delay in sending a decision on your manuscript titled "Historical observations of the Earth energy budget show the climate feedback parameter varies with time in response to the Pacific Decadal Oscillation". It has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to

• address these comments (that is, either to incorporate the suggestions or provide a compelling argument why the points made by the reviewers are not valid, or relevant to the editorial threshold as outlined below)

AND

• meet our editorial thresholds as outlined below,

Then we would be happy to look at a substantially revised manuscript. In the following, we list our main editorial concerns.

****

***Editorial threshold 1: Present a compelling case for a significant role of PDO in the global energy budget's climate response and models that underestimate the pattern effects, including consideration of other potential natural climate variabilities.

***Editorial threshold 2: Ensure your conclusions are supported by a robust statistical analysis taking into account the uncertainty of the regression approaches used in the model.

****

If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will 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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not 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 reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Min-Hui Lo

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EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of “Historical observations of the Earth energy budget show the climate feedback parameter varies with time in response to the Pacific Decadal Oscillation” by Benoit Meyssignac, Jonathan Chenal, Norman Loeb, and Robin Guillaume-Castel

Reviewer: Shiv Priyam Raghuraman

Climate feedbacks depend on patterns of surface warming, however, it has not been linked to any mode of variability. The study suggests that decadal variations in the net climate feedback is due to the Pacific Decadal Oscillation. This is an intriguing and exciting idea but needs more evidence, quantification, and tests of causation before being published.

Major Comments:

1. Lines 292-295 are the crux of the argument in my opinion. However, what is the correlation between the net feedback variations and PDO? What is the correlation of the net feedback with other indices such as the Atlantic Multidecadal Oscillation (AMO/AMV), Indian Ocean Dipole (IOD), South Pacific Decadal Oscillation (SPDO), Tripole Index (TPI)/Interdecadal Pacific Oscillation (IPO)? It could be that these other indices also show equally good correlation, implying that the PDO is not the driver. This study is showing some correlation (not quantified) between the PDO and net feedback but has not showed causation. The study needs more tests to falsify other hypotheses.

2. What is the sensitivity of the results to different filtering processes? How would a 5-year filter instead of 10-year filter impact the results? What about a 10-year window instead of a 25-year window? How would unfiltered data impact the results? Please perform more such tests to confirm that the PDO influences the net feedback parameter.

Minor Comments:

1. Figure 2 is a bit hard to grasp. Suggest simplifying it or by providing more in-depth explanations on how the figure should be read and interpreted. Is there a reason for why the bottom two angles of the triangle are exceptionally negative?

2. Figure 3a,b: Is the net feedback time series the same in both a and b? The colors seem different. Please make it consistent.

3. Figure 3a,c: What is the black line?

4. Lines 275-277: Although studies have shown a dependence of flux changes on the Pacific SST pattern, the timescale matters (as is pointed out by the authors). For example in our recent study (Raghuraman et al., 2021), we found that on monthly-interannual timescales the tropical reflection of SW radiation decreased as the East Pacific warmed more than the West Pacific, consistent with pattern effect theories. However, on decadal timescales, there was a neutral warming pattern yet there was a large decrease in reflection. Furthermore, LW changes were opposite to pattern effect theories. Could you provide some reconciliation of our paper's results and the results presented here?

5. Lines 302- 303 claims that the study's finding of the PDO-feedback connection has a basis in previous studies too. However, most of these studies do not support this since they look at changes in the Pacific but not the PDO index itself. Andrews and Webb, 2018; Ceppi and Gregory, 2017; Fueglistaler, 2019; Zhou et al., 2016 do not look at PDO. Please revise. Zhou et al., 2017 and Dessler, 2020 do look at PDO but the latter only reports that PDO correlates with SW cloud feedback but does not provide any

correlation value. Furthermore, he does not provide any correlation between the total feedback parameter and PDO, which this study is interested in. Please revise.

6. In fact, Andrews and Webb, 2018 state: “There would be value in future targeted experiments analogous to the ones presented here—including regional patch experiments (e.g., Zhou et al. 2017)—but focusing on prescribed patterns of SST change associated modes of climate variability (such as ENSO and the PDO)…”. Could the authors use one of the climate models in this paper to achieve this? Or use statistical techniques that could achieve this with observations? This would bolster the paper’s argument.

7. On that note, Raghuraman et al., 2021 show that in a climate model forced with fixed SSTs, i.e., each year the same SST is given to the atmosphere model as lower boundary conditions, large variability in the net radiative flux can still be obtained. The range is comparable to a fully coupled model’s net flux range (Supplementary Figure 17c,d). Furthermore, the range of decadal trends obtained in this experiment vs. a fully coupled model’s experiment are similar (Figures 2,3). Together, this means that atmospheric stochasticity alone, without any SST changes, causes decadal variations in the net flux. This should be addressed/acknowledged in the text.

8. Line 325: On 40-year timescales (Figure 3c), it appears that the dependence on PDO disappears, i.e., it shows a flat line. Does this contradict the main result?

9. Line 750: Etminan et al., missing year. It is a 2016 paper. On that note, Etminan et al., 2016 provides IRF and SARF, not other adjustments. How do you account for the non-aerosol cloud adjustments which contribute to ERF?

10. This study in essence is claiming that pattern effect is unforced. It also could be forced. Please see a discussion on this topic in Dessler, 2020 and acknowledge this possibility in the text.

References

1. Dessler, Andrew E. "Potential problems measuring climate sensitivity from the historical record." Journal of Climate 33.6 (2020): 2237-2248. https://doi.org/10.1175/JCLI-D-19-0476.1

2. Raghuraman, Shiv Priyam, David Paynter, and V. Ramaswamy. "Anthropogenic forcing and response yield observed positive trend in Earth’s energy imbalance." Nature communications 12.1 (2021): 1-10. https://doi.org/10.1038/s41467-021-24544-4

Reviewer #2 (Remarks to the Author):

Overview:

In this paper, Meyssignac et al. calculated the feedback parameter over 25 and 40 years windows using data of radiative forcing, global mean surface temperature and planetary heat uptake, and compared these values with that derived from AMIP-piforcing simulations. The authors conclude that these variations are primarily a response to the SST pattern changes associated with the Pacific Decadal Oscillation (PDO). The author suggest that these features are not captured by GCM simulations forced at the surface boundary with the 20th century SST, and claims that GCMs underestimate the pattern effect.

In general, the methods and writing of this paper are good, and I agree with the authors that PDO is important to the pattern effect. However, the evidences provided by this paper do not support the main conclusions, so major revisions are required.

1.The evidences provided by this paper do not support the conclusion that "GCMs underestimate the pattern effect":

(1)According to Fig. 3(c), there is a same decreasing trend for the green line and black line, indicating that the pattern effect indicated from the planetary heat uptake observations are generally consistent with that from models. The mean value of lambda during this period can be determined by ECS instead of pattern effect.

(2)4 out of the 7 models presented in Fig. 3 (c) are within the 17%-83% confidence intervals of the observation-based values, so at least these 4 models do not underestimate the pattern effect.

2.The evidences provided by this paper are not sufficient to conclude that "these variations are primarily a response to the SST pattern changes associated with the Pacific Decadal Oscillation".

(1)According to line 297 "We can not determine either which leads the other", the authors agreed that they could not determine if these variations are a response to the PDO-induced SST pattern changes.

(2)The uncertainty intervals of Fig. 2b are quite big, and the degree of freedom of Fig. 2b is small (60 years in total, and the window is 25, so the effective degree of freedom is only ~3), so we could not conclude that the variation lambda is primarily induced by changes in PDO index (unless a new statistical analysis is performed to demonstrate this).

3.It is premature to conclude that "thy are not captured by GCM simulations forced at the surface boundary with the 20th century SST" in the abstract. According to Fig. 3(a), most model lines are within the uncertainty interval of the observations.

4.The results of this paper seems to be opposite to the following paper: https://doi.org/10.1038/s41558-020-00955-x, which shows that the observed energy imbalance can be reproduced by the GCM-produced pattern effect. What are the causes of these differences?

Reviewer #3 (Remarks to the Author):

Review of “Historical observations of the Earth energy budget show the climate feedback parameter varies with time in response to the Pacific Decadal Oscillation” by Meyssignac et al.

Meyssignac et al. estimate the radiative feedback parameter from observations (as much as possible, forcing estimates still stem from models), specifically from a uniquely long estimate of the planetary heat uptake since 1957. The authors include the knowledge gained over the last ten years or so on feedback variations. Thus, the focus is not to estimate ECS from the entire historical record (as has been done before) but on the time variations of radiative feedbacks. After quantifying them in a novel way, the authors argue that the Pacific Decadal Oscillation varies on the same timescales and thus, likely forces the global variations of the feedback parameter.

The paper is well written and has a clear and followable focus. The methods are novel in several regards: Basing N on the planetary heat uptake and thus extending back several decades and estimating the radiative feedback thoroughly (covering different intervals, start and end dates for the regressions).

I have two major comments/suggestions, which could make the manuscript much stronger: 1. embedding the methods and findings more into the recent literature on the subject and 2. fleshing out the argument that the PDO influences radiative feedbacks (which is currently merely a visual correlation of two heavily processed lines).

1. Embedding the methods and findings more into the recent literature

a) Around Fig. 3 and the associated analyses of AMIP models, it stays unclear how the analysis presented here relates to and why it differs from Andrew et al. 2018 (which is note cited; note that there is an update on the paper in press/available online confirming the general findings and extending it to more models etc.): https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018GL078887 and https://www.essoar.org/doi/abs/10.1002/essoar.10510623.3

Andrews et al. 2018 (and earlier papers on single models) show (with a 30-year regression including inter-annual variability of N and T in AMIP-style simulations) the feedback parameter continually decreased since 1940 or so and is currently between -1.5 and 3 Wm-2/K (e.g. Fig. 2f in the 2018 paper or Fig.6a in the preprint), with a tendency stop stabilize since the 70ies.

How are the findings of Meyssignac et al. reconcilable with Andrews et al. in the time variation? Why are the AMIP simulations just flat lines in Fig. 3c while using very similar simulations (or even the same?) as in Andrews yield strong decadal variability? Which method yields more insights? Are radiative feedbacks varying only on timescales below 40 years? Why?

Along similar lines (not necessarily connecting to the Andrews papers):

In Fig.3c the 40-year feedback parameter based on the observations is -1Wm-2 while the models are around -2Wm-2. Does this imply the models are notoriously too stable? Is that due to T (probably not, since T has rather too high warming rates in CMIP6), N, or F? Isn't that a serious problem for the models (beyond what is discussed here for the shorter timescales)?

b) Acknowledging that the literature on feedback definitions and ECS estimates is extremely messy, there are some recent suggestions for definitions trying to unify different strands of discussions, e.g. ECS estimates from internal variability, the difference between two time periods, or regression based estimates as used here. In the notation of Gregory et al. 2020 (https://link.springer.com/article/10.1007/s00382-019-04991-y) and Rugenstein and Armour 2021 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021GL092983), the lambda in equation 1 would be lambda_eq, the one in equation 2 would be lambda_tilde (or lambda_diff) indicating the regression (opposed to lambda_eff which is called "classical method" here and is used e.g. in Otto et al. 2013). This is not a mere nitpicking, because the lambda used in equation 2 cannot actually plugged into equation 1! The authors are careful not to actually do that (opposed to e.g. Otto et al. 2013) but by calling both quantities simply "lambda", it seems like they are mathematically interchangeable. Similarly using ECS and EffCS interchangeable is not up-to-date anymore (they differ 5-25%). These issues are seemingly not central to the scientific findings in the paper but to move the discussion forward and e.g. compare the findings of Meyssignac et al. with Andrews et al. they are very relevant (and easy to implement).

2. Fleshing out the argument that the PDO influences radiative feedbacks

The authors argue, through Fig.3b, that the PDO varies with the observed feedback parameter (and very likely causes it). The proposed mechanism (hypothesized in other studies) is that a shift from a positive to a negative PDO (occurring since the 80ies or so) warms the western tropical pacific and cools the eastern tropical pacific, which strengthens the low-level atmospheric stability and hence low cloud cover.

Ideally these two steps could be confirmed to at least have the same timescales: a) warming/cooling in the tropical west/east pacific (independent of the global mean warming) and b) increasing low cloud cover. Only a) can be done for the observations (but would be one easy step) and both, a and b could be done in the climate models. Why do AMIP simulations fail at reproducing the feedback evolution — because of a or b? Their west pacific warming should be extremely similar to the one used for the analysis here? Can the authors show that this is true? Thus, the failure of the models must lay in the translation of warming pattern (west vs. east pacific) to the cloud response? If that was true, the conclusion would be diametral to Loeb et al. 2018 and 2020 who argue that the AGCMs do a good job once given the observed SST patterns?

I understand that fleshing out the connection is really hard and might be impossible on the timescales of this paper (i.e. 40-yr windows). However, currently the argument is really based on these two lines, which is a little vague. e.g. the PDO line turns around 1985 or os, but the feedback parameter only around 1990. The feedback parameter turns towards slightly more positive since 2003 or so, but the PDO not. how dependent is this good fit on the 40-year window and 10-year low pass filter? Maybe the clouds in Reanalyses would be OK-enough to see whether the time series had the same time scales?

Minor comments:

Timescales and internal variability:

Proistosescu et al. 2017 and Gregory et al. 2020 discuss a phenomenon called “regression dilution” — making feedback estimates which include (predominantly) internal variability too positive. Meyssignac et al. uses ten year low pass filtered time series to begin with. Does that mean the regression dilution is not occurring here? Are 25-years enough to speak of a feedback or are we still seeing internal variability?

What kept the world stable between 1980 and 1990 when the feedback parameter was 0.5Wm-2 (with some probability above zero)?

line 45 (? lines are not actually aligned, so take the line numbers as tentative): “equilibrium climate sensitivity that is 26% higher than inferred from observations” — one example where using “effective climate sensitivity” would be much more precise. There is literature showing that the actual equilibrium sensitivity is another 15% or so higher than the effective (150-yr regression based) one.

line 66/67 This makes it sound there is only internal variability. What about the forced pattern? What if what we observe is already forced?

line 130-134 “if this decrease …. close to equilibrium is confirmed … “ Given substantial literature on this, my sense is that this debate is settled (within the models) and doesn’t need an “if”.

line 139 equilibrium —> effective

line 142 “as the extent to which GCMs can simulated the observed pattern in SST” — is pretty well analyzed, see e.g. Seager et al. 2019 https://www.nature.com/articles/s41558-019-0505-x, Seager et al. 2022 https://journals.ametsoc.org/view/journals/clim/35/14/JCLI-D-21-0648.1.xml, Olonscheck et al. 2020 https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019GL086773

line 238 Is is clear that the response to volcanoes is due to the pattern effect? How much of the net effect is forcing? This is not the subject of the paper here, but the wording makes it sound as if the entire radiative response to volcanoes comes about through the pattern effect, i.e. through the surface, and not (e.g. the spatial structure?) of the forcing.

Around Fig.1

(I might have missed this) How is the base state defined across the different data sources? The EEI is zero in 1957, the FNA is 1Wm-2, the FAER is -0.4Wm-2 and T is 0.2K —?

Why is the uncertainty in FAER increasing with time (I’d expect the opposite) and why is the uncertainty not centered around the median or best estimate?

Around Fig.2:

Awesome figure! It visualized so clearly how dependent feedback calculations depends on the time and length.

Truncating or rounding the numbers to one or two digits would make it a little bit easier to read off the numbers.

Around Fig.3:

Ideally the differences between a and b (with and without the volcanic correction) could be discussed a bit more. By eye, it seems like the corrected version has smaller feedback values — already in the 1980ies “feeling” Pinatubo already? How meaningful is the strong smoothing when discussing the 3-yr-ish timescale for the volcanoes?

line 750 ?????

line 786-915: I have no expertise in judging the validity of these choices. Is this treatment done for specifically this paper or does it refer to a more generally used dataset which might be documented and available online?

line 794 The ocean

line 997 for these short timescales an orthogonal regression might be more meaningful than an ordinary least square?

REVIEWER COMMENTS:

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We thank reviewer 1 for his positive comments

Major Comments:

1. Lines 292-295 are the crux of the argument in my opinion. However, what is the correlation between the net feedback variations and PDO? What is the correlation of the net feedback with other indices such as the Atlantic Multidecadal Oscillation (AMO/AMV), Indian Ocean Dipole (IOD), South Pacific Decadal Oscillation (SPDO), Tripole Index (TPI)/Interdecadal Pacific Oscillation (IPO)? It could be that these other indices also show equally good correlation, implying that the PDO is not the driver. This study is showing some correlation (not quantified) between the PDO and net feedback but has not showed causation. The study needs more tests to falsify other hypotheses.

Reviewer 1 is right. We should have tested indices of other climate modes of variability. In the revised manuscript we test the correlation between the climate feedback parameter and indices of other climate modes of variability which sign on decadal and longer time scales. We consider the following climate mode indices: the Atlantic Multidecadal Oscillation (AMO), the Indian Ocean Dipole (IOD), the Interdecadal Pacific Oscillation (IPO) and the Southern Annular Mode (SAM) (see Figure 1 below). The correlation coefficients between each climate mode index and the climate feedback parameter time series are given in table 1 below.

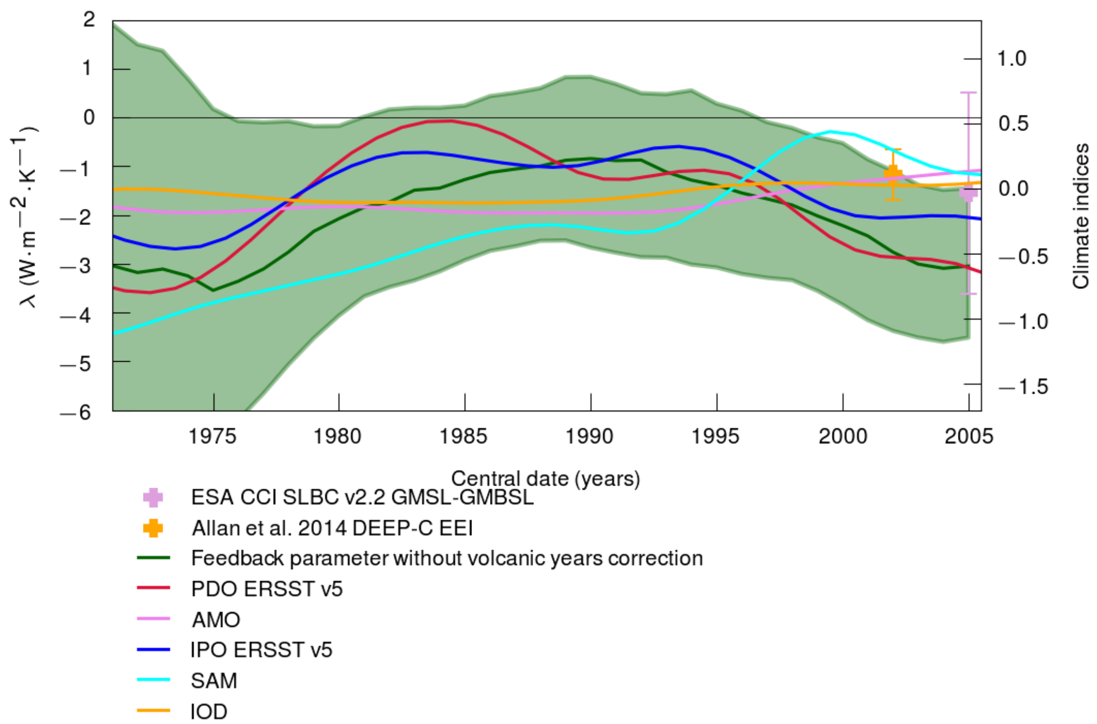

Figure 1 Time series of the climate feedback parameter λ computed over widows of length 25 years. This time series correspond to the horizontal sections indicated on the triangle of Manuscript's Fig. 2 (see the grey dashed

lines on Fig.2). a 25-yr time series of λ (green curve) and 17%-83% confidence level (green shaded area) .The yellow dot indicates the estimate of λ derived with the planetary heat uptake record of the DEEP-C project (Allan et al, 2014). The magenta dot indicates the estimate of λ derived with the planetary heat uptake record of the CCI sea level budget project (Horwath et al, 2022) (see Methods) b 25-yr time series of λ (green curve) corrected for volcanic eruptions and 17%-83% confidence level (green shaded area). The red curve is NOAA ERSST v5 Pacific decadal oscillation index. The blue curve is NOAA ERSST v5 interdecadal Pacific oscillation index. The yellow curve is the Indian Ocean Dipole index. The pink curve is NOAA ERSST v5 Atlantic Multidecadall oscillation index. The light blue curve is the Southern annular mode index. All indices are low-pass filtered with a cutoff period of 15 years

climate mode index	PDO	IPO	SAM	IOD	AMO
Correlation with λ and p value. **No filtering**	Cor :-0.26 Pv : 0.25	Cor :0.04 Pv : 0.85	Cor :0.35 Pv : 0.10	Cor :0.02 Pv : 0.90	Cor :0.21 Pv : 0.25
Correlation with λ and p value. **5yr filtering**	Cor :0.31 Pv : 0.11	Cor :0.40 Pv :0.02	Cor :0.33 Pv :0.08	Cor :-0.04 Pv :0.81	Cor :-0.16 Pv :0.36
Correlation with λ and p value. **10yr filtering**	Cor :0.59 Pv :0.0003	Cor :0.68 Pv :0.0000	Cor :0.32 Pv :0.08	Cor :-0.12 Pv :0.51	Cor :-0.22 Pv :0.23
Correlation with λ and p value. **15yr filtering**	Cor :0.78 Pv :0.0000	Cor :0.89 Pv :0.0000	Cor :-0.20 Pv :0.33	Cor :-0.29 Pv :0.10	Cor :-0.32 Pv :0.07

Table 1 : correlation coefficients and p-values of the correlations between the climate feedback parameter time variations and different climate indices. Results are given for different filtering applied to the input data used for the estimate of λ and for the climate indices. Green values indicate correlations that are significant because the p-value is below 0.05.

Table 1 shows that the correlation of the climate feedback parameter time variations is significant only with the PDO and IPO indices. For any other indices, the correlation is never significant whatever filtering process is used. The PDO and the IPO account for a variety of tropical and extra tropical processes in the Pacific basin which sign on decadal and longer time scales. Although the PDO is computed from the northern Pacific SST only while the IPO is computed with the entire Pacific basin SST, both indices show very similar time variations (their correlation is above 0.9) and very similar signature in the common region (tropics and north of the equator) meaning both indices actually characterize the same basin scale climate mode of variability. In particular both the IPO and PDO are dominated at decadal time scale by an east-west dipole signal in SST which changes sign periodically. The high correlation of PDO and IPO with the time variations of λ suggest a relationship between the decadal SST dipole signal and the time variations in λ. The non-significant correlation of λ time variations with the AMO the SAM and the IOD suggest that the SST in the Indian ocean, the southern ocean and the Atlantic ocean are not related with λ time variations. We added a sentence in the main text of the manuscript on line 312-318 to explain this conclusion and we added Table 1 in the supplementary material to justify this conclusion

2. What is the sensitivity of the results to different filtering processes? How would a 5-year filter instead of 10-year filter impact the results? What about a 10-year window instead of a 25-year window? How would unfiltered data impact the results? Please perform more such tests to confirm that the PDO influences the net feedback parameter.

The filtering process of input data has only a marginal effect on the estimate of λ time variations (see Fig.2 and fig.3). This is because λ estimates are derived from regressions over 25-years long (or longer) windows which tend to filter the data anyway. When comparing fig2a with fig2b, fig2c and fig2d we find that, whatever the filtering, λ shows variations at times scales between 25 and 40 years with a decrease in λ until 1975 followed by an increase until 1990 and then a decrease from 1990 to 2010. It means that λ time variations are only marginally changed under different filtering process. This is confirmed by fig.3. The filtering process has a significant impact only on the climate indices time series. When climate indices time series are filtered with low-pass filters and cut-off frequencies of 10, 5 and 0 years, high frequencies tend to arise in the time series. These high frequencies (at time scales <25 yrs) are inconsistent with λ time variations. Despite the presence of these high frequencies, the low frequencies included in the PDO index and the IPO index are still in phase with λ time variations leading to a significant correlation with λ time variations (see the p-values above 0.05 in table 1 under a 5yr and 10 yr filtering). So in summary, we find that the filtering process impact the results only marginally. For 5yr and 10 yr filtering, the low frequencies in the IPO and PDO index are energetic enough to lead to a correlation with λ that is still significant. In the case of no filtering, the low frequencies are not energetic enough, climate indices are dominated by high frequencies and the correlation with λ vanishes. We added a section in the main text of the manuscript on line 1232-1267 to explain this conclusion and we added 5 fig in the supplementary material to justify this conclusion

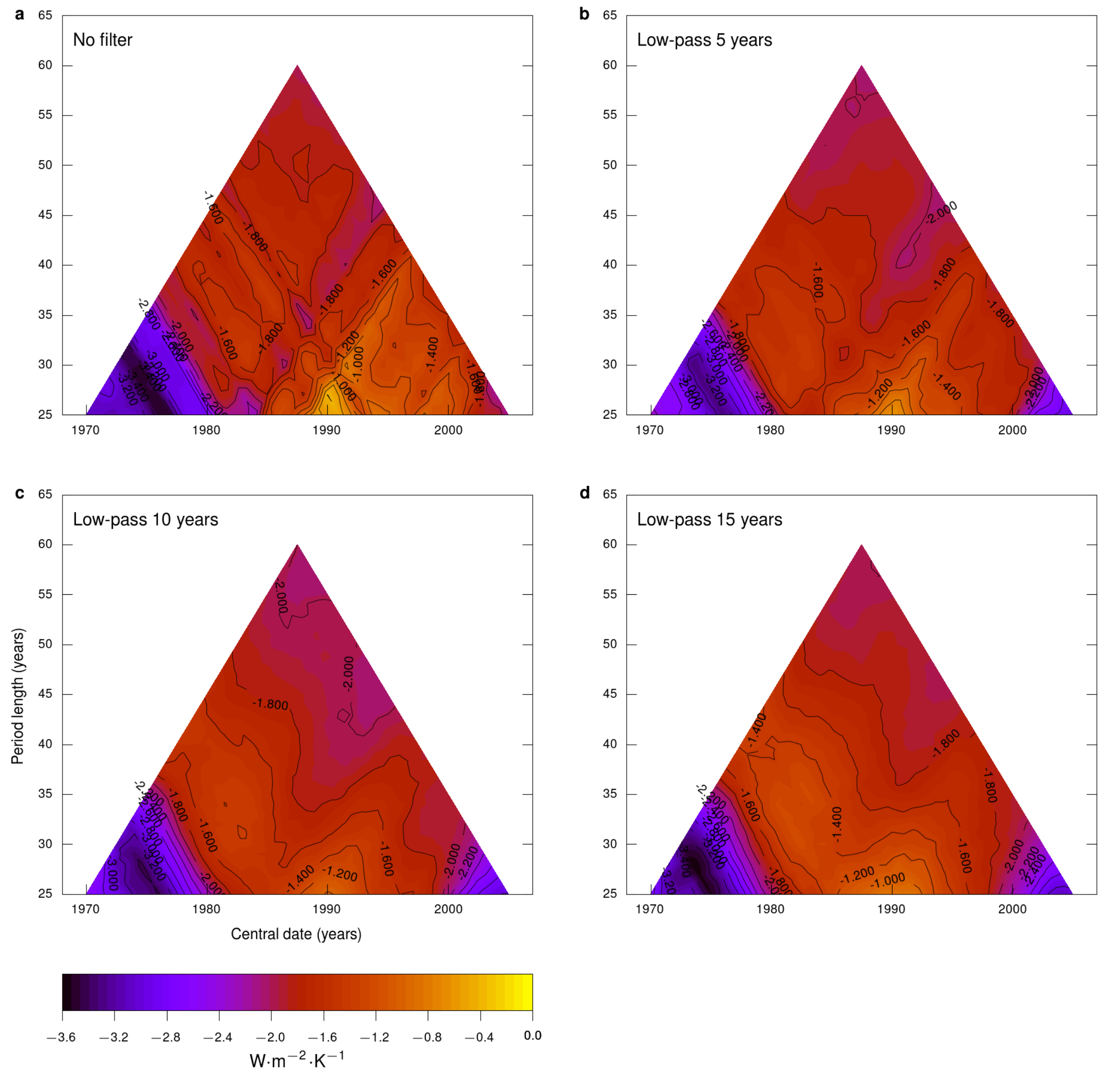

Figure 2 Same as Fig2b) of the main text but with a different filtering of input data.A) 0 year filtering, b)5-year filtering, c) 10-year filtering and d) 15 year filtering

Minor Comments:

1. Figure 2 is a bit hard to grasp. Suggest simplifying it or by providing more in-depth explanations on how the figure should be read and interpreted. Is there a reason for why the bottom two angles of the triangle are exceptionally negative?

The bottom two angles are negative because it corresponds to periods when the PDO was particularly negative.We now explain more in details how Fig 2 should be read. See line 779-782

2. Figure 3a,b: Is the net feedback time series the same in both a and b? The colors seem different. Please make it consistent.

No, the time series are different. In Fig 3a it is not corrected for volcanic eruptions while in Fig3.b it is corrected for volcanic eruptions. For this reason we want to keep a different color. We now clarify this in Fig 3 caption. Please see Fig 3 caption

3. Figure 3a,c: What is the black line?
The black line is the λ time series computed from the ensemble mean of AMIP simulations. It is now clarified in Fig3 caption

4. Lines 275-277: Although studies have shown a dependence of flux changes on the Pacific SST pattern, the timescale matters (as is pointed out by the authors). For example in our recent study (Raghuraman et al., 2021), we found that on monthly-interannual timescales the tropical reflection of SW radiation decreased as the East Pacific warmed more than the West Pacific, consistent with pattern effect theories. However, on decadal timescales, there was a neutral warming pattern yet there was a large decrease in reflection. Furthermore, LW changes were opposite to pattern effect theories. Could you provide some reconciliation of our paper's results and the results presented here?

Over 2001-2020 the trend in Pacific SST shows a clear positive PDO pattern (see Fig 1 below).At the same time λ is increasing because of a decrease in SW reflection partly compensated by an increase in LW (as shown by Fig 11 of Raghuraman et al. Supplementary material). This is consistent with our analysis that shows the high correlation between PDO variations and lambda variations.
It is interesting to note from Fig 3 that over 2001-2020, although the trend in Pacific SST shows a PDO pattern, the East-west gradient in Pacific tropical SST remained almost neutral (see Fig 3. Below). Raghuraman et al. 2021 observed this neutral gradient in the tropics over 2001-2020 and the concomitant decrease in SW reflection. For this reason they suggested that at decadal time scale the pattern effect theories do not hold. We think the situation is more complex and the pattern effect does not only involve tropical SSTs but also extra tropical SSTs (especially at decadal time scales where extra tropical SSts show more variance). We hypothesize that at decadal time scales, when the tropical gradient in SST is weak, the pattern effect is dominated by extratropical SSTs and lead to variations in λ that are significant.

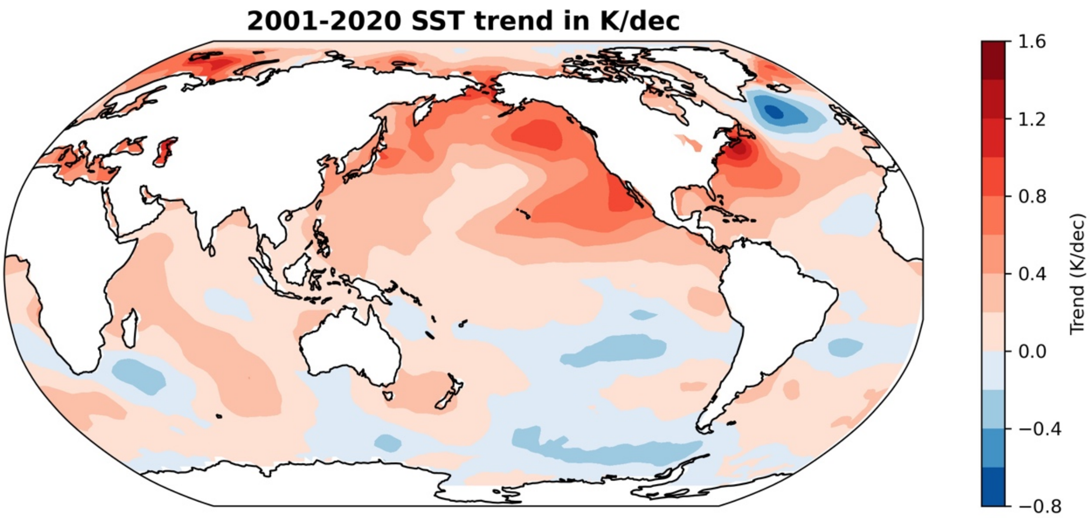

Figure 3 SST trend over 2001-2020

5. Lines 302- 303 claims that the study's finding of the PDO-feedback connection has a basis in previous studies too. However, most of these studies do not support this since they look at changes in the Pacific but not the PDO index itself. Andrews and Webb, 2018; Ceppi and Gregory, 2017; Fueglistaler, 2019; Zhou et al., 2016 do not look at PDO. Please revise. Zhou et al., 2017 and Dessler, 2020 do look at PDO but the latter only reports that PDO correlates with SW cloud feedback but does not provide any correlation value. Furthermore, he does not provide any correlation between the total feedback parameter and PDO, which this study is interested in. Please revise.
Reviewer 1 s right. We revised this sentence accordingly

6. In fact, Andrews and Webb, 2018 state: "There would be value in future targeted experiments analogous to the ones presented here—including regional patch experiments (e.g., Zhou et al. 2017)—but focusing on prescribed patterns of SST change associated modes of climate variability (such as ENSO and the PDO)…". Could the authors use one of the climate models in this paper to achieve this? Or use statistical techniques that could achieve this with observations? This would bolster the paper's argument.

To go in this direction and test this hypothesis in observations, We computed the correlation of our estimate of λ time variations against local SST. The result is striking: see Fig5. Fig 5 is dominated by a PDO/ IPO like pattern over the whole Pacific ocean. The correlation is positive in the Eastern part of the Pacific and negative in the western part meaning that a cooling (resp. warming) in the western pacific and a warming (resp. cooling) in the eastern Pacific leads to a strengthening (resp. weakening) of the global feedback (i.e. a global feedback that becomes less negative) . This independent comparison of λ against local SST shows that lambda variations are indeed associated to the pacific seesaw of the SST characterized by the PDO/IPO. This point confirms that, in observations, λ variations are associated to a Pacific scale East-West dipole structure in SST. If we add to this argument the fact that λ time-variations correlate with the PDO index then we can say that the time scales in the PDO are the same time scales as in λ variations. The correlation map between λ and SST plus the time series correlation between λ and the PDO/IPO index show that there is a match in space and time between PDO/IPO and λ variations. This spatio-temporal match gives confidence in our interpretation that the lambda variations are associated with PDO/IPO variations. We add now this new diagnostic in the core text of the new version of the manuscript. Please see figure 4 of the new version of the manuscript

7. On that note, Raghuraman et al., 2021 show that in a climate model forced with fixed SSTs, i.e., each year the same SST is given to the atmosphere model as lower boundary conditions, large variability in the net radiative flux can still be obtained. The range is comparable to a fully coupled model's net flux range (Supplementary Figure 17c,d). Furthermore, the range of decadal trends obtained in this experiment vs. a fully coupled model's experiment are similar (Figures 2,3). Together, this means that atmospheric stochasticity alone, without any SST changes, causes decadal variations in the net flux. This should be addressed/acknowledged in the text.

We discuss this in the text now and ackowledge this aspect. See line 406

8. Line 325: On 40-year timescales (Figure 3c), it appears that the dependence on PDO disappears, i.e., it shows a flat line. Does this contradict the main result?

No. The main result of the paper is that for" high" frequencies (i.e. periods <40 yrs) λ varies in time in synchronicity with the PDO/IPO. But for longer time scales it is the case anymore. We clarify this in the text now see line 350

9. Line 750: Etminan et al., missing year. It is a 2016 paper. On that note, Etminan et al., 2016 provides IRF and SARF, not other adjustments. How do you account for the non-aerosol cloud adjustments which contribute to ERF?
In the new version of the manuscript we now use the ensemble of ERF from IPCC AR6 in order to propagate properly the uncertainties and estimate rigorously the confidence level of the time variations in lambda (please see the answer to reviewer 2). The ERF of the IPCC AR6 is computed from 2 prescribed SST and sea-ice experiments after removing the TOA energy budget change associated with the land surface temperature response. So it includes the non-aerosol cloud adjustments . Note however that the IPCC ERF only accounts for the direct radiative effect of the land surface temperature response and not the indirect effect on water vaper and tropospheric temperature. But this effect is small of the order a tenth of W.m-2 (see IPCC chap 7)

10. This study in essence is claiming that pattern effect is unforced. It also could be forced. Please see a discussion on this topic in Dessler, 2020 and acknowledge this possibility in the text.
We now acknowledge that point see line 51-53 and 127-128

References
1. Dessler, Andrew E. "Potential problems measuring climate sensitivity from the historical record." Journal of Climate 33.6 (2020): 2237-2248. https://doi.org/10.1175/JCLI-D-19-0476.1
2. Raghuraman, Shiv Priyam, David Paynter, and V. Ramaswamy. "Anthropogenic forcing and response yield observed positive trend in Earth's energy imbalance." Nature communications 12.1 (2021): 1-10. https://doi.org/10.1038/s41467-021-24544-4

Reviewer #2 (Remarks to the Author):

Overview:
In this paper, Meyssignac et al. calculated the feedback parameter over 25 and 40 years windows using data of radiative forcing, global mean surface temperature and planetary heat uptake, and compared these values with

that derived from AMIP-piforcing simulations. The authors conclude that these variations are primarily a response to the SST pattern changes associated with the Pacific Decadal Oscillation (PDO). The author suggest that these features are not captured by GCM simulations forced at the surface boundary with the 20th century SST, and claims that GCMs underestimate the pattern effect.
In general, the methods and writing of this paper are good, and I agree with the authors that PDO is important to the pattern effect. However, the evidences provided by this paper do not support the main conclusions, so major revisions are required.

We thank reviewer 2 for the constructive comments

1.The evidences provided by this paper do not support the conclusion that "GCMs underestimate the pattern effect":
(1)According to Fig. 3(c), there is a same decreasing trend for the green line and black line, indicating that the pattern effect indicated from the planetary heat uptake observations are generally consistent with that from models. The mean value of lambda during this period can be determined by ECS instead of pattern effect.
(2)4 out of the 7 models presented in Fig. 3 (c) are within the 17%-83% confidence intervals of the observation-based values, so at least these 4 models do not underestimate the pattern effect.

Reviewer 2 is right. The statement that "GCM underestimate the pattern" is too strong and unspecific. Over the period 1970-2010 Fig3c clearly shows that some GCMs' estimate of λ are within the uncertainty range of the observational estimate of λ meaning both are consistent with our observational estimate. Actually Fig2 combined with Fig3 shows that λ computed over long windows of time (above 45 years) is almost constant which is consistent with GCMs' estimates of λ. However at smaller time scales (over widows of length <45 years) observations show "high" frequency changes in λ (with variations at time scales of 20 to 30 years). These observed "high" frequency changes in λ are significant (see our answer to the comment below) but they are not reproduced in AGCMs. This is what we meant by ""GCMs underestimate the pattern effect" in the first version of the manuscript. We acknowledge the wording was confusing. In the revised version we now explain in detail that GCMs agree with the observed λ estimate at low frequency but less so at high frequency. See line 350

2.The evidences provided by this paper are not sufficient to conclude that "these variations are primarily a response to the SST pattern changes associated with the Pacific Decadal Oscillation".
(1)According to line 297 "We can not determine either which leads the other", the authors agreed that they could not determine if these variations are a response to the PDO-induced SST pattern changes.

That is true we can not determine which time series leads the other and thus our results do not support the assertion that "the variations in λ are primarly a response to the SST pattern associated to the PDO." This sentence is too affirmative. The only thing we can conclude from our results is that the observed variations of λ are dominated at decadal time scales by time variations that are synchronous and significantly correlated with the Pacific decadal oscillation which suggest a tight link between the net feedback decadal variations and the Pacific decadal oscillation. In addition from the analysis of the recent period that is covered by Ceres we find that λ changed over the last two decades in response to a change in low cloud cover that is concomitant with a shift in the SST pattern from a PDO+ like pattern to PDO- like pattern. It adds further strength in the hypothesis that the variations we observe in λ are a response to SST although it is not conclusive. To reflect this we changed the sentence "these variations are primarily a response to the SST pattern changes associated with the Pacific Decadal Oscillation" into "these variations are probably a response to the SST pattern changes associated with the Pacific Decadal Oscillation". We also changed the general statements of the manuscript to emphasize the spatio temporal match between λ variations and the PDO and we removed any reference to causality

(2)The uncertainty intervals of Fig. 2b are quite big, and the degree of freedom of Fig. 2b is small (60 years in total, and the window is 25, so the effective degree of freedom is only ~3), so we could not conclude that the variation lambda is primarily induced by changes in PDO index (unless a new statistical analysis is performed to demonstrate this).

We understand reviewer 2 is interrogating here 2 aspects of our statistical analysis. First, because of the low degree of freedom, he is asking whether the changes in the λ time series are actually significant or just an artefact linked to the time-correlation of some errors in the λ time series. Second, if the changes prove to be significant he is asking whether the correlation with the PDO is significant.

Concerning the first point, we acknowledge the λ time series is showing a high level of time-correlation and thus errors in the λ time series are likely to be highly correlated in time as well which makes wonder about the significance of the estimated time variations. To check this point, we changed our statistical analysis in order to propagate rigorously the time correlation in errors from the input time series (i.e. time series in N and F) to the λ time series and then we checked in the final λ time series whether the change between its maximum in 1990 and its 2 local minima in 2005 and 1976 are significant (i.e. out of the uncertainty ranges). To propagate the time-correlation in errors we now use from the beginning 500 Monte Carlo draws of the time series in N and F accounting for their uncertainty (and its time correlation) and then we calculate λ on the sliding windows applied to the time series in N and F. This is different from our previous approach where we computed N and F and their uncertainty ranges independently over each sliding window. Our new approach allows to generate a-priori an ensemble of 500 F-time series and 500 N-time series which include the time correlation of their errors. To calculate the 500 F-time series we used the ensemble of F-time series from the IPCC AR6 and we selected randomly 500 time series out of the 100 000 F-time series provided by the IPCC AR6. For the 500 N-time series we assumed the time correlation in errors in N is dominated by the error induced by the internal variability. The time correlation in the internal variability in N decrease exponentially from 1 to 0 with a typical e-folding time scale of 2 years (as suggested by climate models see Palmer and Mc Neal 2014). So to draw the 500 N-time series we used a variance covariance matrix with diagonal terms equal to the variance of the ensemble of the three ocean heat uptake estimate (i.e. the in-situ solution, the Aran solution and the sea level budget solution see methods) and cross diagonal term aligned with a 2-yr e-folding exponentially decreasing covariance. Fig 3 of the new version of the manuscript shows the updated estimate of λ estimated over successive 25 year windows(update of fig 3 of the first version of the manuscript).

The similarity of the new version of fig3 and fig3 of the first version of the manuscript is striking meaning that the uncertainty range computed on λ is robust across different calculation methods. There is an important difference between this new figure 3 and fig3 from the first version of the manuscript. Fig 3 here is generated with an ensemble of λ time series. Thus here the uncertainty ranges at different times are correlated meaning that points of the green shaded area at different times are not independent. With our new statistical approach we retained the correlation information so we can now check whether the change in the λ time series between its maximum in 1990 and its 2 local minima in 2005 and 1976 are significant accounting for the uncertainty ranges' correlation. To do so we compute the difference in l λ between 1990 and 1976 and between 2005 and 1990 for each time series of the ensemble of lambda time series and check whether these differences are statistically significantly different from 0. We find that lambda(2005)-lambda(1990) is significantly negative at the 71.0%CL and lambda(1990)-lambda(1976) is significantly positive at the 76.1%CL. It shows that the changes we observe in lambda are significant. The text of the manuscript is now changed to provide this new piece of information. We think the statistical method developed here is superior to the statistical method developed in the first version of the manuscript. We decided to use the new statistical method as the primary method. The results of the manuscript, the figures and the method section has been updated accordingly.

Concerning the second point and the significance of the correlation with the PDO, this point has been raised by reviewer 1 as well. We tested the correlation with different indices including the PDO, the IPO, the SAM, the IOD and the AMO. For each index we tested the significance of the correlation. We also tested the sensitivity of the correlations to the smoothing of the data. Please see our answer to reviewer 1 comment. In short, we find that the correlation of the λ time series is significant only with the PDO and IPO indices and this is robust for different smoothing of the data from a 5 year smoothing to a 15 year smoothing. Both IPO and PDO are indices of the Pacific basin-wide SST changes. They are highly correlated with each other (>0.9) meaning both indices actually characterize the same basin scale climate mode of variability. In particular both the IPO and PDO are dominated at decadal time scale by an east-west dipole signal in SST which changes sign periodically. The high correlation of PDO and IPO with the time variations of λ confirms a tight relationship between the decadal SST dipole signal and the time variations in λ.

3.It is premature to conclude that "thy are not captured by GCM simulations forced at the surface boundary with the 20th century SST" in the abstract. According to Fig. 3(a), most model lines are within the uncertainty interval of the observations.

That is true that some models do capture the mean lambda over the period 1971-2000 but none of the model actually shows significant variations at "high" frequency i.e. with a typical time scale of ~15 yrs as observtions do. We agree the sentence "they are not captured by GCM simulations forced at the surface boundary with the 20th century SST" is not clear and we reformulate now see line 350 and the associated paragraph

4.The results of this paper seems to be opposite to the following paper: https://doi.org/10.1038/s41558-020-00955-x, which shows that the observed energy imbalance can be reproduced by the GCM-produced pattern effect. What are the causes of these differences?

Zhou et al. 2021 never compute estimate of the historical lambda so it is difficult to compare our results with theirs. In addition, in Zhou et al. approach, the authors look at interannual time scales rather than decadal time scales as we do here. They also make comparisons with observational estimates only from 1984 to 2010 while we look at a longer period here (from 1971on). And, they do not correct for volcanic eruptions which complicate further the

comparison. So it is not clear how our results compare with Zhou et al. 2021 and we don't exactly understand what reviewer 2 means with "this paper seems to be opposite to the following paper: https://doi.org/10.1038/s41558-020-00955-x, ». We suspect reviewer 2 actually means that Zhou et al. 2021 find a good agreement between the TAO radiation budget of Amip simulations and the TOA radiation budget from CERES and DEEPC (as shown on their Fig 1c) meaning that Amip simulations should show a λ that is consistent with observations at least since 1984.
If it comes to that, we find actually that there is qualitatively good agreement between fig1c of Zhou et al. 2021 and our Fig 3a. Indeed, if you look at Zhou et al. Fig1c in details you will see that there is a positive trend since 1984 in DEEPC and CERES TOA radiation budgets that is not reproduced by amip simulations. A positive trend in N while the trend in SST remains the same means a negative trend in lambda. So Zhou et al. fig 1c suggest that amip simulations show small trends in lambda since 1984 while observations show a negative trend. That is consistent with our Fig3a. Another important feature of Zhou et al. fig.1c is that it does not show any important bias in the mean N over 1984-2010 between Amip simulations and observions suggesting that the mean λ should be similar over 1984 -2010 between amip simulations and observations. This is because Zhou et al. added a constant Nref of +0.04W.m-2 to N from the amip simulations to account for the radiative imbalance in 1870-1880. If we do the same, it results in an upward shift of the time series in λ estimated from amip simulations and it leads to a consistent mean between amip λ and observed λover the period 1984-2010. So we find that our results are actually qualitatively consistent to Zhou et al. 2021 in terms of mean and trend over 1984-2010.

Reviewer #3 (Remarks to the Author):

Review of "Historical observations of the Earth energy budget show the climate feedback parameter varies with time in response to the Pacific Decadal Oscillation" by Meyssignac et al.

Meyssignac et al. estimate the radiative feedback parameter from observations (as much as possible, forcing estimates still stem from models), specifically from a uniquely long estimate of the planetary heat uptake since 1957. The authors include the knowledge gained over the last ten years or so on feedback variations. Thus, the focus is not to estimate ECS from the entire historical record (as has been done before) but on the time variations of radiative feedbacks. After quantifying them in a novel way, the authors argue that the Pacific Decadal Oscillation varies on the same timescales and thus, likely forces the global variations of the feedback parameter.

The paper is well written and has a clear and followable focus. The methods are novel in several regards: Basing N on the planetary heat uptake and thus extending back several decades and estimating the radiative feedback thoroughly (covering different intervals, start and end dates for the regressions).

I have two major comments/suggestions, which could make the manuscript much stronger: 1. embedding the methods and findings more into the recent literature on the subject and 2. fleshing out the argument that the PDO influences radiative feedbacks (which is currently merely a visual correlation of two heavily processed lines).

1. Embedding the methods and findings more into the recent literature

a) Around Fig. 3 and the associated analyses of AMIP models, it stays unclear how the analysis presented here relates to and why it differs from Andrew et al. 2018 (which is note cited; note that there is an update on the paper in press/available online confirming the general findings and extending it to more models etc.): https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018GL078887 and https://www.essoar.org/doi/abs/10.1002/essoar.10510623.3

Andrews et al. 2018 (and earlier papers on single models) show (with a 30-year regression including inter-annual variability of N and T in AMIP-style simulations) the feedback parameter continually decreased since 1940 or so and is currently between -1.5 and 3 Wm-2/K (e.g. Fig. 2f in the 2018 paper or Fig.6a in the preprint), with a tendency stop stabilize since the 70ies. How are the findings of Meyssignac et al. reconcilable with Andrews et al. in the time variation? Why are the AMIP simulations just flat lines in Fig. 3c while using very similar simulations (or even the same?) as in Andrews yield strong decadal variability? Which method yields more insights? Are radiative feedbacks varying only on timescales below 40 years? Why?

We agree Andrews et al. 2018 is an important piece of work that need to be commented in the light of our results. We now refer to it and comment on it manyt time in the last pargarph of the manuscript.
Concerning the differences with Andrews et al. 2018, actually our calculation of λ is totally consistent with Andrews et al. calculation. We show on Fig4 below our computation of lambda from the AMIP simulation of the HadAM3 model over 30 years windows with the same method as Andrews et al. 2018 and the same axis as in Fig2.f of Andrews et al. 2018.

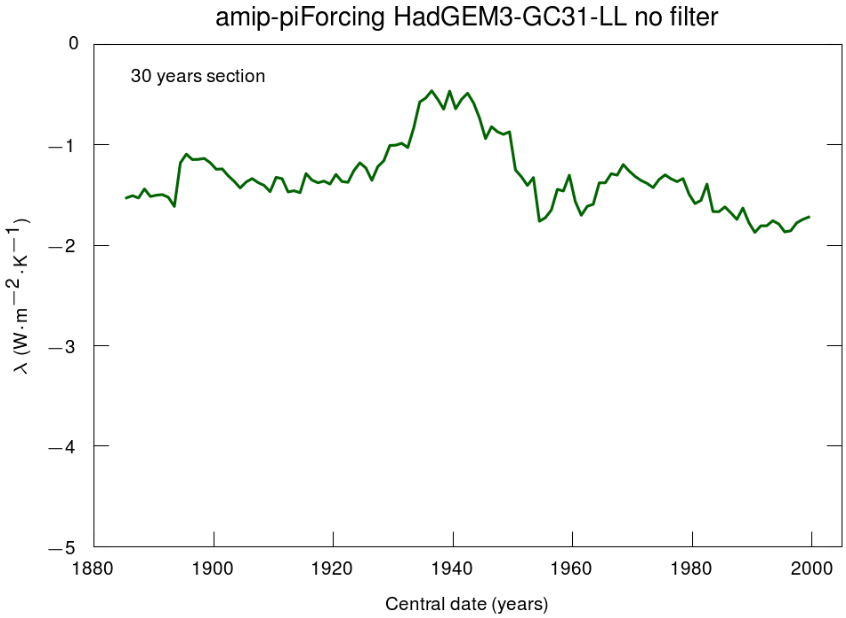

Figure 4 : same as fig2f of Andrews et al. 2018but computed with our code

Figure 4 reproduces precisely the HadAM3 curve of figure2.f of Andrews et al. 2018 meaning that, when we apply Andrews et al. 2018 method we find the same estimate of λ. On fig 3a and 3c of our manuscript, the λ time series from HadAM3 is flat compared to Figure 4 above and to fig 2f of Andrews et al. 2018. This is because of the smoothing that is applied to the input data before the computation of λ. In our calculation, we apply the same smoothing to the input data for observations and for amip simulations (Fig 3a and 3c). This smoothing tends to flatten the interannual variability (that is strong in amip simulations between 1971 and 2010 see fig4 above) and it tends to emphasize the decadal variability (which is small in amip simulations between 1971 and 2010). Since there is low decadal variability in amip simulations of lambda from 1970 to 2010 we end up with this flat line. In contrast, our observational estimate of λ is dominated by decadal variability and shows significant variations at time scales around 25 years.

In summary, we find that our estimate of λ from amip simulations is consistent with the estimate from Andrews et al. 2018. It is just that, in our estimate, the smoothing of the input data (i.e. before the computation of lambda) emphasize the decadal variability in lambda that is very small over the period 1971-2010 in amip simulations. To check that our results are not too sensitive to the smoothing we re-ploted fig 3b but with no smoothing. The result is shown on Fig 5 below

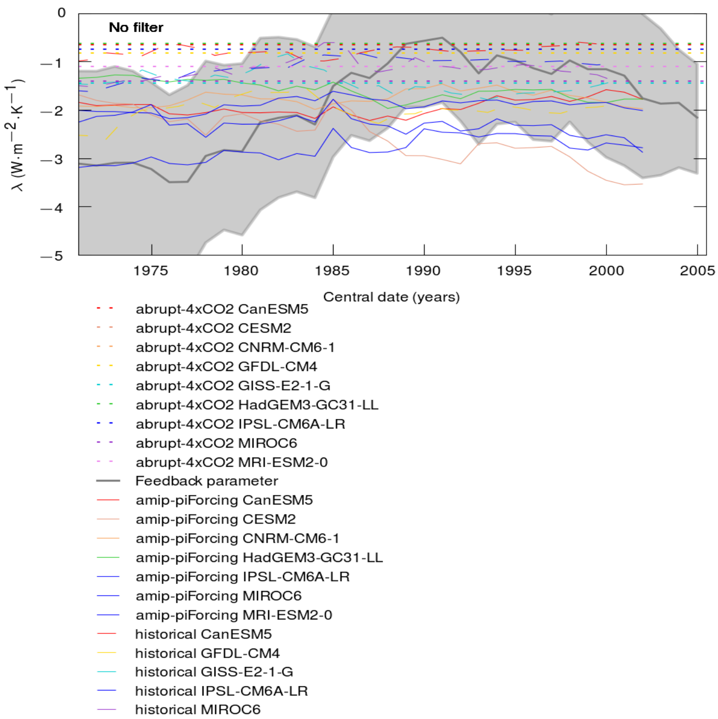

Figure 5: same as fig3a and fig3c of the manuscript but without any smoothing of the input data

In figure 5, the time series of λ from amip simulations is now similar to Andrews et al. 2018 time series , as expected. WE see also from fig 5 that the time series of lambda from observations is largely dominated by decadal variability with a typical time scale of 25 yrs. This large decadal variability is absent from the amip curve confirming that our result is robust whatever the smoothing applied to the input data (see also the method section of the new manuscript)

Along similar lines (not necessarily connecting to the Andrews papers):

In Fig.3c the 40-year feedback parameter based on the observations is -1Wm-2 while the models are around -2Wm-2. Does this imply the models are notoriously too stable? Is that due to T (probably not, since T has rather too high warming rates in CMIP6), N, or F? Isn't that a serious problem for the models (beyond what is discussed here for the shorter timescales)?

It is difficult to say that models are too stable compared to observations as the uncertainty range of observations actually encompass at least four models and also the ensemble mean of the models. So for several models the difference with observation (in particular the mean over 1971-2010) is not significant. We actually looked at the mean λ (and the ECS) in details in a previous paper (Chenal et al. 2022) and we compared it with model simulations. We found they agree within uncertainties. In this previous paper we detail the different sources of uncertainties in the observed estimate of the mean λ. We acknowledge the uncertainties are large. We would need smaller uncertainties in observed lambda to determine whether the difference with models is significant. But getting smaller uncertainties is not an easy thing to do. In particular, we would need much more precise estimate of the aerosol forcing…At this stage the only thing we can say concerning the mean of λ is that it is consistent between amip simulations and observations within uncertainties and we just don't have sufficiently accurate observations of the aerosols and the ocean heat uptake to reduce these uncertainties. We aknowledge that on line 350

b) Acknowledging that the literature on feedback definitions and ECS estimates is extremely messy, there are some recent suggestions for definitions trying to unify different strands of discussions, e.g. ECS estimates from internal variability, the difference between two time periods, or regression based estimates as used here. In the notation of Gregory et al. 2020 (https://link.springer.com/article/10.1007/s00382-019-04991-y) and Rugenstein and Armour 2021 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021GL092983), the lambda in equation 1 would be lambda_eq, the one in equation 2 would be lambda_tilde (or lambda_diff) indicating the regression (opposed to lambda_eff which is called "classical method" here and is used e.g. in Otto et al. 2013). This is not a mere nitpicking, because the lambda used in equation 2 cannot actually plugged into equation 1! The authors are careful not to actually do that (opposed to e.g. Otto et al. 2013) but by calling both quantities simply “lambda”, it seems like they are mathematically interchangeable. Similarly using ECS and EffCS interchangeable is not up-to-date anymore (they differ 5-25%). These issues are seemingly not central to the scientific findings in the paper but to move the discussion forward and e.g. compare the findings of Meyssignac et al. with Andrews et al. they are very relevant (and easy to implement).

We agree this vocabulary issue is an important issue. We now acknowledge that there are different definitions of the global feedback parmeter and the ECS and we also clarify our notation to avoid confusion. Please see the new notations for λ in the text

2. Fleshing out the argument that the PDO influences radiative feedbacks

The authors argue, through Fig.3b, that the PDO varies with the observed feedback parameter (and very likely causes it). The proposed mechanism (hypothesized in other studies) is that a shift from a positive to a negative PDO (occurring since the 80ies or so) warms the western tropical pacific and cools the eastern tropical pacific, which strengthens the low-level atmospheric stability and hence low cloud cover.
Ideally these two steps could be confirmed to at least have the same timescales: a) warming/cooling in the tropical west/east pacific (independent of the global mean warming) and b) increasing low cloud cover. Only a) can be done for the observations (but would be one easy step) and both, a and b could be done in the climate models.
Why do AMIP simulations fail at reproducing the feedback evolution — because of a or b? Their west pacific warming should be extremely similar to the one used for the analysis here? Can the authors show that this is true? Thus, the failure of the models must lay in the translation of warming pattern (west vs. east pacific) to the cloud response? If that was true, the conclusion would be diametral to Loeb et al. 2018 and 2020 who argue that the AGCMs do a good job once given the observed SST patterns?
I understand that fleshing out the connection is really hard and might be impossible on the timescales of this paper (i.e. 40-yr windows). However, currently the argument is really based on these two lines, which is a little vague. e.g. the PDO line turns around 1985 or os, but the feedback parameter only around 1990. The feedback parameter turns towards slightly more positive since 2003 or so, but the PDO not. how dependent is this good fit on the 40-year window and 10-year low pass filter? Maybe the clouds in Reanalyses would be OK-enough to see whether the time series had the same time scales?

To determine the exact SST pattern that is associated to the variations of lambda, we regressed the observed λ against the time series in SST (smoothed with a 15 yr moving average) at each grid point of the SST product. Fig 6 shows the map of correlation coefficients. The result is striking. Fig 6 is dominated by a PDO/ IPO like pattern over the whole Pacific ocean. The correlation is positive in the Eastern part of the Pacific and negative in the western part meaning that a cooling (resp. warming) in the western pacific and a warming (resp. cooling) in the eastern Pacific leads to a strengthening (resp. weakening) of the global feedback (i.e. a global feedback that becomes less negative) . This comparison of λ against local SST shows that λ variations are indeed associated to the Pacific seesaw in SST characterized by the PDO/IPO. This point confirms step a) for the observations. If we add to this argument the fact that lambda variations correlate with the PDO index then we can say that the time scales in the PDO are the same time scales as in λ variations. The correlation map between λ and SST plus the time series correlation between λ and the PDO/IPO index show that there is a match in space and time between PDO/IPO and λ variations. This spatio-temporal match gives confidence in our interpretation that the lambda variations are associated with PDO/IPO variations. We add now this new diagnostic in the core text of the new version of the manuscript. Please see Fig 4 of the new version of the manuscript and the whole discussion around this figure

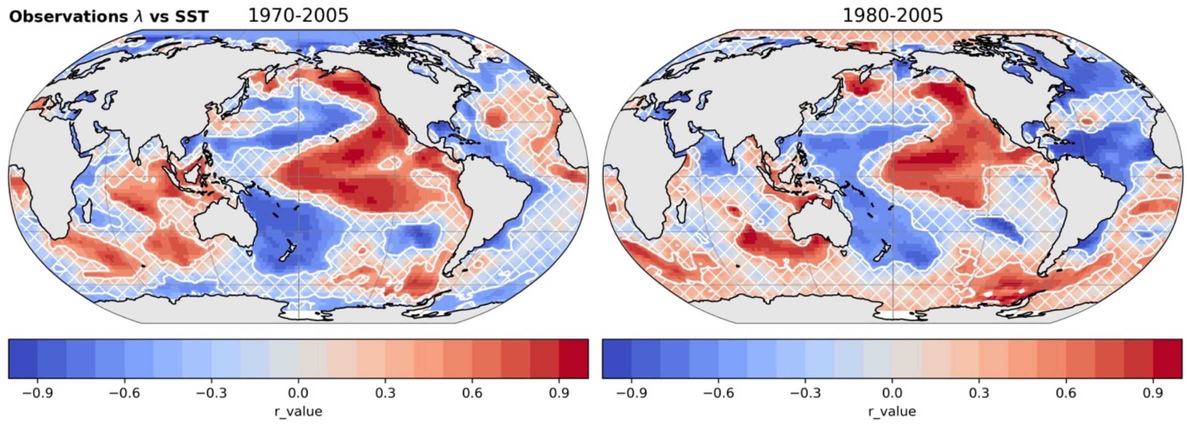

Figure 6 : correlation coefficient between the time series of lambda (as in Fig3) and the local SST smoothed with a 15 yr low pass filter. The hatched areas marqus the regions where the correlation between lambda and SST are not significant at the 90%CL (pvalue<0.1). Upper panel: the correlation is computed over the period 1970-2005, lower panel: the correlation is computed over the period 1980-2005

We checked in AMIP simulations if we could find the same spatio-temporal match between λ variations and the PDO/IPO. We regressed the AMIP λ against the time series in SST (smoothed with a 15 yr moving average) at each grid point of the AMIP simulation. The spatio-temporal match is not as clear in AMIP simulations as in observations. Over the period 1980-2005 correlation map shows a general PDO/IPO pattern although the pattern correlation is not significant for large portions of the Pacific ocean. Note that over this period of 1980-2005 , large portions of the southern ocean SST correlates as well with λ variations unlike in observations .It suggest that the PDO/IPO is not the only responsible for λ variations in the AMIP simulations, some changes in the southern ocean play a role as well. For the longer period 1970-2005 the correlation map does not show the IPO/PDO pattern any more (unlike observations). It only shows a strong correlation with the Southern ocean SST. This result suggests that before 1980 the relation between λ and SST has changed in AMIP simulations. It explains certainly why AMIP simulation don't show any significant correlation between λ time-variations and the IPO/PDO index over the whole period 1970-2005. The strong correlation pattern in the southern ocean over 1970-2005 suggests AMIP λ variations are more sensitive to SST changes in the southern ocean than in the tropical Pacific on the overall period 1970-2005. This means that, although AMIP simulations are forced with an SST that shows observed see saw in PDO/IPO since 1970, λ variations in AMIP simulations are more sensitive on the overall decadal changes in Southern ocean SST. The relationship with southern SST is particularly strong before 1980. I is less so after 1980 such that the relation of λ with the PDO/IPO SST see saw emerges. On the overall these results suggest that AMIP simulations are generally more sensitive to the southern ocean SST decadal changes than observations.

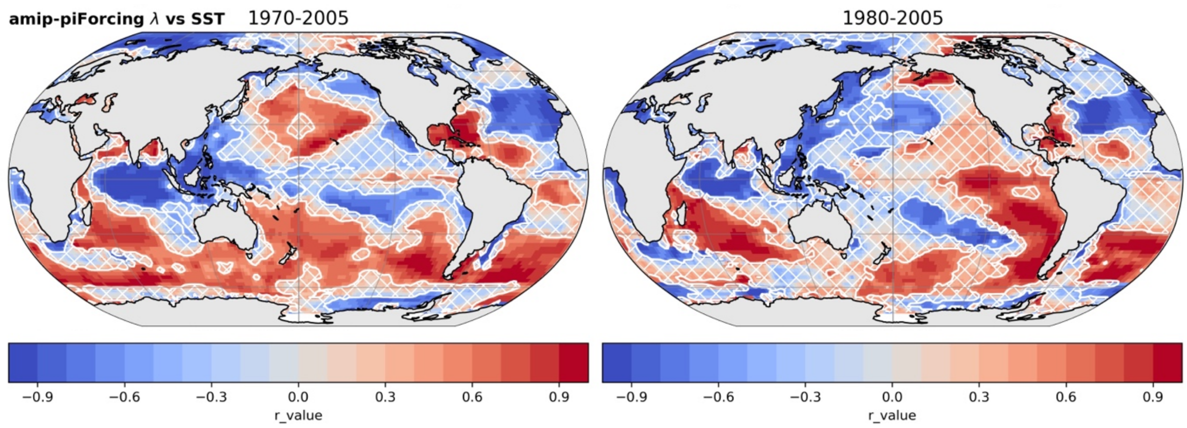

Figure 6same as fig5 but with HadAM3 amip simulation

We check the relation with cloud cover in AMIP simulations. We computed the correlation map of lambda variations against total cloud cover. As for SST we find that the correlation map has changed between 1980-2005 and 1970-2005. Over 1980-2005 the correlation map shows that, when changes in λ are associated with the

PDO/IPO SST pattern there is a decreasing cloud cover in the south-eastern Pacific which is consistent with the mechanism proposed by Loeb et al. 2021 and recalled in the core text. However over 1970-2005 when changes in λ are no more associated with the PDO/IPO SST pattern the decreasing in cloud cover in the south-eastern Pacific is compensated by an increasing cloud cover over the tropical eastern pacific. This compensation is probably linked to the SST in the southern ocean and it is potentially the explanation for the low variations in λ over 1970-2005 in AMIP simulations.

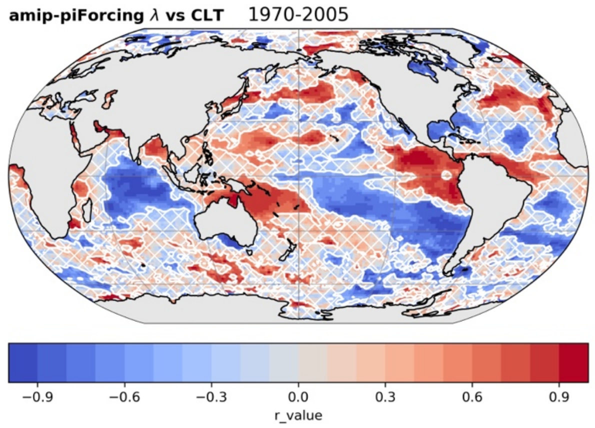

Figure 7: correlation coefficient between the time series of lambda (as in Fig3) and the local total cloud cover smoothed with a 15 yr low pass filter. The hatched areas marqus the regions where the correlation between lambda and SST are not significant at the 90%CL (pvalue<0.1). Upper panel: the correlation is computed over the period 1970-2005, lower panel: the correlation is computed over the period 1980-2005

We checked that our results are not sensitive to the smoothing. We show below figures 5, 6 and 7 with a 5yr smoothing. Results are the same: the correlation patterns are the same as for Fig 5,6 and 7. It is just that, because of the higher level of noise from interannual variability, regions where the correlation pattern is significant above the noise are reduced. See Fig. 8

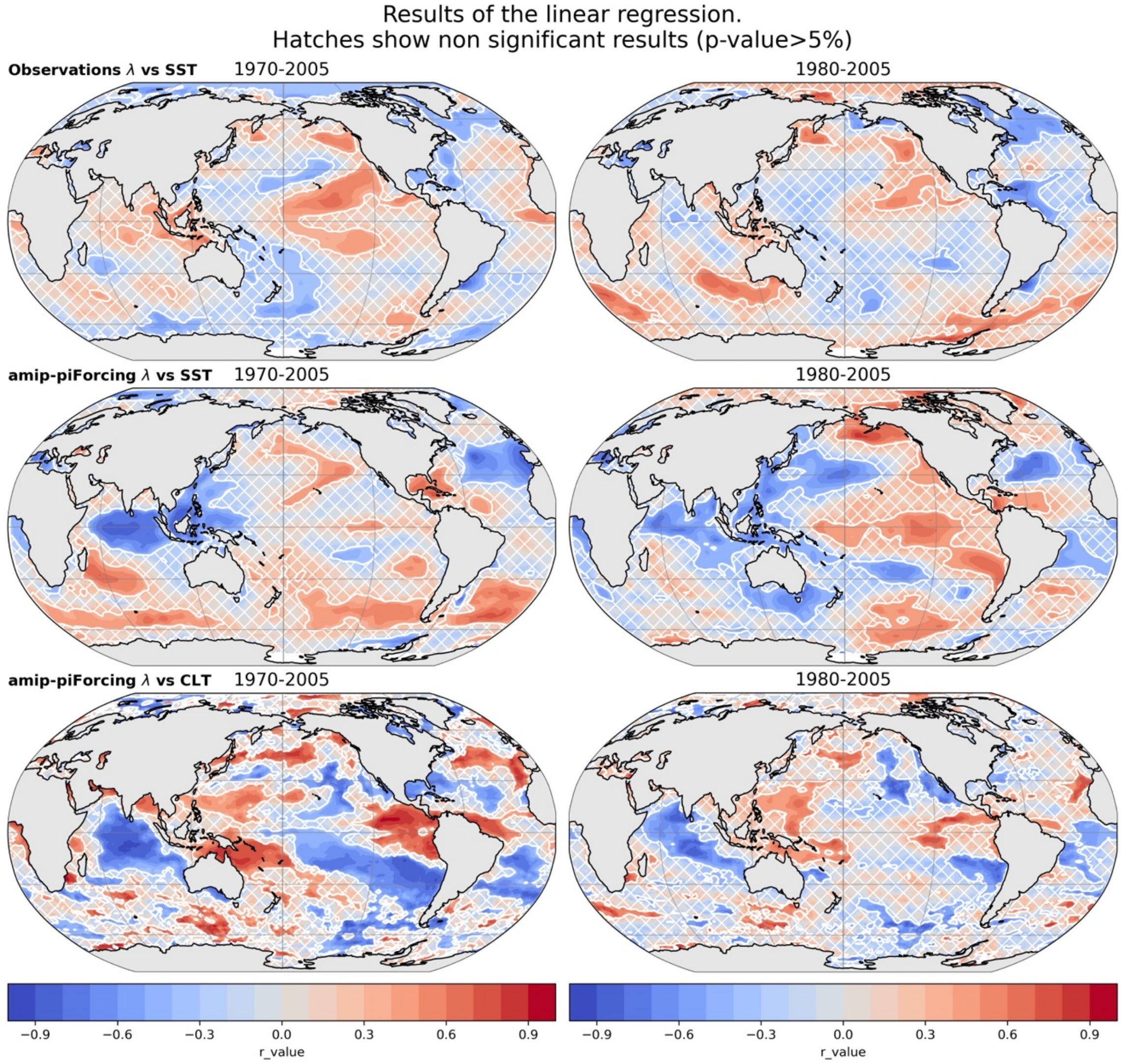

Figure8 : same as fig 5,6 and 7 but with a 5 yr smoothing for the input data used for the computation of lambda and a 5 yr smoothing on the SS variationsT, and the cloud cover variations

Minor comments:

Timescales and internal variability:

Proistosescu et al. 2017 and Gregory et al. 2020 discuss a phenomenon called “regression dilution” — making feedback estimates which include (predominantly) internal variability too positive. Meyssignac et al. uses ten year low pass filtered time series to begin with. Does that mean the regression dilution is not occurring here? Are 25-years enough to speak of a feedback or are we still seeing internal variability?

That is true, there is some regression dilution as we regress R against T of each realization without filtering for the noise in GSAT which does not produce radiative response in the form λT. The regression dilution generates a bias and a dispersion around the true lambda. We did not estimate the lambda of the ensemble-mean realization so we did not estimate the bias generated by regression dilution and we did not correct for it. However, we estimated the uncertainty range in lambda with the spread in lambda across different AGCM historical realizations (which show different realization of the noise in GSAT which does not produce radiative response in the form λT) (See Chenal et al. 2022 for more details on the method). So our computation of the uncertainty includes the uncertainty due to the dispersion of lambda generated by the regression dilution.

our study focuses on the relative time variations of lambda which is insensitive to the mean bias. Thus, although our lambda estimates may all be biased by the same amount because of the regression dilution, the relative time variations of lambda are correct and the uncertainty induced by regression dilution on the time variations of lambda is accounted for in the uncertainty range.
We added a paragraph in the mehods to indicate these aspect. See line 1159-1177

What kept the world stable between 1980 and 1990 when the feedback parameter was 0.5Wm-2 (with some probability above zero)?
The probability of lambda above zero does not represent a probability that physically the climate could become unstable. It represents a probability that, the combination of observations, which are uncertain, can lead in some cases to estimates of lambda that are positive. Although these observational estimates of positive lambda are obviously wrong (indeed If the climate were unstable then surface temperature would quickly drift away which did not happen over 1980-1990!), from the perspective of the observing system they are consistent with the uncertainty of our measurement of the energy budget. It shows that our uncertainty estimate could be further improved .

line 45 (? lines are not actually aligned, so take the line numbers as tentative): “equilibrium climate sensitivity that is 26% higher than inferred from observations” — one example where using “effective climate sensitivity” would be much more precise. There is literature showing that the actual equilibrium sensitivity is another 15% or so higher than the effective (150-yr regression based) one.

Corrected.

line 66/67 This makes it sound there is only internal variability. What about the forced pattern? What if what we observe is already forced?

Here we only talk about the response of lambda to PDO. And we argue that if the PDO is shifted then the response of lambda to PDO can be expected to shift as well leading to faster warming than over the past decade. Some could argue that the PDO itself is actually changed by the forcing and thus there is a forced response in the PDO. That is possible but this is probably a second order signal compared to the shift in PDO due to internal variability. Indeed climate models show hardly any significant changes in PDO or ENSO due to the forcing in the coming decades.

line 130-134 “if this decrease …. close to equilibrium is confirmed … “ Given substantial literature on this, my sense is that this debate is settled (within the models) and doesn’t need an “if”.

I agree the debate is settled within models but it is not confirmed in the real world. The “if” was refering to this aspect. In order to stick to model literature and to avoid confusion we follow your advice and remove the “if”.

line 139 equilibrium —> effective

corrected. Thanks

line 142 “as the extent to which GCMs can simulated the observed pattern in SST” — is pretty well analyzed, see e.g. Seager et al. 2019 https://www.nature.com/articles/s41558-019-0505-x, Seager et al. 2022 https://journals.ametsoc.org/view/journals/clim/35/14/JCLI-D-21-0648.1.xml, Olonscheck et al. 2020 https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019GL086773

That is true we changed the sentence to explain that it is the the extent to which GCMs can simulate observed lambda variations on decades that has not been evaluated so far

line 238 Is is clear that the response to volcanoes is due to the pattern effect? How much of the net effect is forcing? This is not the subject of the paper here, but the wording makes it sound as if the entire radiative response to volcanoes comes about through the pattern effect, i.e. through the surface, and not (e.g. the spatial structure?) of the forcing.

No of course there is a significant response in global mean TAS to volcanic forcing. It is just that this response is temporary and relatively small compared to other forcing that are in general long lasting. We reformulate to avoid confusion.

Around Fig.1
(I might have missed this) How is the base state defined across the different data sources? The EEI is zero in 1957, the FNA is 1Wm-2, the FAER is -0.4Wm-2 and T is 0.2K —?

Why is the uncertainty in FAER increasing with time (I'd expect the opposite) and why is the uncertainty not centered around the median or best estimate?

We are using a regresion method so there are no base state. We regress variations in N against variations in T. It is one of the strength of our approach compared to the classical state difference approach (see Chenal et al. 2022 JCLIM for more details). In the new version of the manuscript we use the ERF of AR6 now and the associated uncertainty so the question is not relevant anymore. Anyway if reviewer 3 wants more detail on the uncertainty in FAER of the old version of the manuscript please se Chenal et al. 2022

Around Fig.2:
Awesome figure! It visualized so clearly how dependent feedback calculations depends on the time and length. Truncating or rounding the numbers to one or two digits would make it a little bit easier to read off the numbers.

Corrected. Thank you for the nice comment

Around Fig.3:
Ideally the differences between a and b (with and without the volcanic correction) could be discussed a bit more. By eye, it seems like the corrected version has smaller feedback values — already in the 1980ies "feeling" Pinatubo already? How meaningful is the strong smoothing when discussing the 3-yr-ish timescale for the volcanoes?

Indeed Pinatubo was in 1991. Thus, all time-windows centered in 1980 include the Pinatubo eruption as they have a length >25 years. It means that time series are already impacted in 1980 by the Pinatubo volcanic eruption.
Although we are applying a strong smoothing to raise the signal to noise ratio, the removal of 3yars from volcanic eruption is meaningful because during volcanic eruptions lambda is systematically more negative and tend to bias the regressions whatever the smoothing

line 750 ?????

Corrected.

line 786-915: I have no expertise in judging the validity of these choices. Is this treatment done for specifically this paper or does it refer to a more generally used dataset which might be documented and available online?

The in situ dataset is available online (please see Chenal et al. 2022). The Aran dataset is also available online. Please see (Bagnell and DeVries 2021). The planetary heat uptake derived from the global sea level budget has been specifically developed for this study. Planetary heat uptake derived from the sea level budget has been proposed earlier (Dieng et al. 2017, Meyssignac et al.2019, Hakuba et al. 2021 and Marti et al. 2022). Here for the first time we apply the method to the period 1954-2020.

line 794 The ocean

Corrected

line 997 for these short timescales an orthogonal regression might be more meaningful than an ordinary least square?
The problem with orthogonal regressions is that in general if the uncertainty ranges of the predictand (here GSAT) are not provided with a precise and exhaustive var/covariance matrix they do not converge. And when the uncertainty range is too large the results are hard to interpret and inconclusive

20th Feb 23

Dear Dr Meyssignac,

First of all, please allow us to apologies for the delay in sending a decision on your revised manuscript titled "Historical observations of the Earth energy budget show the time-variations of the climate feedback parameter are associated with the Pacific Decadal Oscillation". It has now been seen by the original 3 reviewers, and I include their comments at the end of this message. They acknowledge improvements in revision, but continue to raise some important points. We are still interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to the remaining concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript once again, along with a point-by-point response that takes into account all the points raised by the reviewers and meets our editorial thresholds as outlined below:

1. Discuss which insights can be gained from the different methods used to define feedbacks, and what we can learn from the feedback variations in Andrews et al.'s studies in 2018 and 2022

2. More recent data up to 2022 should be used to account for changes in the PDO sign around 1999/2000 and 2016.

Please highlight all changes in the manuscript text file.

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) and the completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me 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,

Min-Hui Lo, PhD

Editor Board Member

Communications Earth & Environment

Heike Langenberg, PhD

Chief Editor

Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements (Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:

Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

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:

- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)

- Accession codes where appropriate

- If applicable, a statement regarding data available with restrictions

- 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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If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

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

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Reviewer: Shiv Priyam Raghuraman

I thank the authors for their time and effort in addressing my previous comments. I have some major comments and a series of minor comments that should be addressed prior to publication.

Major Comments:

1. The assertion in the abstract (lines 72-73) and toward the end of the paper (lines 334-339) that the PDO shifting phase will bring about enhanced warming is quite speculative. Since late 2017 the PDO has been in a negative phase, in fact reaching minimums not seen since the 1950s (https://www.ncei.noaa.gov/access/monitoring/pdo/), yet GMST and EEI have both increased consistently in the last 5+ years. So, there are other factors at play outside of PDO. I suggest deleting these lines or substantiate this claim more.

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readers unfamiliar with these modes of variability should have a sense of how the indices themselves look spatially (over the same time period as Figure 4).

3. Please highlight in the text more clearly that the North-South Pacific pattern within the tropics as well as extratropics is also important, not just the East-West pattern; it is a nice result. Moreover, it bridges some gaps with previous work that showed a poor correlation between E-W SST gradient and tropical TOA radiative changes at decadal time scales (mentioned in my previous review (Minor Comment #4); https://doi.org/10.1038/s41467-021-24544-4 Supplementary Figures 10-11, Table S6).

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Reviewer #3 (Remarks to the Author):

Review of “Historical observations of the Earth energy budget show the climate feedback parameter varies with time in response to the Pacific Decadal Oscillation” by Meyssignac et al.; second round

I appreciate the authors' efforts to address my comments and think the new Fig.3 helps to connect the global mean values with the argument that variations in lambda are driven by PDO/IPO. I still have two major concerns and do not get the sense yet that the paper thoroughly presents evidence to support their hypothesis.

In my first major comment, the authors did not address the main questions:

Which method (of defining feedbacks) yields more insights? In other words, what do we learn from the feedback variations in Andrews et al. 2018/2022 (models agree in the evolution towards more negative values since the 50ies or so and tend to flatten over the last couple of years) and what from the current analysis based on 40 year window feedback definitions (models mostly did not change their feedback strength since the 50ies; in the observations, the feedback strength increases between the 50ies and 90ies and then decreases). To me these are (seemingly?) conflicting findings. If you used a smaller window size to define feedbacks, then the models would not look as bad compared to the observations (presumably they would do the same as Andrews et al., namely becoming more negative with time?).

I do not fully follow the new Fig.3. The caption says that the correlation is done between a *changing* lambda and the SST. So for the observations the changing lambda is something like 2W/m^-2 (difference between 1970 and 2005) and changes sign within that time period. What is shown in Fig.3a exactly? Is the correlation done with the absolute lambda values but then moved through that time window (which is less than 40 years to begin with so there are actually very few changes in lambda possible?) and then the change of this over time is averaged and shown here? Or is the correlation done with the actual change in lambda?

For the models the change in lambda over that time is 0.1Wm^-2K^-1 or less — how can anything be regressed against that non-existing change?

How representative is the HadGEM model of the other models? Why not showing the mean of all the models?

Circling back to Andrews et al. 2022, for their feedback definitions the spread between model was as large as the feedback changes themselves (0.5-1Wm^-2K^-1 or so) - implying the models differ quite a bit in their ability/sensitivity to explain that pattern effect?

My last and major concern around time is that the observed PDO changed sign around the year 1999/2000 and around 2016. Both these switches are not covered in the paper (because of the 40 year time window and also because the end date is not 2022 but 2005). It seems like the perfect test of the hypothesis to switch to shorter timescales and show that past 2005 we would switch back to a less negative feedback parameter. I do not understand how the most current 20 years can be neglected in the entire analysis.

Minor comments:

What is the role of the Southern Ocean ice and surface temperature problems in the analysis around Fig.3 lower panels? There are known biases in the forcing files of AMIP (see e.g. the discussion in Andrews et al. 2018 (supplement) or Andrews et al. 2022 (main paper), or Lewis and Mauritzen 2020). For other applications the Southern Ocean is just ignored (I think e.g. in Raghuraman et al. 2021).

I mentioned that the difference between the estimates of ECS using the historical time period and 150 year simulations is 26% but that additional 15% (varies among models) change towards the equilibrium climate sensitivity (difference between EffCS and ECS). The "rebuttal" document mentioned "corrected" but the actual paper text is not (e.g. line 50 and line 150).

In the comment about the PDO/IPO pattern potentially not being an imprint of internal variability but the forced response, I was not clear enough, I guess. I did not mean the PDO would change already in magnitude due to climate change (maybe that's expected towards the end of the 21st century, I agree that this is definitely a second order effect according to our current knowledge). There are papers/people arguing that the PDO-like trend pattern we observe over the last 40 years is an imprint of the forced response already. See discussion e.g., in Liu et al. 2018 (DOI: 10.1175/JCLI-D-17-0462.1) "most excitable mode" or Heede et al. 2020 (DOI: 10.1175/JCLI-D-19-0690.1) in which the East Pacific cooling trend is discussed as initial part of a forced response.

In summary, I think the hypothesis is valid and the new version of the manuscript supports also evidence for the hypothesis. In addition, extending the feedback analysis back is novel and worth reporting. I find the timescales very hard to interpret and to link to the existing literature which leads to seemingly different findings (mostly with Andrews et al. 2022). At minimum these have to be explained clearly (meaning, the implication of them, not just showing that the method to compute feedbacks is compatible). I also think the observed record should be used until 2022, even thought the CERES time is discussed elsewhere already it would allow to connect the findings of Meyssignac et al. to the existing literature and serve as a test bed for the PDO hypothesis because the feedback evolution should change again between 2005 or 2010 onward. Hence, I suggest major revisions.

REVIEWER COMMENTS:

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Reviewer: Shiv Priyam Raghuraman

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Actually we think it is phases made of long periods with positive or negative PDO that lead to changes in lambda. Indeed, lambda is a variable which integrates the relationship between the radiative response and the SST over 25 years; If over 25 years the PDO shifts successively from positive to negative values several times then its effect tends to average out in lambda. Since 1999 the PDO has indeed shifted already 5 times which is probably the reason why we don't see important changes in lambda from 2000 to 2005 on Fig 3a. However, over 1957 to 1978 the PDO has been almost always negative and over 1978 to 1998 it has been almost always positive. This is probably the reason why lambda shows an important change between 1975-1985 and 1985- 2000

Having said that we agree the sentence about the change in PDO is speculative and we remove it from the abstract. In the end of the paper we changed the sentence to explain our point above. Now the sentence at the end of the paper reads: "This result has consequences for the decadal variability of climate. If confirmed, it means that when the PDO switches to a long positive (resp negative) phase (as it happened in 1976 when the PDO switched for a positive phase until 1998) , the SST pattern switches as well and the negative (resp positive) feedback due to SST pattern changes, leading to rapid enhanced (resp damped) warming for several decades. »

2. Thank you for looking into the correlation with other modes of variability. Could you plot (perhaps in Figure 4) the PDO and IPO patterns separately? At present, there is only a reference to Figure 4 where the correlation between lambda and SSTs are supposed to guide the reader on the PDO/IPO pattern. But readers unfamiliar with these modes of variability should have a sense of how the indices themselves look spatially (over the same time period as Figure 4).

The IPO is the Pacific-wide manifestation of the Pacific Decadal Oscillation of Mantua et al (1997). It shows precisely the same time variations as the PDO but since it includes the southern part of the Pacific in its pattern it shows in the southern Pacific the SST that covariates with the PDO. This is evident information for people interested in climate mode and iPO and PDO have been compared many times in the literature (see for example Folland CK, Parker DE, Colman AW, Washington R. 1999. Large scale modes of ocean surface temperature since the latenineteenth century. InBeyond El Nin˜o:Decadal and Interdecadal Climate Variability, Navarra A (ed.). Springer: Berlin; 73–102).
So we feel that adding these details on the relation between PDO and iPO will just overwhelm the reader; It brings in confusion without improving the paper; so we propose to remove the reference to the IPO in the paper. We choose to remove the refeence to IPO rather than to PDO because IPO is less reknown than the PDO out of the climate mode's community.
In the new version of the manuscript we remove the references to IPO;

3. Please highlight in the text more clearly that the North-South Pacific pattern within the tropics as well as extratropics is also important, not just the East-West pattern; it is a nice result. Moreover, it bridges some gaps with previous work that showed a poor correlation between E-W SST gradient and tropical TOA radiative changes at decadal time scales (mentioned in my previous review (Minor Comment #4); https://doi.org/10.1038/s41467-021-24544-4 Supplementary Figures 10-11, Table S6).

This is done (l é99) in the new version of the manuscript. Thanks

Minor Comments:

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Please beware here that the no filtering and 5 yrs filtering time series are filled with noise coming from instrumental noise and interannual internal variability that is unrelated between SST and the radiative response of the Earth. In addition, for these time series the correlation is not significant (see the p values). So you should not trust these higher correlations of lambda with SAM.

2. Figure 3b: Which climate indices? PDO? IPO? Please use $\tilde{\lambda}$ in the figures too as done in the text.

Corrected thanks

3. The references need to be formatted appropriately throughout the text. In various instances the parentheses are missing (e.g., lines 96, 115, etc.). Please also check with the journal if they prefer alphabetic ordering of references in the text (e.g. line 151 I would expect Armour 2017 to be first).

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Corrected thanks

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Corrected thanks

7. Line 250: what does "CL" mean?

Confidence level, Corrected thanks

8. Line 323: delete "On the".

Corrected thanks

9. Lines 330-331: as mentioned in my last review (Minor Comment #5), these studies did not look at PDO's impact on global cloud feedback, only the Pacific SST pattern. Please revise.

Indeed in this study we do not have long cloud data to estimate the cloud feedback. We clarify now tha the conclusion on cloudfeedback comes from previous studies based on climate models

Now the sentence reads

« This line of evidence combined with previous evidence from climate model studies that the PDO modulates the global mean cloud feedback (Zhou et al, 2016, 2017; Andrews and Webb, 2018; Ceppi and Gregory, 2017; Fueglistaler, 2019; Dessler, 2020) suggest that at multidecadal time scales the primary driver for the pattern effect is actually the PDO »

10. Line 351: what does the >40 years refer to? The time period length? Please elaborate. I thought the results were robust to changing the time period window/length?

It refers to windows of 40 years length and above

Now It is clarified

11. Line 359: wrong Figure reference? Fig. 2a doesn't have AGCMS.

Corrected thanks

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We thank reviewer 2 for his positive comment

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We think reviewer 3 misunderstood our answer to his first major comment. In short, the climate feedback parameter as estimated by Andrews et al. (2022) with AMIP simulations (Fig 6a in the 2022 paper) is in agreement with our estimate of the climate feedback parameter from AMIP simulations of fig 3.a. This is confirmed by fig 1 below which shows that both estimates of lambda have the same decadal variations over their common period 1970-2005. These decadal variations over 1970-2005are basically flat and slightly decreasing. Fig 1 means that our definition of the feedback is consistent with Andrews et al. 2022 definition.
Now when we apply our definition of the feedback (which is consistent with Andrews et al.) on observations rather than on AMIP simulations, we find that lambda shows significant decadal variations over period 1970-2005 (see Fig 3 in the main text). This means that lambda estimated from AMIP (either with our definition or with Andrews et al. which are both consistent) differs significantly from lambda estimated with observations

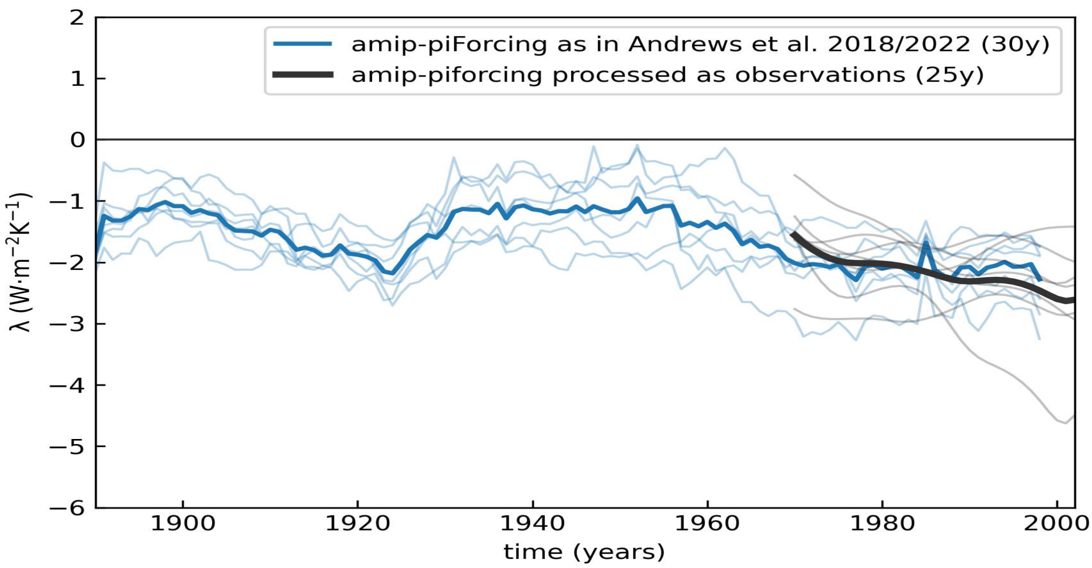

Figure 1: Blue lines: Lambda estimated from AMIP simulations over 30-year windows (as in Andrews et al. 2018 and 2022 their Fig 6a). Black lines: Lambda estimated from the same AMIP simulations over 25-year windows and with T EEI and F time series, low-pass filtered with a cutoff period at 15 years (as in Fig 3a of the main text). Bold lines indicate the ensemble mean;

The conflicting result here is between observations and AMIP simulations but not between Andrews et al. (2018/2022) and our manuscript. We believe reviewer 3 did not realize that our fig3.A only represents the variations of the climate feedback parameter over the 70s, 80s, 90s and 2000s (we don’t estimate lambda over the 50s or 60s because we don’t have sufficient data back in time). Probably reviewer 3 did not realize as well that over the period 1970-1995 (the common period between Andrews et al. and our manuscript) both Andrews et al. estimate of lambda and ours' show variations that are flat on decadal time scales.

Actually the consistency between Andrews et al's definition of the global climate feedback and ours' was expected , because the difference between Andrews et al. calculation of the global climate feedback parameter and ours' is small. In total There are only 3 small differences in the calculation. Of both global climate feedback:

1. Andrews et al. uses a 30 year window while we use a 25 year window
2. Andrews et al. compute the climate feedback parameter time series over the period 1885-1995 because they use AMIP simulations which provide data over 1870-2010 and they use 30 years windows. On our side, we compute the climate feedback parameter time series over the period 1970-2005 because we use observation data over 1957-2017 and we use a 25 year window
3. We smooth the data with low pass filter and a cutoff period at 15 yrs, before computing the climate feedback parameter to remove interannual variability that is unrelated to the forced response of climate in the observations

The consequence of these small differences is actually very limited. It affects only the interannual variability of lambda. In Fig.1 we plot lambda estimates computed as in Andrews et al. 2022 (their figure 6a) and computed as in our fig3a. They both agree well on decadal time scales. This shows that both methods of defining feedback yield the same insights on decadal time scales.

We suspect the confusion of reviewer 3 was coming from the statement line 349 "We find that, on long time scales (i.e. for windows of 40years length and above, see Fig 3c), AGCMs estimates of λ(t) are consistent across models (confirming earlier results from Andrews et al (2022)) and are consistent with observations despite a small bias (see Fig. 3c)"
We agree this statement is confusing because we are refering to estimates of λ(t) over 40 yr windows and we are comparing them with Andrews et al; who works with 30 yr windows.We now remove this reference to Andrews et al. to avoid confusion

I do not fully follow the new Fig.3. The caption says that the correlation is done between a *changing* lambda and the SST. So for the observations the changing lambda is something like 2W/m^-2 (difference between 1970 and 2005) and changes sign within that time period. What is shown in Fig.3a exactly? Is the correlation done with the absolute lambda values but then moved through that time window (which is less than 40 years to begin with so there are actually very few changes in lambda possible?) and then the change of this over time is averaged and shown here? Or is the correlation done with the actual change in lambda?
For the models the change in lambda over that time is 0.1Wm^-2K^-1 or less — how can anything be regressed against that non-existing change?

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So there are 2 findings in this paper that concern AMIP simulations:

1- We find that AMIp simulations of lambda show very small variations in lambda at decadal time scales since 1970 while observations show large variations
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We clarify the caption in the new version of the manuscript to clarify its content

How representative is the HadGEM model of the other models? Why not showing the mean of all the models? Circling back to Andrews et al. 2022, for their feedback definitions the spread between model was as large as the feedback changes themselves (0.5-1Wm^-2K^-1 or so) - implying the models differ quite a bit in their ability/sensitivity to explain that pattern effect?

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My last and major concern around time is that the observed PDO changed sign around the year 1999/2000 and around 2016. Both these switches are not covered in the paper (because of the 40 year time window and also because the end date is not 2022 but 2005). It seems like the perfect test of the hypothesis to switch to shorter timescales and show that past 2005 we would switch back to a less negative feedback parameter. I do not understand how the most current 20 years can be neglected in the entire analysis.

There is a misunderstanding here, we did not neglect the current 20 years in our study; The data that we use starts in 1957 and ends in 2017 (there is a problem with Argo salinity since 2017 that prevents from using more recent data; the sea level reconstructions are not available after 2017 as well). Note that, although we use data from 1957 to 2017, we are able to compute lambda only from 1970 to 2005 because each lambda estimate is computed over a 25 year window. So strictly speaking the switch in PDO in 1999/2000 and in 2016 are included in our record's latest values of lambda. However we don't see much effect on lambda for 2 reasons:

1- First because these switches are late in the record so they only account in the latest values of lambda. In addition, in the case of the 2016 shift, it accounts only marginally because it is on the edge of the most recent 25 year window.
2- Second because lambda is a variable which integrates the relationship between the radiative response and the SST over 25 years; If over 25 years the PDO shifts from positive to negative values several times then its effect tends to average out in lambda. Since 1999 the PDO has indeed shifted already 5 times which is probably the reason why we don't see important changes in lambda from 2000 to 2005. However over 1960 to 1976 the PDO has been almost always negative and over 1976 to 1998 it has been almost always positive. This is probably the reason why lambda shows an important change around 1976

Minor comments:

What is the role of the Southern Ocean ice and surface temperature problems in the analysis around Fig.3 lower panels? There are known biases in the forcing files of AMIP (see e.g. the discussion in Andrews et al. 2018 (supplement) or Andrews et al. 2022 (main paper), or Lewis and Mauritzen 2020). For other applications the Southern Ocean is just ignored (I think e.g. in Raghuraman et al. 2021).

That is a good question. For sure the sea ice variability in not properly simulated in AOGCM as the ocean and the atmosphere are not coupled to the Antarctica ice sheet although the antarctica ice sheet plays a key role in sea ice formation in the southern ocean ; This problem may lead to spurious SST in the southern ocean and thus spurious pattern effect. It could definitely explain an important part of the discrepancy between AMIP-piforcing simulation and observations of the time variations in lambda

I mentioned that the difference between the estimates of ECS using the historical time period and 150 year simulations is 26% but that additional 15% (varies among models) change towards the equilibrium climate sensitivity (difference between EffCS and ECS). The "rebuttal" document mentioned "corrected" but the actual paper text is not (e.g. line 50 and line 150).

Indeed. We don't know how it happened; It should have been corrected. Anyhow in the new version of the manuscript we shortened the abstract to respect nature guidelines and this sentence was removed

In the comment about the PDO/IPO pattern potentially not being an imprint of internal variability but the forced response, I was not clear enough, I guess. I did not mean the PDO would change already in magnitude due to climate change (maybe that's expected towards the end of the 21st century, I agree that this is definitely a second order effect according to our current knowledge). There are papers/people arguing that the PDO-like trend pattern we observe over the last 40 years is an imprint of the forced response already. See discussion e.g., in Liu et al. 2018 (DOI: 10.1175/JCLI-D-17-0462.1) "most excitable mode" or Heede et al. 2020 (DOI: 10.1175/JCLI-D-19-0690.1) in which the East Pacific cooling trend is discussed as initial part of a forced response.

Indeed, we did not understand the comment about the PDO/IPO in the first round of revision; Now it is much clearer thank you.
We agree with reviewer 3 such a forced response with a PDO-like trend pattern cannot be ruled out and should be acknowledge here. However, In the new version of the manuscript the sentence on line 66-68 has been removed because reviewer 1 argued it was too speculative (which is quite true actually)

In summary, I think the hypothesis is valid and the new version of the manuscript supports also evidence for the hypothesis. In addition, extending the feedback analysis back is novel and worth reporting. I find the timescales very hard to interpret and to link to the existing literature which leads to seemingly different findings (mostly with Andrews et al. 2022). At minimum these have to be explained clearly (meaning, the implication of them, not just showing that the method to compute feedbacks is compatible). I also think the observed record should be used until 2022, even thought the CERES time is discussed elsewhere already it would allow to connect the findings of Meyssignac et al. to the existing literature and serve as a test bed for the PDO hypothesis because the feedback evolution should change again between 2005 or 2010 onward. Hence, I suggest major revisions.

We hope that our point concerning the past literature (in particular Andrew et al. 2022) is clear now. In summary, our definition of lambda, when taken over 25 year windows (I.e. as in Fig 3 a and 3b), is very close and consistent with Andrews et al. (2022)'s definition. Indeed, when we apply our definition of lambda and Andrews et al.'s definition on the same AMIP simulations we find very similar decadal variability (Fig 1 above). Now we agree that when we apply our definition of lambda to observations then we find a different decadal variability than in AMIP simulations. We propose some insights in the last paragraph of the main text to explain these discrepancies between observed lambda and AMIP lambda. We acknowledge these insights are not enough to understand the causes for these discrepancies. But it is beyond the scope of the present study to further. We argue at the end of the main text this question call for more research.

As the record we use, unfortunately we can go beyond 2017 yet because reconstructions of sea level are not yet available after 2017 and there is an issue in Argo data that need to be solved after 2017. For sure in the future when the data will be available we intend to update our estimate of the variations in lambda.

24th Apr 23

Dear Dr Meyssignac,

We have now carefully assessed your revised manuscript titled "Historical observations of the Earth energy budget show the time-variations of the climate feedback parameter are associated with the Pacific Decadal Oscillation" along with your responses to the reviewer reports, and in light of this information we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to 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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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Min-Hui Lo, PhD

Editor Board Member

Communications Earth & Environment

Heike Langenberg, PhD

Chief Editor

Communications Earth & Environment

On Twitter: @CommsEarth

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Reviewer: Shiv Priyam Raghuraman

I thank the authors for their time and effort in addressing my previous comments. I have some major comments and a series of minor comments that should be addressed prior to publication.

Major Comments:
1. The assertion in the abstract (lines 72-73) and toward the end of the paper (lines 334-339) that the PDO shifting phase will bring about enhanced warming is quite speculative. Since late 2017 the PDO has been in a negative phase, in fact reaching minimums not seen since the 1950s (https://www.ncei.noaa.gov/access/monitoring/pdo/), yet GMST and EEI have both increased consistently in the last 5+ years. So, there are other factors at play outside of PDO. I suggest deleting these lines or substantiate this claim more.

Actually we think it is phases made of long periods with positive or negative PDO that lead to changes in lambda. Indeed, lambda is a variable which integrates the relationship between the radiative response and the SST over 25 years; If over 25 years the PDO shifts successively from positive to negative values several times then its effect tends to average out in lambda. Since 1999 the PDO has indeed shifted already 5 times which is probably the reason why we don't see important changes in lambda from 2000 to 2005 on Fig 3a. However, over 1957 to 1978 the PDO has been almost always negative and over 1978 to 1998 it has been almost always positive. This is probably the reason why lambda shows an important change between 1975-1985 and 1985- 2000

Having said that we agree the sentence about the change in PDO is speculative and we remove it from the abstract. In the end of the paper we changed the sentence to explain our point above. Now the sentence at the end of the paper reads: "This result has consequences for the decadal variability of climate. If confirmed, it means that when the PDO switches to a long positive (resp negative) phase (as it happened in 1976 when the PDO switched for a positive phase until 1998) , the SST pattern switches as well and the negative (resp positive) feedback due to SST pattern changes, leading to rapid enhanced (resp damped) warming for several decades. »

2. Thank you for looking into the correlation with other modes of variability. Could you plot (perhaps in Figure 4) the PDO and IPO patterns separately? At present, there is only a reference to Figure 4 where the correlation between lambda and SSTs are supposed to guide the reader on the PDO/IPO pattern. But readers unfamiliar with these modes of variability should have a sense of how the indices themselves look spatially (over the same time period as Figure 4).

The IPO is the Pacific-wide manifestation of the Pacific Decadal Oscillation of Mantua et al (1997). It shows precisely the same time variations as the PDO but since it includes the southern part of the Pacific in its pattern it shows in the southern Pacific the SST that covariates with the PDO. This is evident information for people interested in climate mode and iPO and PDO have been compared many times in the literature (see for example Folland CK, Parker DE, Colman AW, Washington R. 1999. Large scale modes of ocean surface temperature since the latenineteenth century. InBeyond El Nin˜o:Decadal and Interdecadal Climate Variability, Navarra A (ed.). Springer: Berlin; 73–102).
So we feel that adding these details on the relation between PDO and iPO will just overwhelm the reader; It brings in confusion without improving the paper; so we propose to remove the reference to the IPO in the paper. We choose to remove the refeence to IPO rather than to PDO because IPO is less reknown than the PDO out of the climate mode's community.
In the new version of the manuscript we remove the references to IPO;

3. Please highlight in the text more clearly that the North-South Pacific pattern within the tropics as well as extratropics is also important, not just the East-West pattern; it is a nice result. Moreover, it bridges some gaps with previous work that showed a poor correlation between E-W SST gradient and tropical TOA radiative changes at decadal time scales (mentioned in my previous review (Minor Comment #4); https://doi.org/10.1038/s41467-021-24544-4 Supplementary Figures 10-11, Table S6).

This is done (l é99) in the new version of the manuscript. Thanks

Minor Comments:

1. I was surprised to see a higher correlation of lambda with SAM than with PDO for the no filtering and 5 yrs filtering cases. Since your results show an impact from the extratropics could you test if SAM and PDO can together (rather than individually as you have done) have a better correlation with lambda (using multilinear regression or some sort of statistical tools/tests)?
Please beware here that the no filtering and 5 yrs filtering time series are filled with noise coming from instrumental noise and interannual internal variability that is unrelated between SST and the radiative response of the Earth. In addition, for these time series the correlation is not significant (see the p values). So you should not trust these higher correlations of lambda with SAM.

2. Figure 3b: Which climate indices? PDO? IPO? Please use $\tilde{\lambda}$ in the figures too as done in the text.
Corrected thanks

3. The references need to be formatted appropriately throughout the text. In various instances the parentheses are missing (e.g., lines 96, 115, etc.). Please also check with the journal if they prefer alphabetic ordering of references in the text (e.g. line 151 I would expect Armour 2017 to be first).
Corrected thanks

4. Left quotation marks need to be fixed throughout the text (e.g., lines 53, 256, etc.). Use `` instead of " in LaTeX.
Corrected thanks

5. Line 44: should be "raises"
Corrected thanks

6. Line 234: "certainly" is a strong word. Please rephrase.
Corrected thanks

7. Line 250: what does "CL" mean?
Confidence level, Corrected thanks

8. Line 323: delete "On the".
Corrected thanks

9. Lines 330-331: as mentioned in my last review (Minor Comment #5), these studies did not look at PDO's impact on global cloud feedback, only the Pacific SST pattern. Please revise.

Indeed in this study we do not have long cloud data to estimate the cloud feedback. We clarify now tha the conclusion on cloudfeedback comes from previous studies based on climate models
Now the sentence reads
« This line of evidence combined with previous evidence from climate model studies that the PDO modulates the global mean cloud feedback (Zhou et al, 2016, 2017; Andrews and Webb, 2018; Ceppi and Gregory, 2017; Fueglistaler, 2019; Dessler, 2020) suggest that at multidecadal time scales the primary driver for the pattern effect is actually the PDO »

10. Line 351: what does the >40 years refer to? The time period length? Please elaborate. I thought the results were robust to changing the time period window/length?
It refers to windows of 40 years length and above
Now It is clarified

11. Line 359: wrong Figure reference? Fig. 2a doesn't have AGCMS.
Corrected thanks

12. Lines 358-420: this is a very long paragraph (it spans 3 pages). Please break it up into smaller paragraphs.
Corrected thanks

Reviewer #2 (Remarks to the Author):

The authors have addressed all my comments. Now the GCM and observations are more consistent than previous draft in Figure 3, and the authors conclude that the mean climate feedback parameter reproduced by GCMs is consistent with observations. The paper might be accepted for publication now.
We thank reviewer 2 for his positive comment

Reviewer #3 (Remarks to the Author):

Review of “Historical observations of the Earth energy budget show the climate feedback parameter varies with time in response to the Pacific Decadal Oscillation” by Meyssignac et al.; second round

I appreciate the authors' efforts to address my comments and think the new Fig.3 helps to connect the global mean values with the argument that variations in lambda are driven by PDO/IPO. I still have two major concerns and do not get the sense yet that the paper thoroughly presents evidence to support their hypothesis.

In my first major comment, the authors did not address the main questions:
Which method (of defining feedbacks) yields more insights? In other words, what do we learn from the feedback variations in Andrews et al. 2018/2022 (models agree in the evolution towards more negative values since the 50ies or so and tend to flatten over the last couple of years) and what from the current analysis based on 40 year window feedback definitions (models mostly did not change their feedback strength since the 50ies; in the observations, the feedback strength increases between the 50ies and 90ies and then decreases). To me these are (seemingly?) conflicting findings. If you used a smaller window size to define feedbacks, then the models would not look as bad compared to the observations (presumably they would do the same as Andrews et al., namely becoming more negative with time?).

We think reviewer 3 misunderstood our answer to his first major comment. In short, the climate feedback parameter as estimated by Andrews et al. (2022) with AMIP simulations (Fig 6a in the 2022 paper) is in agreement with our estimate of the climate feedback parameter from AMIP simulations of fig 3.a. This is confirmed by fig 1 below which shows that both estimates of lambda have the same decadal variations over their common period 1970-2005. These decadal variations over 1970-2005are basically flat and slightly decreasing. Fig 1 means that our definition of the feedback is consistent with Andrews et al. 2022 definition.
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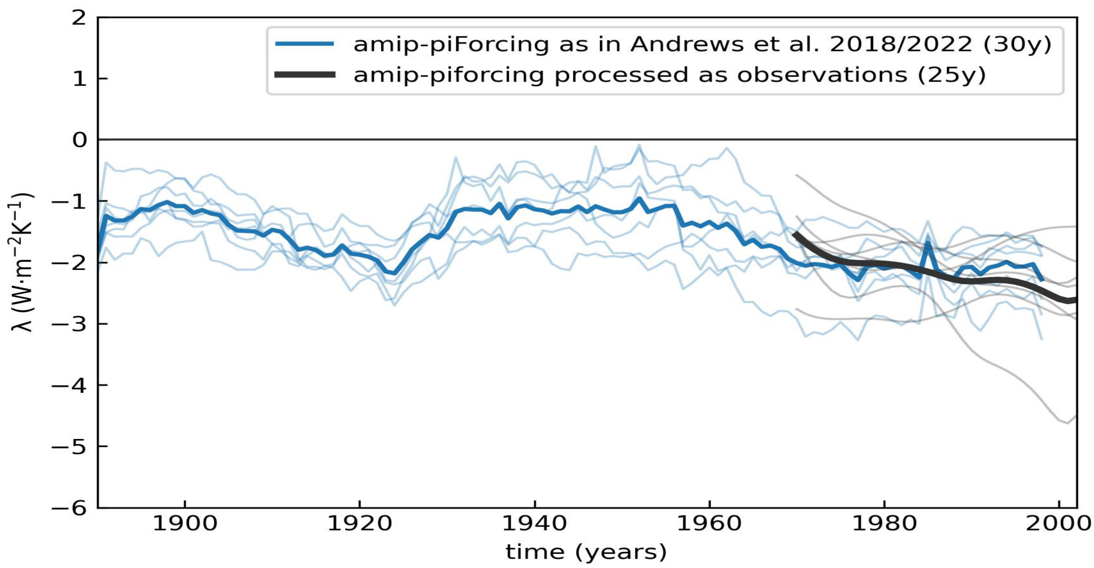

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I do not fully follow the new Fig.3. The caption says that the correlation is done between a *changing* lambda and the SST. So for the observations the changing lambda is something like 2W/m^-2 (difference between 1970 and 2005) and changes sign within that time period. What is shown in Fig.3a exactly? Is the correlation done with the absolute lambda values but then moved through that time window (which is less than 40 years to begin with so there are actually very few changes in lambda possible?) and then the change of this over time is averaged and shown here? Or is the correlation done with the actual change in lambda?
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That is a good question. For sure the sea ice variability in not properly simulated in AOGCM as the ocean and the atmosphere are not coupled to the Antarctica ice sheet although the antarctica ice sheet plays a key role in sea ice formation in the southern ocean ; This problem may lead to spurious SST in the southern ocean and thus spurious pattern effect. It could definitely explain an important part of the discrepancy between AMIP-piforcing simulation and observations of the time variations in lambda

I mentioned that the difference between the estimates of ECS using the historical time period and 150 year simulations is 26% but that additional 15% (varies among models) change towards the equilibrium climate sensitivity (difference between EffCS and ECS). The "rebuttal" document mentioned "corrected" but the actual paper text is not (e.g. line 50 and line 150).

Indeed. We don't know how it happened; It should have been corrected. Anyhow in the new version of the manuscript we shortened the abstract to respect nature guidelines and this sentence was removed

In the comment about the PDO/IPO pattern potentially not being an imprint of internal variability but the forced response, I was not clear enough, I guess. I did not mean the PDO would change already in magnitude due to climate change (maybe that's expected towards the end of the 21st century, I agree that this is definitely a second order effect according to our current knowledge). There are papers/people arguing that the PDO-like trend pattern we observe over the last 40 years is an imprint of the forced response already. See discussion e.g., in Liu et al. 2018 (DOI: 10.1175/JCLI-D-17-0462.1) "most excitable mode" or Heede et al. 2020 (DOI: 10.1175/JCLI-D-19-0690.1) in which the East Pacific cooling trend is discussed as initial part of a forced response.

Indeed, we did not understand the comment about the PDO/IPO in the first round of revision; Now it is much clearer thank you.
We agree with reviewer 3 such a forced response with a PDO-like trend pattern cannot be ruled out and should be acknowledge here. However, In the new version of the manuscript the sentence on line 66-68 has been removed because reviewer 1 argued it was too speculative (which is quite true actually)

In summary, I think the hypothesis is valid and the new version of the manuscript supports also evidence for the hypothesis. In addition, extending the feedback analysis back is novel and worth reporting. I find the timescales very hard to interpret and to link to the existing literature which leads to seemingly different findings (mostly with Andrews et al. 2022). At minimum these have to be explained clearly (meaning, the implication of them, not just showing that the method to compute feedbacks is compatible). I also think the observed record should be used until 2022, even thought the CERES time is discussed elsewhere already it would allow to connect the findings of Meyssignac et al. to the existing literature and serve as a test bed for the PDO hypothesis because the feedback evolution should change again between 2005 or 2010 onward. Hence, I suggest major revisions.

We hope that our point concerning the past literature (in particular Andrew et al. 2022) is clear now. In summary, our definition of lambda, when taken over 25 year windows (I.e. as in Fig 3 a and 3b), is very close and consistent with Andrews et al. (2022)'s definition. Indeed, when we apply our definition of lambda and Andrews et al.'s definition on the same AMIP simulations we find very similar decadal variability (Fig 1 above). Now we agree that when we apply our definition of lambda to observations then we find a different decadal variability than in AMIP simulations. We propose some insights in the last paragraph of the main text to explain these discrepancies between observed lambda and AMIP lambda. We acknowledge these insights are not enough to understand the causes for these discrepancies. But it is beyond the scope of the present study to further. We argue at the end of the main text this question call for more research.

As the record we use, unfortunately we can go beyond 2017 yet because reconstructions of sea level are not yet available after 2017 and there is an issue in Argo data that need to be solved after 2017. For sure in the future when the data will be available we intend to update our estimate of the variations in lambda.