nature portfolio

Peer Review File

Ocean's largest chlorophyll-rich tongue is extending westward (2002-2022)

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

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

Version 0:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
GENERAL COMMENTS following the question prompts from the journal

What are the noteworthy results?
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The tropical Pacific upwelling tongue is an important part of the ocean because it is central to ENSO which has global teleconnection impacts. It is a large source of CO2 to the atmosphere because of iron limitation of nutrient and carbon drawdown. Despite productivity and phytoplankton biomass levels that are lower than they might be under iron-replete conditions, the region hosts ecologically and economically important fisheries. In this manuscript, the authors show that the area covered by moderately high chlorophyll concentrations (a proxy for productivity fuelled by nutrient upwelling) has expanded westwards over the last 2 decades. They provide a plausible explanation for this expansion: stronger upwelling-favorable winds associated with a mostly negative phase of the Pacific decadal oscillation.

Will the work be of significance to the field and related fields? How does it compare to the established literature? If the work is not original, please provide relevant references.
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The work follows on somewhat from previous papers (Polovina and others cited in this ms) that have looked at the expansion of oligotrophic gyres. So in that sense it is not original. The results are interesting and well-explained, but I don't think the work reaches the standard of Nature Communications. Previous papers similar to this were published in GRL and Frontiers, which are of course excellent journals. Publications of that calibre might be more suitable for this work, if some issues with the current manuscript are fixed.

Does the work support the conclusions and claims, or is additional evidence needed?
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Yes, mostly, but there are some 'throwaway' comments in the last page or so that could be elaborated on. See detailed comments.

Are there any flaws in the data analysis, interpretation and conclusions? Do these prohibit publication or require revision?
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No, but some of the figures are probably not essential (eg Fig 3, maybe Fig 5) and could be explained in the text.

Is the methodology sound? Does the work meet the expected standards in your field? Is there enough detail provided in the methods for the work to be reproduced?
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Yes.

DETAILED COMMENTS
Abstract, line 20: Change '...and the Southern Equatorial Current...' to '...and a strengthened Southern Equatorial Current...'

Main text, line 28: delete 'natural'. There are no 'un-natural' oceanic sources.
L32: Just my personal opinion, but I would suggest a term like 'equatorial upwelling tongue (EUT)' as opposed to 'CRT'.
L34-36: Delete 'Yet, following decades of climate change, '. Delete 'various'. Insert 'that' after 'currents'.
L37: After citing refs 9-14, describe some specific examples of changes in atmospheric and oceanic processes.
L38: Replace 'in recent' with 'over the last'.
L42: Replace 'on' with 'for'.
L43: Using 'etc' like this is vague and not useful. Replace 'vital' with 'important to understand' and delete 'the'.
L47: delete 'if any' and add a comma after 'discussed'.
L53: Insert 'via the southern equatorial current (SEC)' after 'water' and delete 'via the southern equatorial current (SEC)' on the next line.
L55: Delete 'by convention'.
Figure 1: In general I think it helps clarity to put the letter labels in the caption right before the panel they refer to. So instead of: 'a-c Climatological mean surface chlorophyll...' try 'Climatological mean (a) surface chlorophyll concentration (Chl, mg/m3), (b) current fields (m/s) and (c) nitrate concentration (μmol/kg) ...'. Also, delete 'respectively' and change c to b when referring to 'the area enclosed...'.
L71: Delete 'can be reasonably explained as it'.
Fig 2: See comments for Fig 1 re (a), (b), (c), (d), and insert 'time' before 'series' in L74. Delete 'respectively'. Similar for the remaining figures in the ms.
L84: Replace 'shrink' with 'reduction in size'.
L86: Replace 'During the El Nino event' with 'During strong El Nino events like 2016...'.
L87: Change 'leads' to 'lead'.
L90-91: Delete 'estimated to be' and '~' and just state the number. Also, what is it as a % increase in area?
L91-92: It is great that the authors raise the sometimes controversial issue of when the time series starts and ends. Given that they do not include the very strong 1997-98 El Nino event, I think their results are particularly robust. That El Nino event would bias the size of the upwelling area smaller at the start of the time series, and accentuate their trends.
L98: Delete 'As shown in Fig 3a' and just insert '(Fig 3a)' at the end of the sentence.
L104-109: This analysis of the movement (or not) of the fronts and the conversion between area and longitude changes is not very interesting and does not add anything to the manuscript.
Figure 3: This figure doesn't add much beyond just the description of the results in the text around line 98.
Figure 4 caption: Add 'boreal' before 'summer'.
L128: Add 'the' before 'CRT'.
L129: Delete 'Implementation' and replace 'to' with 'of'.
Figure 5 caption: Replace 'westward propagation' with 'strengthened easterly trades or westward SEC'.
L147-148: Replace 'is found increasing by' with 'increased by'.
L150: The choice of the word 'consequently' here is not useful. It would suggest that the changes in transport at the northern and southern edges should sum to the change in Ekman divergence at the equator, but they don't. Aside from the choice of word, the authors should explain why these numbers don't match. It has to do with what happens with the water between the equator and 5N/5S and also transport at the western edge, but this is not explained at all.
L171-172: The mention of ecosystem impacts is so brief as to not be useful at all. Expand.
L172-175: Same comment but for heating of surface waters. I understand the phenomenon that the authors are alluding to, but such brief mention of it is not helpful to the reader. Expand. Also change 'het' to 'heat'.
L176: The comment re span of the data is a good and could perhaps be expanded on along the lines of my comment above because it lends more meaning to the results.
L179-180: Illustrating the variability of the PDO is probably important, so maybe it could be added to Figure 2 or 5?
L180-181: The comment about links to climate change is weak and not informative.
END OF REVIEW

Reviewer #3

(Remarks to the Author)
General comments for the authors

The scientific question is interesting and the manuscript is well organized.

I was not familiar with EMDs (rather in Empirical Wavelets Transforms, whose results are often similar) but I admit that EMDs are adequate methods for trend determination, if correctly used.

About EMDs, your references are adequate. Nevertheless, you may also refer to the recent paper of Serio et al 2023, "Trend and Multi-Frequency Analysis Through Empirical Mode Decomposition: An Application to a 20-Year Record of Atmospheric Carbonyl Sulfide Measurements" where "residual" are assimilated to the "trend", with at most two zero crossing. In that cases. Note that, from this authors, trends can be sometimes similar to a lowess or moving averages in general (see examples in their figures 5 and 9).
Consequently, what is called trend here can be significantly different from a linear trend notwithstanding that linear trends are effectively not common, especially in a fast changing world.

This is not a central point for the present study that mostly focuses on trends, but I wonder if you can estimate the weight of the amplitude of the trend compared to superior decompositions (maybe 10%, not computed).

About methods, some points must be checked:

1) By choosing the 20-year period "August 2002 -September 2022" period you added two extra months to the period, compared to a multiple of 12 months. This introduces a seasonal bias in the time series, especially when computing the trend.
You should consider the "August 2002 -July 2022", or any number of exact yearly period, for example the "January 2003 - December 2022" period.
Yous hould also read (and cote) Stalonne et al. (2020) (https://www.nature.com/articles/s41598-020-72193-2) about caveats in EMDs and the potential importance and boundary errors.
2) About the confidence limit: the fact that you define the confidence of each IMF only from 5 IMF sets makes sense but this is logicaly conservative (because associated to the method itself) and therefore has a tendency to produces small values. Maybe you should use 2x StDev. (95% limit, assuming that the residual distribution is gaussian), which is a bit more conventional.
3) Optionally, the very last years of the time series (2-3) could have an important weight in the determination of the trend. What is the trend value without these 2-3 years? It is likely that it will be reduced. This is the same effect that you observed between Extended Fig. 2 b-c.
This suggestion is also reinforce by the fact that the trend of the area is strong towards the end of the time series.
4) nothing is said about the precessing level of MODIS Level-3 data. Are they R2020? (cf oceancolor.gsfc.nasa.gov)

As detailed bellow, a nice confirmation of the value of the area trend would be to estimate it also from a purely graphical calculation (result will be a very similar value).

Another, more general suggestion is about the definition of a second more quantitative index, that would consists in the average chl-a value over the CRT area that you define. Is this index in phase with the area? (Is it correlated). In all cases, this would a nice complement.

About trend: What is called a trend (including in EMD) is in fact quite variable (see Moghtaderi et al 2011 "Trend Filtering via Empirical Mode Decompositions"). For these authors, a trend "can contain oscillations while in Wu et al. (2007) only the residual of EMD was deemed a trend, hence constraining it to have no oscillations at all".

You can maybe cite the useful work of Eriksen and Rehman (2007) "Data-driven nonstationary signal decomposition approaches: a comparative analysis" that state for example that "The EMD method is even able to find optimal number of signal modes, K, automatically."

Detailed comments for the authors

Note: line numbers refers to the pdf version of the document

Introduction

I assume that "Main text" is the "Introduction"?

line 29: " supporting half of the annual global new production" : this is not exact, the estimation by [1] was only 18-56% in 1985 i.e. about 37% +-18% , and this was before more precise satellite-based estimations. Much later "Chavez and Toggweiler (1995) estimated that ... this upwelling supported 18% of global new production. ", more in phase with recent results. Also modifiy it in the abstract.

Line 32: ref [3]. You suggests that the surface signature of the equatorial upwelling was discovered in 2023. Please, choose a more suitable reference.

Line 32: " the international date line" is not a very convenient way to say 180°E (or 180°W) or the 180th meridian (at equator).

Line 46: See impact of the length of the time series, as mentioned above.

In the RESULTS section:

Westward extension (of the CRT)
The sentence in Lines 51-54 is general and should be integrated in the previous part of the text.

Lines 70-71: The average seasonal signal of the CRT area (shown in Fig. 2b) do not well support your assertion about the maximum and minimum periods of the equatorial upwelling. The reference #21 do not really confirms this result. There are more adequate references for this (i.e. Phytoplankton carbon and chlorophyll distributions in the equatorial Pacific and Atlantic: A basin-scale comparative study, from Wang et al. and internal references).

The rate of increase of the CRT area that you estimate (from a linear fit to the last IMF component) is ~8.46 × 104 km2/yr.

This is a major point of your study.
Interestingly, a graphical calculation from your Figure 4 shows that the surface of the expanded area (minus the 2003-2007 one) is quite close to the value of your trend (I estimate it to 1.2 105 km2/yr, which is quite close to you result. This would be a satisfying confirmation, that indirectly shows the adequacy of the EMD to measure it). I suggests that you do this calculation to confirm you result.
Comparatively, the use of the OC-CCI data set is not as important because a composite product is less adequate to compute trends (to place it as supplement is a good choice).

A second central result (not original) is that the interannual signal is well correlated to the Multivariate ENSO index (MEI). Note that this signal is also much higher than the trend itself. This raises the point of the real significance of the trend, especially increasing at the end of the time series, maybe because of the strong niña event during the last two years. This raises the more general impact of the influence of the tails of the series previously mentioned.
This Niña period can severely biases the determination of the trend. This point must be absolutely evaluated and discussed because it could strongly minimise the main message of the paper which is clearly based on a significant trend that maybe do not exists at all.

Why the result shown in Figure 4. is build only from the boreal summer data?
What would be the results with all data combined?
If the main result of the study is about the existence of a trend in the CRT area and position, this must be better defined. For example, if the "trend" is mostly seasonal (i.e. in summer and not in winter), this has a major significance on the underlying mechanisms, e.g. trend wind variability and/or fluctuations in surface currents.

Extended Figure 1: what is defined as winter… (Dec-Jan-Feb)? Etc.

In support to figure 2: The average seasonal signal is not represented . It could be added to figure 2, for more clarity as well as to support your assertion lines 70-71 about minimum/maximum.

Discussions and implications

The main results of the work are relevant and are correctly discussed (potential effects or light shading; negative phase of the Pacific Decadal Oscillation).
The limitations of the relatively short (20-year) period is of course a classic limitation in such a study but is adequately discussed.

Lines 344: repetition, to remove.

Reviewer #4

(Remarks to the Author)
KEY RESULTS

The manuscript presents a timely and careful analysis of the potential changes in the spatial extent of the so-called chlorophyll-rich tongue (CRT) in the equatorial Pacific Ocean - a prominent feature in an area whose dynamics are of great climatic and ecosystem significance on a global scale. Based on the spatio-temporal analysis of satellite-derived data, the authors conclude that the CRT has expanded westwards over the last 2 decades, providing plausible partial explanations for the physical mechanisms controlling this phenomenon. The manuscript is well written with a clear structure and articulation of interpretation and conclusions drawn.

SIGNIFICANCE

In general the main findings of the paper are interesting and relevant for the scientific marine biogeochemical community. The presented results are potentially significant and appear to address the need for detailed regional analysis of potential climate change driven trends in indicators of ocean ecology, such as the concentration of chlorophyll-a as a proxy for changes in phytoplankton biomass. For example, the area of CRT's westward expansion corresponds well with the area of some of the highest signal-to-noise ratios reported to occur globally when analysing long-term trends in remote-sensing reflectance (Cael et al. 2023).

At the same time, the manuscript does not provide any specific justification for why the CRT expansion per se represents such an important issue. In lines 42-43 the authors claim that "changes in CRT may have consequences on ecosystems, heat exchanges, air-sea CO2 fluxes, etc. (...)" yet they do not provide any reference to support such claims. The presented study itself also does not include any results to verify such claims.

More justification, even if just based on the literature, for why the CRT westward expansion is important is needed. The manuscript should also indicate whether prior observations or modelling studies of this phenomenon were attempted or not.

VALIDITY

The authors have adequately picked the available data to support the research investigation they set out to perform. The data seems to be interpreted robustly, however, the analysis is not presented in a fully transparent or clear way.

The conclusions are supported by the interpretation of the data. However, there are several critical questions concerning especially: (i) the rationale for and details behind trend detection method applied, (ii) the selection of parts of the original data sets for spatio-temporal analysis, and (iii) the implications of the above for drawing the right conclusions.

Firstly, visual inspection of the original data does not immediately reveal such a strong increasing residual trend as detected through the selected trend detection method. While EMD is a very powerful trend detection method as stated in lines 336-228, the authors do not explain why they choose EMD as opposed to other, perhaps more conventional, time-series analysis methods. Similarly, it would be good to know why the authors chose EMD and not EEMD (ensemble empirical mode decomposition) which according to Wu and Huang (2009) "represents a substantial improvement over the original EMD (...)." Comparison with at least one other trend-detection method could help support the conclusions.

Secondly, it is not clear why the authors do not take into account the Pacific Decadal Oscillation (PDO) anywhere in their analysis prior to mentioning its significance in lines 178-180. In general, it is surprising to see the lack of any distinct decadal signal explicitly identified in their time series analysis despite the fact that EMD should result in distinct IMF(s) corresponding to low-frequency signal, if present. A visual inspection of Fig.2 (b-c) suggests that the decadal signal related to the PDO may have been mixed within the inter-annual variability signal correlated with the MEI. The authors don't clarify what they mean by inter-annual variability (i.e. period/frequency range) even though EMD should produce IMFs with quite distinct frequency signals, unless combined in post-processing. Similarly, the high-frequency IMF appears to be in fact a mix of at least two IMFs corresponding to the seasonal and intra-seasonal signal. More detailed results of EMD need to be presented and described, at least as extended Data Figures.

While the authors speculate on the potential residual trend being in fact a component of decadal oscillation, they also don't use the full potential of the EMD method to test such a hypothesis. Wu et al. (2007), cited by the authors (ref. #34), talks about the value of evaluating multi-decadal trends as opposed to looking at simple linear trends or the overall adaptive (residual) trend as applied in this manuscript. Additional analysis in that direction appears critical considering the authors' choice of the title which points at a clear long-term trend, even though elsewhere, in lines 175-180, the authors admit that the observed trend is "(...) likely a result of naturally occurring, internal variability (...)"

More detailed comments and suggestions for improvements are listed below under Data and Methodology.

DATA & METHODOLOGY

Please add significance values (p-values) to all the trends plotted in the paper to see if they are significant.

Please add a plot/plots to see gradual evolution of CRT area through the years. Your conclusions can be a part of inter-annual variability, or a method artifact which is difficult to track with the way you visualise the data.

Lines 34-35: Please add some references. There are past global or regional studies on temporal evolution of chlorophyll in the equatorial Pacific (e.g. the already mentioned Cael et al., 2023).

Lines 47-48: delete "if any" or expand what you mean by having no forcing behind such an observed change. "Ocean currents data" instead of "current data." What about the effects of cooling/warming? Have you considered looking at parallel changes in satellite sea surface temperature? If not, why?

Lines 53-54: The reader might be convinced that westward advection of "nutrient-rich water" stimulates phytoplankton growth in the region, however, it is not nitrate which controls phytoplankton growth in much of the equatorial Pacific. Silicate and iron are the growth-limiting nutrients, whose supply via upwelling and passage of Tropical Instability Waves moderates much of the production in the area. Also, the role of the upwelling of nutrients via the Equatorial Undercurrent has been clearly established on top of the redistribution via the SEC. See for example Nelson & Landry (2011) and the references therein to several related articles in that special issue. Please clarify why you are looking at nitrate in this study, and not any other nutrients, especially since the presented nutrient climatology data is not subject to any further analysis.

Figure 1: Caption title mentions nutrients but you are only showing nitrate concentrations.

Figure 2: Please add labels (years) to all panel x-axes. All panels have different scales on the y-axes. Panels b and c should have the same ranges, or if not, explain why in the caption. It is not clear whether the presented EMD results are the original IMFs or combined IMFs with a broader frequency range.

Line 91: The rate of increase does not include any uncertainty estimate. Fig. 2 includes confidence limits which should be used to report uncertainty along with the final result. Also, the choice of MODIS-Aqua vs OC-CCI dataset and period considered (extended Data Fig. 2) very strongly affects the magnitude of the trend. Please take that into account when reporting the final result and associated uncertainty.

Lines 109-114: Why is this analysis limited to the boreal summer? Would be helpful to know the seasonal evolution of the extension of CRT. Is it expanding uniformly throughout the year? Why/why not?

Figure 4: Why do you only show these specific time frames (2003-2007 and 2018-2022)? Based on visual inspection of Figure 2a), choosing CRT area for e.g. 2010-2014 vs 2016-2020 would result in an opposite conclusion, i.e we will see the CRT area decreasing. It would be helpful to see the gradual evolution of the CRT area through the years. I suggest to make the graphs and/or maps showing evolution from year to year or one map with isolines showing i.e. evolution of maximum CRT area from year to year, like in studies of i.e. Arctic Sea Ice evolution - see for example Figure 2 in Francis and Wu (2020) or Figure 2 here (https://ccin.ca/ccw/seaice/future).

Lines 132-133: the statement “reveals an increasing trend” sincere there seems to be a decreasing trend in Figure 5b.

Line 163: I would delete “detailed”

Line 174: heat? Could you give examples from previous studies of how much change in chlorophyll-rich areas can influence ocean heat budget?

Lines 177-178: “It is difficult, if not impossible, to predict if the CRT will continue to expand beyond 2022” - if you want to mention further predictions please cite the studies forecasting spatial chlorophyll evolution after 2022. Numerous modelling studies like this exist with varying degrees of certainty.

Line 336: efficiency instead of efficacy

Lines 338-349: This is a general description of EMD but not a description of the exact procedure applied by the authors in this study. In particular, how many IMFs were generated for each time series using a single S criterion, and at what characteristic frequencies? Was there any mixing of IMFs prior to presenting the results in Fig. 2? If so, how and on what grounds? This information is crucial to ensuring reproducibility of the analysis.

Lines 352-353: Wu et al. (2007), your reference #34, includes a very useful discussion on other types of trends which might be relevant for this case, with large likelihood that the presented residual trend is in fact a component of the long-term natural variability related to the PDO.

Lines 354-358: Consider also the white noise assisted EEMD method (Wu and Huang, 2009) as an alternative to provide explicit confidence intervals around each set of IMFs, even for a single S criterion.

Line 360: Did I understand it right that World Ocean Atlas 2018 has nitrate data for 2002-2022 that you use in the analysis (see your Fig.1 caption)? Also, it is not nutrient data, but nitrate data only.

REFERENCES USED IN THE REVIEW COMMENTS

Cael, B.B., Bisson, K., Boss, E. et al. (2023). Global climate-change trends detected in indicators of ocean ecology. Nature 619, 551–554; doi: 10.1038/s41586-023-06321-z

Francis, J. and Wu, B., (2020). Why has no new record-minimum Arctic sea-ice extent occurred since September 2012? Environmental Research Letters, 15; doi: 10.1088/1748-9326/abc047

Nelson, D. M., & Landry, M. R. (2011). Regulation of phytoplankton production and upper-ocean biogeochemistry in the eastern equatorial Pacific: Introduction to results of the Equatorial Biocomplexity project. Deep Sea Research Part II: Topical Studies in Oceanography, 58(3-4), 277-283; doi: 10.1016/j.dsr2.2010.08.001

Wu, Z., & Huang, N. E. (2009). Ensemble empirical mode decomposition: a noise-assisted data analysis method. Advances in adaptive data analysis, 1(01), 1-41; doi:10.1142/S1793536909000047

Reviewer #5

(Remarks to the Author)
"I co-reviewed this manuscript with one of the reviewers who provided the listed reports (Dr Artur Palacz). This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts."

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

Reviewer #3

(Remarks to the Author)
The manuscript has been significantly improved from the its first draft and my previous suggestions have been correctly answered, as well as those from the other reviewers. The EMD is more precisely explained in the method section, specially regarding frequency combinations.
The importance of ENSO influence (through MEI) is better discussed.
I thank the authors for having tested the EEMD method on their data set (extended Fig. 3.), that shows a similar trend, although 30% lower and with a higher theoretical uncertainty.

I personally regret that the various contributions of the seasonal, interanneual and trend components (respectively 32, 59 and 9%) are not explicitly mentioned, at least in the results because this is just a very interesting quantification, independently of the scope of the paper, more about trends.

Maybe because of the better timing, the Western/southern expansions of the CRT also look spatialy more "regular".

Title: I suggest : "Ocean's largest …. westward (2002-2022)" (not "in"). Alternatively replace "in" by "during".

Abstract:
Because the nitrate mapping part (and "enrichment" considerations) have been removed, I do not think that the mention of "Our findings imply a broader cover of productive water along the equator" it still relevant. This sentence is acceptable but a bit general...You be the judge.

Second point for the Abtract: I personally think that the simultaneous extension of the Pacific gyres, expanding poleward (as already described in the literature) is an important point that should be briefly added in the abstract, because this result is not so straightforward in this new context.

Introduction: OK, a bit improved from the last version

Westward extension
The fact that the surface of the CRT is computed for all months of data (not only in summer) improves the quality and stability of the results, numerically and graphically. Alternatively it shows a much higher positive extension trend and I imagine that your were satisfied by this result.

Strengthening of trade winds and upwelling :
I do not have any particular comment, the Ekman transport is now more adequately explained (thanks to another reviewer).

Detailed corrections (Note: line numbers refers to the last pdf version of the document)

- Figure 1b: it would be great to place the current names (SEC, NEC, NECC) and corresponding main stream (arrows).
- Extended data Fig. 2: move the Y axis of Fig. (b) on the left.
- Extended data Fig. 4: I do not understand why you increase the length of the arrows, they do not represent the actual changes! (sorry I did not paid attention to that before).
- In the whole text: it would be more precise to replace "Equatotial current" by "South Equatorial Current"

Line 77: Reference 27 (Wang et al. 2013) is not dealing at all with the Eq. Current system. It must be removed or replaced.

About EMD decomposition:
A personal question: the trend of the Westward extension has more than doubled from (8.46 × 104 km2/yr to 1.87 (± 0.82) × 105 km2/yr), Is it only explained by the fact that you consider now all months of the year instead summer months only?
By the way, the interannual component, that also is higher than in the previous calculation which makes cense because the MEI is supposed to be stronger in boreal winter.

Lin 117: I would remove " considering that 1° at the equator corresponds to ~111.2 km" (quite well known)

Discussions and implications
I am sorry but "the CRT in the equatorial Pacific did not shrink but expanded from 2002 to 2022" is not exactly a question but an assertion. Please correct it, in one way or another.

Line 208: Stallone (instead of Stollen)

Reviewer #4

(Remarks to the Author)
I would like to thank the authors for a careful and detailed response to the original comments provided by all reviewers, and for implementing the suggested modifications. I think that the authors have done a very good job in providing additional

information about their methods of analysis, and presentation of results. The manuscript has in consequence improved in quality significantly.

However, I still have concerns about the overall interpretation of the time series analysis results, and in particular, about drawing consistent conclusions about the allegedly observed westward expansion of the CRT vs concluding that the expansion is part of a lower frequency oscillation related to the PDO. A clear and consistent message is missing from the current version of the manuscript. Below please find detailed comments focusing on identifying those sections of the manuscript where there are major inconsistencies or lack of clarity in such interpretation.

I recommend that these issues be resolved prior to publication.

Throughout the manuscript there appear inconsistencies in referring to interannual, interdecadal (e.g line 434, Extended Data Figure 5) and multidecadal (e.g. line 431) signals. For instance, in line 70-71, an abritrary frequency threshold of >8 years was selected for an internannual signal. Should such a signal not be called "interannual to decadal"? On page 19, lines 418-419, interannual is defined as signal >2 years…. In lines 446-448 authors talk of extracting low-frequency (>8 years) signal from the PDO index. I'm convinced that early on in the manuscript the authors needs to clearly explain how they define these different frequency signals and whether they are applied consistently across the different methods of time series analysis.

p. 4, lines 41-43: "The changes in CRT may have consequences for ecosystems, heat exchanges, and air-sea CO_2 fluxes, which is especially important to understand amidst ongoing global climate change." - If possible, please specify the details of such consequences, better with numerical approximations i.e. "potential expansion of CRT was shown to increase (or decrease) the heat budget in xx area xx times and increase (or decrease) the CO_2 uptake from the atmosphere xx times". Please put relevant numbers instead of "xx" from the literature you cite.

p.4 line 67: the phrase "relatively easy to implement" is not precise and does not add anything meaningful to the justification for choosing this method. I suggest to remove or revise this statement.

p.4 lines 69-77: This text includes many contradictions. What is the difference between "long-term" and "multi-decadal" in the context of this manuscript? I suggest that here the authors only mention that the focus of the analysis is on distinguishing between seasonal and interannual variability and the residual trend which may or may not be part of a longer, multi-decadal cycle - something which cannot be unambiguously determined from a 20-year time series. Parts of this text are also already included in the Fig. 2 caption. Information about combining IMFs can also be moved there I think.

p. 6 line 96: Please define the "remaining residual component" which is not defined by the authors until this point.

p. 6 lines 101-105: Why is there no error bar on the EEMD-derived residual trend reported here if it is given in Extended Data Fig. 3? The phrase "high degree of certainty" seems inadequate considering that the presented EEMD results reveal possible lack of trend within the 95% confidence limit. The p-value of <0.05 only refers to the average trend.

Figure 4: both colorbar labels are for 2002, I would suggest the last one should be "2022"

p. 12 lines 214-223: Up until this point the reader is left under the impression that the detected trend is that of steady increase across the two decades under study. This paragraph contradicts the results presented in the previous sections. In my view, the conclusion of this paragraph must be reported much earlier in the text.

p. 19 lines 408-412: I don't understand what is meant by phrases "relatively simple" or "relatively complex" when describing EMD and EEMD. Also, to be precise, EEMD was only applied to one dataset (CRT expansion) while EMD applied to all time series used in the study. Differences between the results of EEMD and EMD are not sufficiently explained in my view.

Extended Data Figure 1: I wish that all panels used the same scale of 10^7 km2. Otherwise, it is difficult to compare IMF6 to the rest of the signals. Based on this figure, I still struggle to understand why the authors focus on reporting a residual trend rather than talk about a very pronounced interannual to decadal signal, with a clear westward expansion of the CRT over the last 10 years, coincident with a negative phase of the PDO.

Extended Data Figure 3: The authors fail to comment on the wide confidence interval over the last 7-8 years of the study period, including uncertainty over the direction of the residual trend. Is this related to oversifting when using certain S stoppage criteria?

Extended Data Figure 5: Why not report the trend in the same unit as above, i.e. in 10^5 km^2? That would make comparisons much easier. Why would the average residual trend be >4 times smaller than reported e.g. on Extended Figure 2 (a)? Were the IMFs combined differently here? In fact, these should render exact same results. I don't understand where the difference comes from.

Reviewer #5

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature

Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Version 2:

Reviewer comments:

Reviewer #3

(Remarks to the Author)
I am satisfied with all the corrections done.
My detailed comments are in the document enclosed "498598_2_rebuttal_9887878_smsjf9_Answer-Reviewer3.docx"

Also add at line 463: "v2" to 'The bi-monthly Multivariate ENSO index'

Reviewer #4

(Remarks to the Author)
I would like to thank the authors for carefully considering my remarks, and for the detailed responses. Overall, I'm very satisfied with the presented responses, and the corresponding changes to the manuscript. Additional explanations and edits to some sections improved the clarity of the manuscript in my view.

At this point I only have very few comments, most of which are of editorial nature. The authors may consider them to further improve the text. Otherwise, I recommend the manuscript for publication.

----- Detailed comments -----

Lines 9-10: Please change "chlorophyll" to "chlorophyll-a" as in line 49, and add "satellite" after "MODIS-Aqua", as in line 50.

Lines 127-130: I suggest to move these two sentences till after line 162, as a concluding statement of that entire section which encompasses also the pixel-based analysis of spatial expansion. Otherwise, such a broad conclusion appears premature.

Lines 213-215: This sentence is awkward and could possibly read better if "namely" and "rather" or similar words were added. E.g.:
"The above analysis offers a compelling answer to the proposed question regarding the changes of : the CRT in the equatorial Pacific, **namely** that the CRT did not shrink but **rather** expanded from 2002 to 2022.

Lines 255-258: Very complex sentence, hard to follow. Consider rephrasing as for example this way:
"The ENSO-CRT area correlation observed in this effort and in prior studies [2,24,29] suggests that, if the occurrences of consecutive La Niña events increase under global warming as projected [14], the CRT will probably further extend to the west."

Reviewer #5

(Remarks to the Author)
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Responses to Comments on Manuscript NCOMMS-24-19672

Preface:

We sincerely thank the four anonymous reviewers for their valuable comments and suggestions, which have greatly enhanced the quality of our manuscript. In response, we have made substantial revisions, with key changes summarized below. For comments by each reviewer, we have provided detailed, point-by-point responses in this letter. The reviewers' comments are presented in black, while our replies are in light blue.

Major Changes:

1) Title:

Added the temporal span (2002-2022) in the title to specify the study period and avoid implying an indefinite long-term trend.

2) Data and Figures:

Adjusted the study period to cover an exact yearly cycle (August 2002 to July 2022) by removing the data of the last two months;

Similar changes were made to the Extended Data Fig. 2, where the trend of CRT area from OC-CCI data was calculated from the period of September 1997 to August 2023;

Removed the nitrate mapping from Fig. 1c and Extended Data Fig. 2 in the previous version, as they don't add much to our analysis.

3) Methods:

Provided the rationale for using the EMD for trend detection and a description of the exact procedure we applied in this study;

Included the results of EEMD as a comparison (presented in the newly added Extended Data Fig. 3);

Added a purely graphical estimate of the CRT expanding rate.

4) Link between the CRT changes and the PDO:

Extended the CRT area time series mathematically, following the method in the suggested reference (Stallone et al., 2020), to enable decomposition of the multi-decadal component, revealing a significant negative relationship between the multi-decadal component and the PDO. These results are presented in the newly added Extended Data Fig. 5;

5) Evolution of the CRT area:

Added year-to-year evolution of the CRT in the newly added Fig. 4a, as suggested. Changed the original Fig. 4 to Fig.4b by using the annual mean CRT area.

Point-by-point responses

Comments from Reviewer #2:

General Comments:

What are the noteworthy results?

The tropical Pacific upwelling tongue is an important part of the ocean because it is central to ENSO which has global teleconnection impacts. It is a large source of CO2 to the atmosphere because of iron limitation of nutrient and carbon drawdown. Despite productivity and phytoplankton biomass levels that are lower than they might be under iron-replete conditions, the region hosts ecologically and economically important fisheries. In this manuscript, the authors show that the area covered by moderately high chlorophyll concentrations (a proxy for productivity fuelled by nutrient upwelling) has expanded westwards over the last 2 decades. They provide a plausible explanation for this expansion: stronger upwelling-favorable winds associated with a mostly negative phase of the Pacific decadal oscillation.

Thank you for the support and the positive comments!

Will the work be of significance to the field and related fields? How does it compare to the established literature? If the work is not original, please provide relevant references.

The work follows on somewhat from previous papers (Polovina and others cited in this ms) that have looked at the expansion of oligotrophic gyres. So in that sense it is not original. The results are interesting and well-explained, but I don't think the work reaches the standard of Nature Communications. Previous papers similar to this were published in GRL and Frontiers, which are of course excellent journals. Publications of that calibre might be more suitable for this work, if some issues with the current manuscript are fixed.

We must emphasize that **this is the first time** a long-term trend and the westward expansion of the chlorophyll-rich tongue (CRT) in the equatorial Pacific Ocean has been revealed. This expanding trend was previously hidden beneath the most prominent high-frequency El Niño-Southern Oscillation signal. In particular, previous reports on the expansion of the North and South Pacific oligotrophic gyres may imply a retraction of the CRT, as it is located right between them. **Our findings demonstrate the simultaneous expansion of the CRT and the two gyres.** This discovery offers critical new insights, making it a significant contribution worthy of publication in Nature Communications. Undoubtedly, this study is highly original, though we were inspired by the work of Polovina et al. (2009) and others cited in the manuscript.

Does the work support the conclusions and claims, or is additional evidence needed?
Yes, mostly, but there are some 'throwaway' comments in the last page or so that could be elaborated on. See detailed comments.
Thanks very much for these detailed comments. We have provided point-by-point responses to these comments and revised the manuscript accordingly.

Are there any flaws in the data analysis, interpretation and conclusions? Do these prohibit publication or require revision?
No, but some of the figures are probably not essential (eg Fig 3, maybe Fig 5) and could be explained in the text.
Fig.3 and Fig.5 directly support two main findings of this study, i.e., the westward expansion of CRT and increased easterly trade winds drove the westward movement, respectively. We think it is better to keep these two figures to better illustrate our findings.

Is the methodology sound? Does the work meet the expected standards in your field? Is there enough detail provided in the methods for the work to be reproduced?
Yes.
Thank you for the positive comments regarding the methodology of the present study.

Detailed Comments:

Abstract, line 20: Change '...and the Southern Equatorial Current...' to '...and a strengthened Southern Equatorial Current...'
Main text, line 28: delete 'natural'. There are no 'un-natural' oceanic sources.
Revised as suggested.

L32: Just my personal opinion, but I would suggest a term like 'equatorial upwelling tongue (EUT)' as opposed to 'CRT'.
Thanks for the suggestion! Note that the chlorophyll-rich tongue is nourished not only by nutrients from equatorial upwelling but also by westward advection of coastal nutrient-rich waters. We think CRT might be a better term than EUT.

L34-36: Delete 'Yet, following decades of climate change, '. Delete 'various'. Insert 'that' after 'currents'.
L37: After citing refs 9-14, describe some specific examples of changes in atmospheric

and oceanic processes.
L38: Replace ‘in recent’ with ‘over the last’.
L42: Replace ‘on’ with ‘for’.
L43: Using ‘etc’ like this is vague and not useful. Replace ‘vital’ with ‘important to understand’ and delete ‘the’.
L47: delete ‘if any’ and add a comma after ‘discussed’.
L53: Insert ‘via the southern equatorial current (SEC)’ after ‘water’ and delete ‘via the southern equatorial current (SEC)’ on the next line.
L55: Delete ‘by convention’.
Thank you very much for these detailed comments. All has been revised as suggested.

Figure 1: In general I think it helps clarity to put the letter labels in the caption right before the panel they refer to. So instead of: ‘a-c Climatological mean surface chlorophyll...’ try ‘Climatological mean (a) surface chlorophyll concentration (Chl, mg/m3), (b) current fields (m/s) and (c) nitrate concentration (μmol/kg) ...’. Also, delete ‘respectively’ and change c to b when referring to ‘the area enclosed...’.
Revised as suggested. In addition, we removed the figure for the distribution of nitrate concentration as it does not add much to our analysis. The caption of Figure 1 is now rewritten as,

“**Fig. 1. | Illustration of the chlorophyll-rich tongue (CRT) and spatial distributions of ocean current fields in the equatorial Pacific.** Climatological mean (**a**) surface chlorophyll concentration (Chl, mg/m^3) and (**b**) current fields (m/s) between August 2002 and July 2022. The CRT climatology boundary is delineated with the black curve, representing the Chl isoline of 0.1 mg/m^3. The area enclosed by the red dashed lines in (**b**) denotes the central positions of the South Equatorial Current (5° S-5° N, 170° E-100° W).”

L71: Delete ‘can be reasonably explained as it’.
Fig 2: See comments for Fig 1 re (a), (b), (c), (d), and insert ‘time’ before ‘series’ in L74. Delete ‘respectively’. Similar for the remaining figures in the ms.
L84: Replace ‘shrink’ with ‘reduction in size’.
L86: Replace ‘During the El Nino event’ with ‘During strong El Nino events like 2016...’.
L87: Change ‘leads’ to ‘lead’.
Corrected.

L90-91: Delete ‘estimated to be’ and ‘~’ and just state the number. Also, what is it as a % increase in area?
Revised as suggested. The sentence is now rewritten as,

“The rate of CRT expansion is 1.87 (± 0.82) × 10^5 km^2/yr, equivalent to an increase of 0.8% per year.”

L91-92: It is great that the authors raise the sometimes controversial issue of when the time series starts and ends. Given that they do not include the very strong 1997-98 El Nino event, I think their results are particularly robust. That El Nino event would bias the size of the upwelling area smaller at the start of the time series, and accentuate their trends.
Thanks for the positive comments.

L98: Delete 'As shown in Fig 3a' and just insert '(Fig 3a)' at the end of the sentence.
Corrected.

L104-109: This analysis of the movement (or not) of the fronts and the conversion between area and longitude changes is not very interesting and does not add anything to the manuscript.
This analysis based on the changes in the CRT western boundary **provides an independent estimate of the expansion rate of the CRT**, which is highly consistent with that directly obtained from the EMD analysis of the time series of the CRT area. These two independent analyses provide cross-validation of the expanding rate, lending additional confidence to the key results of this manuscript.

Figure 3: This figure doesn't add much beyond just the description of the results in the text around line 98.
Figure 3 offers direct evidence of the westward expansion of the CRT, which is the key finding of this study. We decided to keep this figure to provide readers with a clear and straightforward understanding of the westward extension of the CRT.

Figure 4 caption: Add 'boreal' before 'summer'.
As suggested by the other two reviewers, we have changed Fig. 4 to show the comparison of the annual mean CRT area between two periods (2003-2007 vs 2018-2022), instead of comparing the mean CRT area in boreal summer. The caption is revised accordingly.

L128: Add 'the' before 'CRT'.
L129: Delete 'Implementation' and replace 'to' with 'of'.
Figure 5 caption: Replace 'westward propagation' with 'strengthened easterly trades or westward SEC'.
L147-148: Replace 'is found increasing by' with 'increased by'.
Revised as suggested.

L150: The choice of the word 'consequently' here is not useful. It would suggest that the changes in transport at the northern and southern edges should sum to the change in Ekman divergence at the equator, but they don't. Aside from the choice of word, the authors should explain why these numbers don't match. It has to do with what happens with the water between the equator and 5N/5S and also transport at the western edge, but this is not explained at all.

Thank you for your reminder! The word "consequently" has now been removed. The mismatch is now explained in the text as suggested. See below as well as lines 172-176 of the revised manuscript.

"Note that this number of the total Ekman divergence (~15.29 Sv) does not match the combination of those at the northern (~16.47 Sv) and the southern edges (~5.02 Sv), which is mainly due to the north-south asymmetry of surface equatorial current system[39, 40] and neglect of the transport at the western edge[41]."

L171-172: The mention of ecosystem impacts is so brief as to not be useful at all. Expand.
Expanded as suggested. See below as well as lines 195-199 of the revised manuscript.

"The expansion of the CRT implies a broader coverage of phytoplankton-rich water along the equator. As phytoplankton are a crucial part of ocean ecosystems, feeding zooplankton and thus living resources at higher trophic levels, the westward extending of the CRT possibly results in large-scale redistribution of certain species, influencing ecosystems and fisheries[9, 42]."

L172-175: Same comment but for heating of surface waters. I understand the phenomenon that the authors are alluding to, but such brief mention of it is not helpful to the reader. Expand. Also change 'het' to 'heat'.
Expanded as suggested. See below as well as lines 199-204 of the revised manuscript.

"Also, as chlorophyll in phytoplankton absorbs solar light, a higher abundance of phytoplankton increases the absorption of solar radiation, leading to an enhanced heating rate at the ocean surface[43]. Therefore, changes in the spatial distribution of the CRT would lead to a redistribution of heat in the surface layer, influencing tropical climate, especially the amplitude and asymmetry of the ENSO[24, 44]."

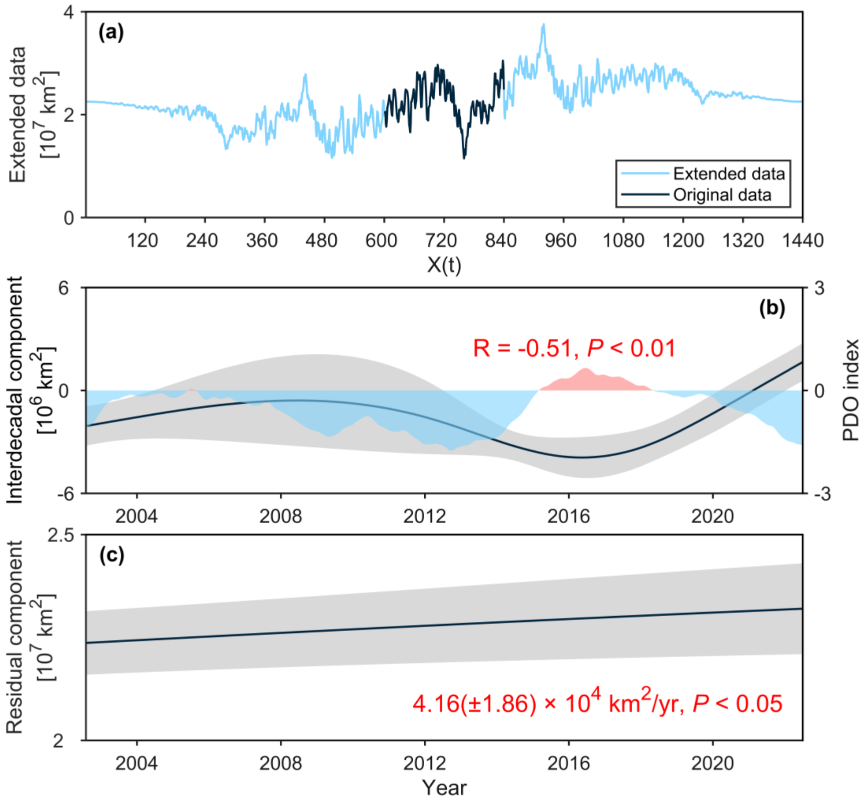
L176: The comment re span of the data is a good and could perhaps be expanded on along the lines of my comment above because it lends more meaning to the results.
We have revised this sentence as suggested. In addition, we specified the span of the data (2002- 2022) in the title of the revised manuscript. See below as well as lines 226-229 of the revised manuscript.

"Of course, the observed long-term trend of the CRT area highly depends on the data span used for trend analysis[25, 49]. Thus, it is necessary to stress that the reported westward extending trend of the CRT is observed within a temporal period of 2002-2022."

L179-180: Illustrating the variability of the PDO is probably important, so maybe it could be added to Figure 2 or 5?
Following suggestions from all reviewers, we have thoroughly updated the presentation

to demonstrate the links of the CRT expansion to the PDO by introducing a new figure (Extended Data Fig. 5). See below and lines 205-218 of the revised manuscript.

"On the other hand, it is noticeable that our study period of 2002-2022 aligns with a mostly negative phase of the Pacific Decadal Oscillation (PDO)[45-47]. To explore potential links of the low-frequency variabilities of the CRT to the PDO, we adopted a mathematical approach of Stollen et al. (2020)[48] to symmetrically extrapolate the CRT area time series out of the boundaries, generating a new time series that is five times longer than the original one (see Methods). The decomposed interdecadal component of the CRT area after applying the EMD to this prolonged time series shows a significant negative correlation with the PDO in 2002-2022; meanwhile, there exists a weaker residual trend at a rate of 4.16 (± 1.86) × 10^4 km^2/yr ($P < 0.05$) (Extended Data Fig. 5) compared to that calculated from the original time series (Fig. 2d). It suggests that the original 20-year CRT residual trend shown in Fig. 2d contains multi-decadal signal to a large extent. Therefore, the observed westward expansion of the CRT in 2002-2022 looks like mostly a result of naturally occurring, internal variability related to the PDO."



Extended Data Fig. 5. | Decomposition of the extended time series of the CRT area. (**a**) The extended monthly time series of the CRT area (blue line) from the original data (black line) (see Methods) The decomposed (**b**) interdecadal and (**c**) residual trend components via EMD analysis are shown only for the temporal span from Aug.2002 to Jul. 2022. The red and blue colors in (b) represent the positive and negative values of the PDO index, respectively. The gray shading represents the 95% confidence limit. The trend and p-values in (**c**) were computed from the data from Aug.2002 to Jul. 2022.

L180-181: The comment about links to climate change is weak and not informative.
Agreed. It may require longer time series observations to confidently link the westward expansion of the CRT to climate change. The related statements have now been revised as follows (lines 221-223 in the revised manuscript),

“Nevertheless, a truly decades-long CRT data record, rather than a mathematically prolonged time series, must be established to confidently attribute the expansion of the CRT extent to climate change.”

Comments from Reviewer #3:

General Comments:

The scientific question is interesting and the manuscript is well organized.
Thank you for the support and positive comments!

I was not familiar with EMDs (rather in Empirical Wavelets Transforms, whose results are often similar) but I admit that EMDs are adequate methods for trend determination, if correctly used.
About EMDs, your references are adequate. Nevertheless, you may also refer to the recent paper of Serio et al 2023, "Trend and Multi-Frequency Analysis Through Empirical Mode Decomposition: An Application to a 20-Year Record of Atmospheric Carbonyl Sulfide Measurements" where "residual" are assimilated to the "trend", with at most two zero crossing. In that cases. Note that, from this authors, trends can be sometimes similar to a lowess or moving averages in general (see examples in their figures 5 and 9).
Consequently, what is called trend here can be significantly different from a linear trend notwithstanding that linear trends are effectively not common, especially in a fast changing world.
Thank you for these great suggestions! We have cited the most recent paper of Serio et al. (2023) (Ref 55 in the revised manuscript), which is an inspiring example of EMD application. See lines 403-405 of the revised manuscript.

“The Empirical Mode Decomposition (EMD) has demonstrated its efficacy in extracting intrinsic trends and natural variability from climate data in geophysical research[25, 53-55].”

This is not a central point for the present study that mostly focuses on trends, but I wonder if you can estimate the weight of the amplitude of the trend compared to superior decompositions (maybe 10%, not computed).
Following the approach of Serio et al. (2023) (the most recent reference you suggested above), we obtained an estimate of the weight of the amplitude of the trend, which contributes 9% to the variability of the total signal, while the interannual and seasonal components explain 59% and 32%, respectively. As you mentioned, these numbers are not the key point of our study, and they are not discussed in the revised manuscript.

About methods, some points must be checked:
1) By choosing the 20-year period "August 2002 -September 2022" period you added two extra months to the period, compared to a multiple of 12 months. This introduces a seasonal bias in the time series, especially when computing the trend.
You should consider the "August 2002 -July 2022", or any number of exact yearly period, for example the "January 2003 -December 2022" period.
As suggested, we have **adjusted the data span** to reflect a precise yearly cycle from

August 2002 to July 2022. All the figures and texts in the revised manuscript have been updated accordingly. Similarly, the OC-CCI data has been constrained to the period from September 1997 to August 2023 (Extended Data Fig. 2).

You should also read (and cote) Stallone et al. (2020) (https://www.nature.com/articles/s41598-020-72193-2) about caveats in EMDs and the potential importance and boundary errors.
Thank you for this constructive suggestion! We tried the approach of Stallone et al. (2020) and expanded the discussion regarding the link of CRT changes to the PDO. See below as well as lines 205-223 in the revised manuscript.

"On the other hand, it is noticeable that our study period of 2002-2022 aligns with a mostly negative phase of the Pacific Decadal Oscillation (PDO)[45-47]. To explore potential links of the low-frequency variabilities of the CRT to the PDO, we adopted a mathematical approach of Stollen et al. (2020)[48] to symmetrically extrapolate the CRT area time series out of the boundaries, generating a new time series that is five times longer than the original one (see Methods). The decomposed interdecadal component of the CRT area after applying the EMD to this prolonged time series shows a significant negative correlation with the PDO in 2002-2022; meanwhile, there exists a weaker residual trend at a rate of 4.16 (± 1.86) × 10^4 km^2/yr ($P < 0.05$) (Extended Data Fig. 5) compared to that calculated from the original time series (Fig. 2d). It suggests that the original 20-year CRT residual trend shown in Fig. 2d contains multi-decadal signal to a large extent. Therefore, the observed westward expansion of the CRT in 2002-2022 looks like mostly a result of naturally occurring, internal variability related to the PDO. The weaker residual trend (4.16 × 10^4 km^2/yr), however, implies a potential linkage of the CRT expanding to climate change, particularly given that the CRT coincides with regions showing the highest signal-to-noise ratio when analyzing climate change-driven trends in ocean color[19]. Nevertheless, a truly decades-long CRT data record, rather than a mathematically prolonged time series, must be established to confidently attribute the expansion of the CRT extent to climate change."

2) About the confidence limit: the fact that you define the confidence of each IMF only from 5 IMF sets makes sense but this is logicaly conservative (because associated to the method itself) and therefore has a tendency to produces small values. Maybe you should use 2x StDev. (95% limit, assuming that the residual distribution is gaussian), which is a bit more conventional.
We now use the 2x standard deviations as the 95% confidence limit. Figures in the revised manuscript have been updated accordingly.

3) Optionally, the very last years of the time series (2-3) could have an important weight in the determination of the trend. What is the trend value without these 2-3 years? It is likely that it will be reduced. This is the same effect that you observed between Extended Fig. 2 b-c.
This suggestion is also reinforce by the fact that the trend of the area is strong towards

the end of the time series.

The trend of the CRT area is undoubtedly affected by the period in which it is calculated. Just for your reference, the expanding rate of the CRT area would be 0.76×10^5 km^2/yr if it is calculated for 2002-2019, compared to a rate of 1.87×10^5 km^2/yr for 2002-2022. However, what we explore in this effort is the trend of the CRT area in 2002-2022. We have now specified the temporal span in the title.

4) nothing is said about the precessing level of MODIS Level-3 data. Are they R2020? (cf oceancolor.gsfc.nasa.gov)

The processing version of the MODIS Level-3 data we used is R2022.0. This is now clearly stated in the text, see line 372 of the revised manuscript.

As detailed bellow, a nice confirmation of the value of the area trend would be to estimate it also from a purely graphical calculation (result will be a very similar value).

The trend of the CRT area from a purely graphical calculation is now provided in the text (1.07×10^5 km^2/yr), which is comparable to that derived from the EMD analysis (1.87×10^5 km^2/yr). See below as well as lines 132-137 of the revised manuscript.

"Based on Fig. 4b, a purely graphical estimation of the expanding rate of the CRT area, by counting the difference in magenta and light blue pixels, is obtained (1.07×10^5 km^2/yr). This estimate is overall consistent with that derived from the EMD analysis (1.87×10^5 km^2/yr), providing an independent confirmation of the estimated expanding rate by the EMD analysis."

Another, more general suggestion is about the definition of a second more quantitative index, that would consists in the average chl-a value over the CRT area that you define. Is this index in phase with the area? (Is it correlated). In all cases, this would a nice complement.

Thank you for the comments! It is indeed an excellent complement to our study. We performed the EMD analysis to MODIS monthly Chl time series and found that the average chlorophyll (Chl) value over the CRT area exhibits a decreasing trend from 2002 to 2022. See the figure below. Note that the overall decreasing Chl trend in the Equatorial Pacific Ocean from the EMD analysis agrees very well with the general understanding of declined ocean productivity under global warming (Gregg et al., 2014). Thus, **changes in the average Chl value over the CRT area are not in phase with the changes in the CRT area**, which is certainly an interesting point and worth further investigation. As our primary focus is on the changes in the CRT area, we decided not to discuss the possible links between the expanding CRT and declining average Chl in this effort.

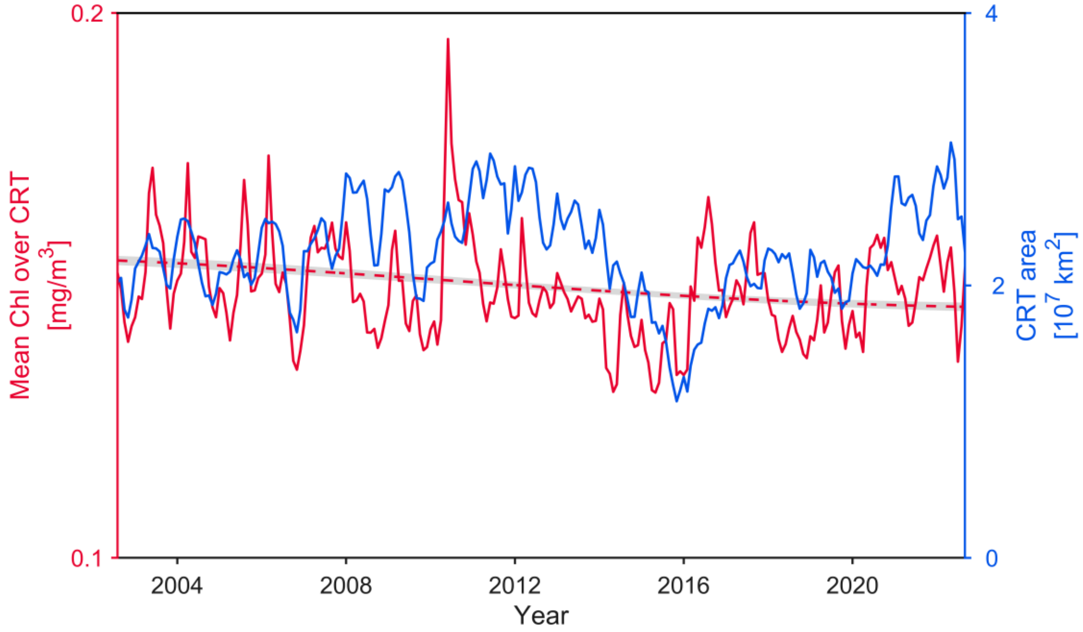


Fig. R1. Monthly series of mean Chl over the CRT area (solid red line) and the CRT area (solid blue line). The red dashed line represents the computed Chl trend by the EMD analysis and the gray shading represents the 95% confidence limit of the trend.

Reference:

Gregg, W. W. & Rousseaux, C. S. Decadal trends in global pelagic ocean chlorophyll: A new assessment integrating multiple satellites, in situ data, and models. J. Geophys. Res. Oceans 119, 5921-5933 (2014).

About trend: What is called a trend (including in EMD) is in fact quite variable (see Moghtaderi et al 2011 "Trend Filtering via Empirical Mode Decompositions"). For these authors, a trend "can contain oscillations while in Wu et al. (2007) only the residual of EMD was deemed a trend, hence constraining it to have no oscillations at all".

You can maybe cite the useful work of Eriksen and Rehman (2007) "Data-driven nonstationary signal decomposition approaches: a comparative analysis" that state for example that "The EMD method is even able to find optimal number of signal modes, K, automatically."

In this study, we examined the nonlinear and adaptive trend of the CRT area, which is defined as an intrinsically fitted monotonic function or a function in which there can be at most one extremum within a given data span of 2002-2022 (Wu et al., 2007). In other words, only the residual component of EMD was deemed a long-term trend, constraining it to have no oscillations in 2002-2022.

Eriksen and Rehman (2007) is now properly cited in the Method section of the revised manuscript (Ref 49 in the revised manuscript, see lines 228).

Detailed Comments:

Introduction

I assume that "Main text" is the "Introduction"?

Corrected.

line 29: " supporting half of the annual global new production" : this is not exact, the estimation by [1] was only 18-56% in 1985 i.e. about 37% +-18% , and this was before more precise satellite-based estimations. Much later "Chavez and Toggweiler (1995) estimated that ... this upwelling supported 18% of global new production. ", more in phase with recent results. Also modifiy it in the abstract.

Revised as suggested. Thank you for pointing this out!

Line 32: ref [3]. You suggests that the surface signature of the equatorial upwelling was discovered in 2023. Please, choose a more suitable reference.

Thank you for your reminder! Here, we introduce the chlorophyll-rich tongue, which is induced by the equatorial upwelling, not the discovery of the upwelling. We have changed the reference to the first report of this tongue that we can find:

Koblentz-Mischke, O.I., Volkovinsky, V.V. and J.G. Kabanova (1970). Plankton primary production of the world ocean. In: Scientific exploration of the South Pacific, W. Wooster (Ed), National Academy of Sciences, pp. 183-193.

Line 32: " the international date line" is not a very convenient way to say 180°E (or 180°W) or the 180th meridian (at equator).

We have replaced “the international date line" with "180°W" in the revised manuscript (see line 24).

Line 46: See impact of the length of the time series, as mentioned above.

As suggested, we have adjusted the data span to an exact yearly cycle from August 2002 to July 2022. All figures and texts have been updated accordingly.

In the RESULTS section:

Westward extension (of the CRT)

The sentence in Lines 51-54 is general and should be integrated in the previous part of the text.

Revised as suggested. See the text in bold fonts below and lines 19-30 in the revised manuscript.

“Owing to the upwelling of nutrient- and carbon dioxide (CO_2)-rich waters, the equatorial Pacific Ocean emerges as the largest oceanic source of atmospheric CO_2, as well as a highly productive area sustaining about 18% of the global oceanic new production[1-3]. The upwelling-supported phytoplankton enrichment forms a tongue of high chlorophyll concentration, spreading westward from the east Peruvian coast to beyond 180°W[4]. **Specifically, the formation of this chlorophyll-rich tongue (CRT)**

is sustained by the local supply of nutrients facilitated by upwelling caused by the trade-wind-driven divergence of the Ekman flow at the equator[5,6], as well as the westward advection of nutrient-rich water via the Southern Equatorial Current (SEC) originating from coastal upwelling off Peru[7]. The extent and intensity of the CRT vary with seasons and years[2], with its interannual variability primarily influenced by El Niño-Southern Oscillation (ENSO)[8-12]"

Lines 70-71: The average seasonal signal of the CRT area (shown in Fig. 2b) do not well support your assertion about the maximum and minimum periods of the equatorial upwelling. The reference #21 do not really confirms this result. There are more adequate references for this (i.e. Phytoplankton carbon and chlorophyll distributions in the equatorial Pacific and Atlantic: A basin-scale comparative study, from Wang et al. and internal references).
Thank you for your reminder! We now present the seasonal pattern of the mean CRT area (shown below and the inserted figure in Fig. 2b of the revised manuscript) and find another peak of the CRT area in January. It looks like the equatorial upwelling is not the sole controlling factor of the seasonal pattern of the CRT area, but the whole equatorial current system works. The reference you suggested has been properly cited. However, we also keep the original reference (Ref 26) in the revised manuscript, in which the seasonal pattern of the West Pacific waters adjacent to the CRT and its association with the equatorial currents were discussed. See the statements below and lines 74-77 of the revised manuscript.

"The seasonal variation of the CRT area highlights peaks of the CRT area in January and May-June and a minimum in October-November (see the insert figure in Fig. 2b). This pattern is in general in phase with the known seasonal cycle of the equatorial current systems[26, 27]."

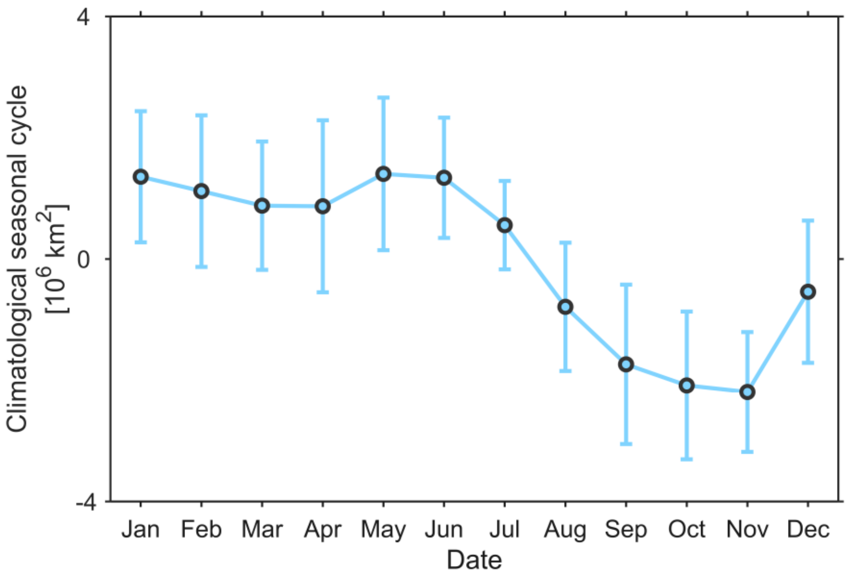


Fig. R2. The monthly mean variation of the CRT area from 2002 to 2022. The standard deviation of the CRT area in each month is represented by the vertical lines.

The rate of increase of the CRT area that you estimate (from a linear fit to the last IMF component) is ~8.46 × 104 km2/yr. This is a major point of your study.
Interestingly, a graphical calculation from your Figure 4 shows that the surface of the expanded area (minus the 2003-2007 one) is quite close to the value of your trend (I estimate it to 1.2 105 km2/yr, which is quite close to you result. This would be a satisfying confirmation, that indirectly shows the adequacy of the EMD to measure it).
I suggests that you do this calculation to confirm you result.
Thank you for this excellent suggestion! In the revised manuscript, we have provided the expansion rate of the CRT area from a purely graphical calculation for proper comparison. See below as well as lines 132-137 of the revised manuscript.

"Based on Fig. 4b, a purely graphical estimation of the expanding rate of the CRT area, by counting the difference in magenta and light blue pixels, is obtained (1.07×10^5 km^2/yr). This estimate is overall consistent with that derived from the EMD analysis (1.87×10^5 km^2/yr), providing an independent confirmation of the estimated expanding rate by the EMD analysis."

Comparatively, the use of the OC-CCI data set is not as important because a composite product is less adequate to compute trends (to place it as supplement is a good choice).
Yes, that is why we chose to present the result of OC-CCI data as a supplement.

A second central result (not original) is that the interannual signal is well correlated to the Multivariate ENSO index (MEI).
Note that this signal is also much higher than the trend itself. This raises the point of the real significance of the trend, especially increasing at the end of the time series, maybe because of the strong niña event during the last two years. This raises the more general impact of the influence of the tails of the series previously mentioned.
This Niña period can severely biases the determination of the trend. This point must be absolutely evaluated and discussed because it could strongly minimise the main message of the paper which is clearly based on a significant trend that maybe do not exists at all.
We agree that the ENSO signal is overwhelming in the equatorial Pacific. Taking your suggestion above, we estimated that the weight of the interannual component is 59% while that of the trend is 9%. That is why using the EMD in this study is very important. The strength of the EMD is its detection of the nonlinear and adaptive trend of a climate variable, which is defined as an intrinsically fitted monotonic function or a function in which there can be at most one extremum within a given data span, as well as its decomposing of signals of different frequencies (Wu et al. 2007). In theory, ENSO signals, with frequencies < 8 years, are separated from the residual trend. Therefore, it is not likely that the La Niña events at the end of the study period may significantly bias the resulting expanding trend from the EMD. Of course, we fully agreed that in trend detection, the span of the data should be well defined, so we have added the temporal span in the title of the revised manuscript and stressed this point in the revised

manuscript (lines 226-229). Also see below.

"Of course, the observed long-term trend of the CRT area highly depends on the data span used for trend analysis[25,49]. Thus, it is necessary to stress that the reported westward extending trend of the CRT is observed within a temporal period of 2002-2022."

Why the result shown in Figure 4. is build only from the boreal summer data?
What would be the results with all data combined?
If the main result of the study is about the existence of a trend in the CRT area and position, this must be better defined. For example, if the "trend" is mostly seasonal (i.e. in summer and not in winter), this has a major significance on the underlying mechanisms, e.g. trend wind variability and/or fluctuations in surface currents.
The westward expansion trend of the CRT is independent of the season. We replaced the boreal summer results with the annual mean CRT distribution in Fig. 4b of the revised manuscript. Previously, we presented the result of boreal summer simply because the CRT is the largest in boreal summer. We thought presenting the results of boreal summer may facilitate a more straightforward illustration of the expansion of the CRT.

Extended Figure 1: what is defined as winter… (Dec-Jan-Feb)? Etc.
Considering that the nutrient and current data do not add much to our result, we have removed the figure from the manuscript.

In support to figure 2: The average seasonal signal is not represented. It could be added to figure 2, for more clarity as well as to support your assertion lines 70-71 about minimum/maximum.
As suggested, the average seasonal pattern has now been added to Fig. 2 (the inserted figure in panel b). Also see below.

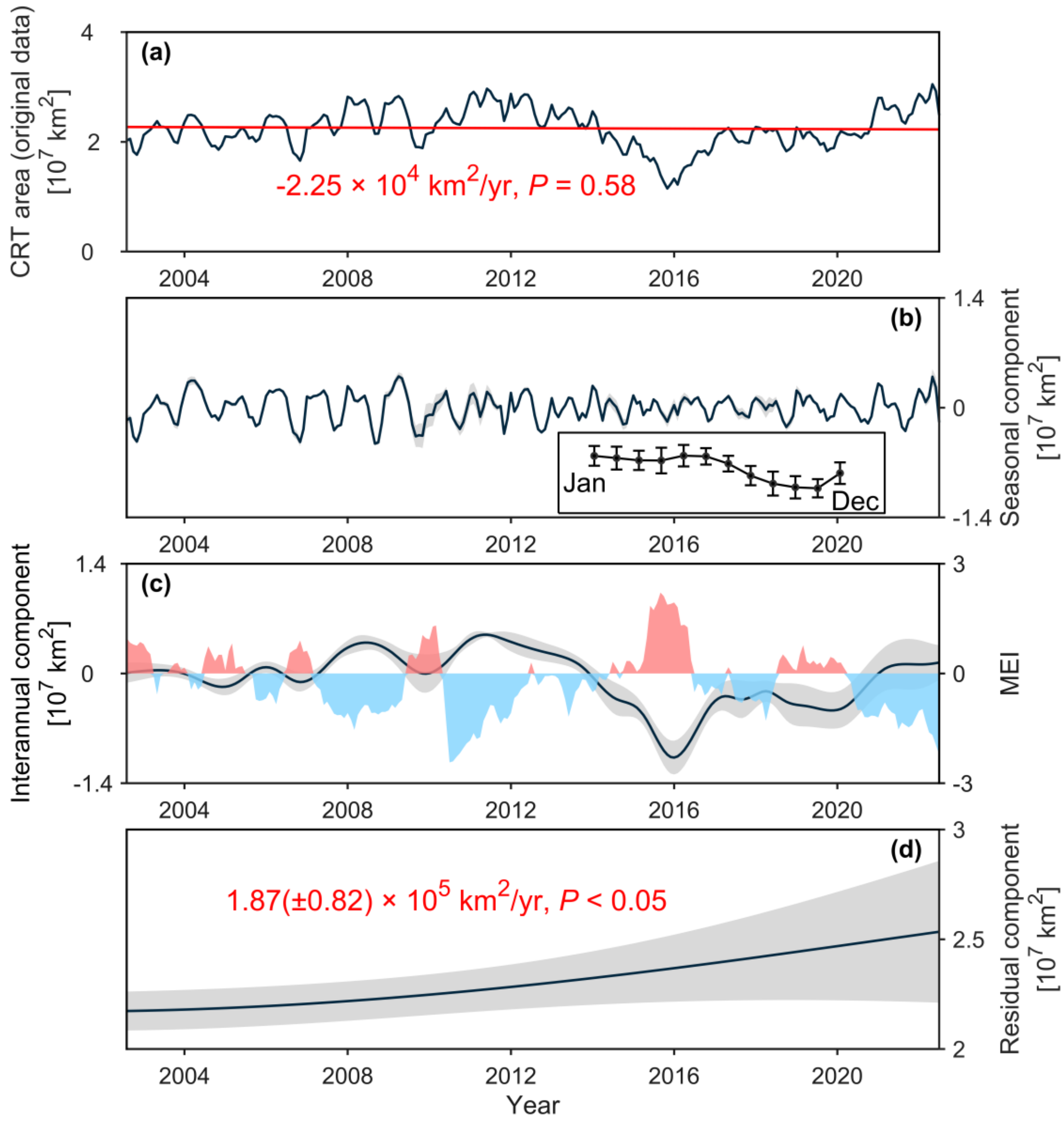


Fig. 2. | Decomposition of the monthly time series of the CRT area using empirical mode decomposition (EMD). (**a**) The original 20-year monthly time series of the CRT area (the solid black line), its linear fitting trend (solid red line), and its decomposition into (**b**) seasonal, (**c**) interannual, and (**d**) residual components. The seasonal pattern of the CRT area is inserted in (**b**), with the solid line and error bar representing the mean CRT area and the standard deviation of each month, respectively. The temporal variation of the Multivariate ENSO Index (MEI) is also included in (**c**), with the red and blue shadings indicating El Niño and La Niña, respectively. The gray shading represents a range of two standard deviations, equivalent to a 95% confidence limit (see Methods).

Discussions and implications

The main results of the work are relevant and are correctly discussed (potential effects or light shading; negative phase of the Pacific Decadal Oscillation).

The limitations of the relatively short (20-year) period is of course a classic limitation in such a study but is adequately discussed.

Thanks!

Lines 344: repetition, to remove.
Removed.

Comments from Reviewer #4:

General Comments:

KEY RESULTS
The manuscript presents a timely and careful analysis of the potential changes in the spatial extent of the so-called chlorophyll-rich tongue (CRT) in the equatorial Pacific Ocean - a prominent feature in an area whose dynamics are of great climatic and ecosystem significance on a global scale. Based on the spatio-temporal analysis of satellite-derived data, the authors conclude that the CRT has expanded westwards over the last 2 decades, providing plausible partial explanations for the physical mechanisms controlling this phenomenon. The manuscript is well written with a clear structure and articulation of interpretation and conclusions drawn.
SIGNIFICANCE
In general the main findings of the paper are interesting and relevant for the scientific marine biogeochemical community. The presented results are potentially significant and appear to address the need for detailed regional analysis of potential climate change driven trends in indicators of ocean ecology, such as the concentration of chlorophyll-a as a proxy for changes in phytoplankton biomass. For example, the area of CRT's westward expansion corresponds well with the area of some of the highest signal-to-noise ratios reported to occur globally when analysing long-term trends in remote-sensing reflectance (Cael et al. 2023).
Thank you for your support and positive comments! The paper of Cael et al. (2023) is now properly cited in the text (Ref. 19, line 32 & line 221). Indeed, after we decompose the multi-decadal variability of the CRT area related to the PDO, there still exists a weaker residual trend (4.16×10^4 km^2/yr) (see our replies to Reviewer #3 and Extended Data Fig. 5 of the revised manuscript). This remaining residual trend could be possibly and/or partly attributed to climate change. See below as well as lines 218-221 of the revised manuscript.

"The weaker residual trend (4.16×10^4 km^2/yr), however, implies a potential linkage of the CRT expanding to climate change, particularly given that the CRT coincides with regions showing the highest signal-to-noise ratio when analyzing climate change-driven trends in ocean color[19]."

At the same time, the manuscript does not provide any specific justification for why the CRT expansion per se represents such an important issue. In lines 42-43 the authors claim that "changes in CRT may have consequences on ecosystems, heat exchanges, air-sea CO2 fluxes, etc. (...)" yet they do not provide any reference to support such claims. The presented study itself also does not include any results to verify such claims. More justification, even if just based on the literature, for why the CRT westward expansion is important is needed. The manuscript should also indicate whether prior observations or modelling studies of this phenomenon were attempted or not.

Thank you for your suggestion! Justifications based on the literature have been added to the revised manuscript. **To our knowledge, there are no prior observations of the CRT westward expansion, and no modeling studies of this phenomenon have ever been attempted**. This has been indicated in the text. See below and lines 41-43, 195-204 & 230-232 of the revised manuscript.

Lines 41-43:

"The changes in CRT may have consequences for ecosystems[9], heat exchanges[24], and air-sea CO_2 fluxes[2], which is especially important to understand amidst ongoing global climate change."

Lines 195-204:

"The expansion of the CRT implies a broader coverage of phytoplankton-rich water along the equator. As phytoplankton are a crucial part of ocean ecosystems, feeding zooplankton and thus living resources at higher trophic levels, the westward extending of the CRT possibly results in large-scale redistribution of certain species, influencing ecosystems and fisheries[9, 42]. Also, as chlorophyll in phytoplankton absorbs solar light, a higher abundance of phytoplankton increases the absorption of solar radiation, leading to an enhanced heating rate at the ocean surface[43]. Therefore, changes in the spatial distribution of the CRT would lead to a redistribution of heat in the surface layer, influencing tropical climate, especially the amplitude and asymmetry of the ENSO[24, 44]"

Lines 230-232:

"Nevertheless, our finding of the CRT westward expansion, never reported in previous observational and modeling efforts, highlights the importance of lower frequency variations hidden under major interannual variability of ENSO in the equatorial Pacific."

VALIDITY

The authors have adequately picked the available data to support the research investigation they set out to perform. The data seems to be interpreted robustly, however, the analysis is not presented in a fully transparent or clear way.

The conclusions are supported by the interpretation of the data. However, there are several critical questions concerning especially: (i) the rationale for and details behind trend detection method applied, (ii) the selection of parts of the original data sets for spatio-temporal analysis, and (iii) the implications of the above for drawing the right conclusions.

Firstly, visual inspection of the original data does not immediately reveal such a strong increasing residual trend as detected through the selected trend detection method. While EMD is a very powerful trend detection method as stated in lines 336-228, the authors do not explain why they choose EMD as opposed to other, perhaps more conventional, time-series analysis methods. Similarly, it would be good to know why the authors chose EMD and not EEMD (ensemble empirical mode decomposition) which according to Wu and Huang (2009) "represents a substantial improvement over the original EMD (...)." Comparison with at least one other trend-detection method could help support the conclusions.

Thank you for your suggestions! We agree that comparison with at least one other trend-detection method could help support the conclusions. We have now provided the rationale for using EMD for trend detection in this study. We first applied the linear trend fitting to the time series of the CRT area, which suggests no statistically significant trend ($P = 0.58$, see the red line in the figure below and Fig. 2a in the revised manuscript). We think it is due to the fact that interannual variability (ENSO activities) is the overwhelming signal in the equatorial Pacific. That is exactly why we chose the EMD method to decompose the original signal into different components, which is powerful in detecting trends and relatively easy to implement.

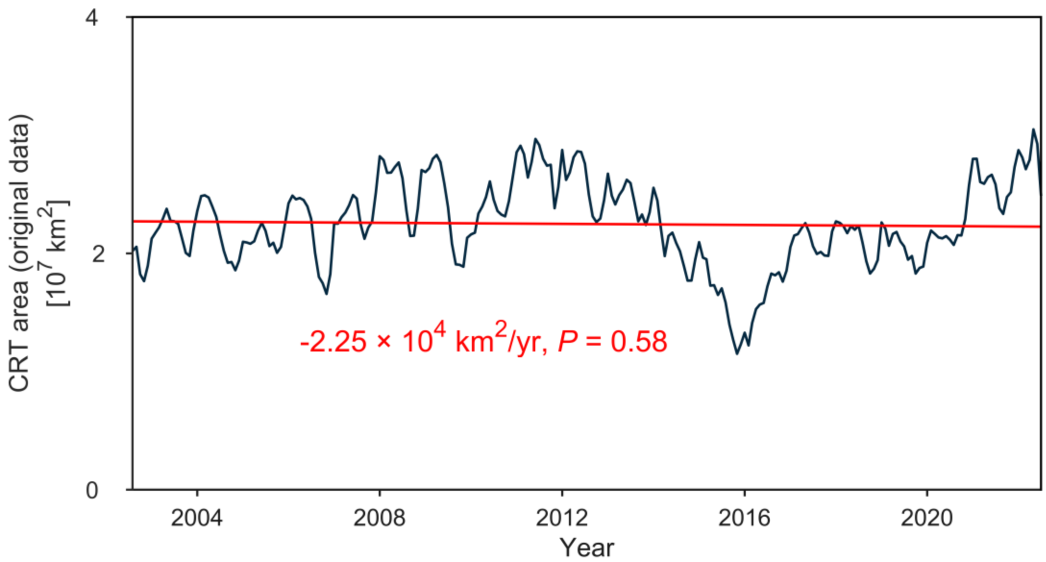
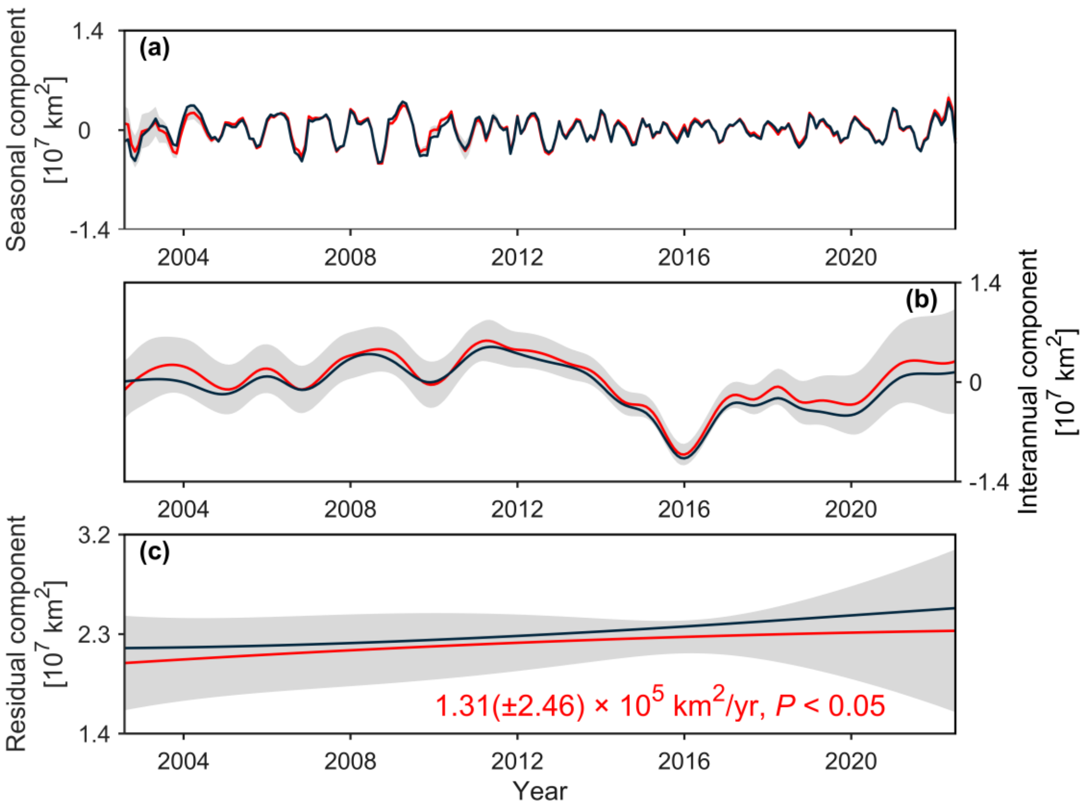


Fig. R3. The CRT area time series (solid black line) and its linear trend (solid red line) with 95% prediction interval (red dotted line).

Per the choice of EMD instead of EEMD, we first attempted EMD and found that it is already efficient for variability and trend detection of the CRT. In the revised manuscript, we also included the results obtained by EEMD in the Extended Data Fig. 3 for proper comparison (See also the figure below). Note that the estimated expanding rate of the CRT area from EEMD (1.31×10^5 km^2/yr) are quite comparable to that of EMD (1.87×10^5 km^2/yr).

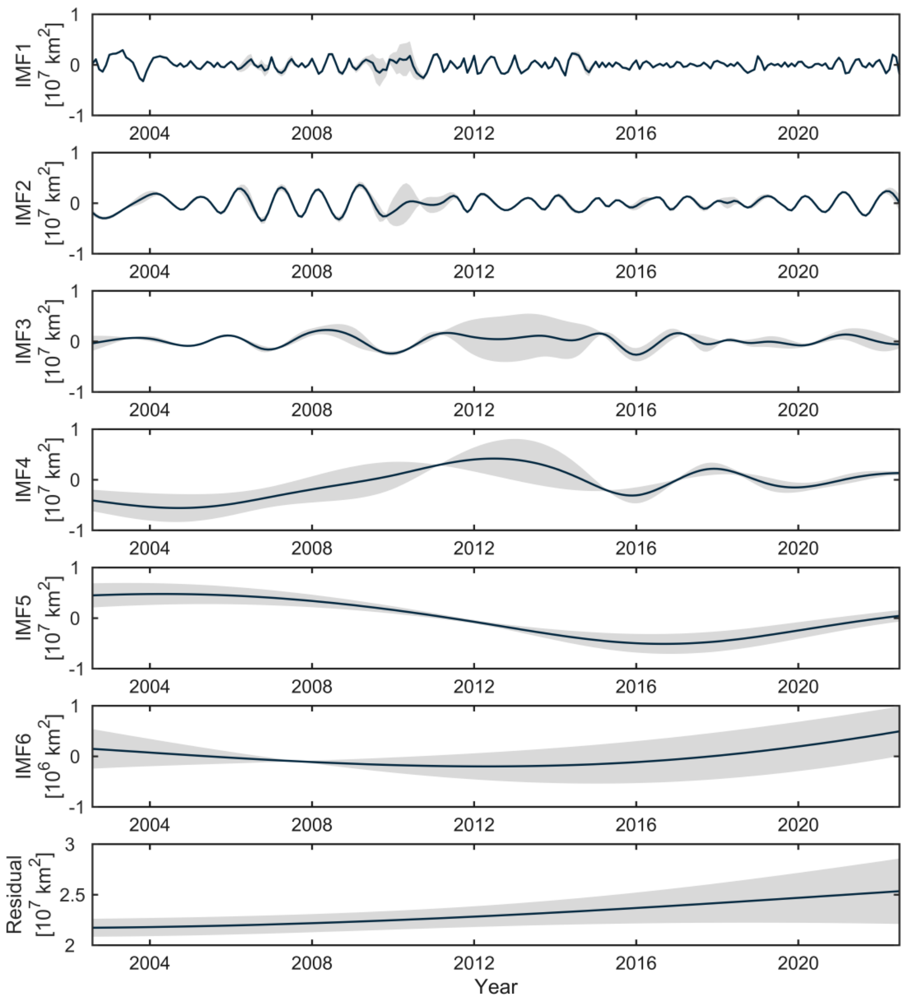


Extended Data Fig. 3. | Decomposition of the monthly series of the CRT area using ensemble empirical mode decomposition (EEMD). The decomposed (**a**) seasonal, (**b**) interannual, and (**c**) residual trend components by EEMD are shown in the red curve compared with the EMD result in the black curve. The gray shading represents two standard deviations, or 95% confidence limit, of the EEMD results.

Secondly, it is not clear why the authors do not take into account the Pacific Decadal Oscillation (PDO) anywhere in their analysis prior to mentioning its significance in lines 178-180. In general, it is surprising to see the lack of any distinct decadal signal explicitly identified in their time series analysis despite the fact that EMD should result in distinct IMF(s) corresponding to low-frequency signal, if present. A visual inspection of Fig.2 (b-c) suggests that the decadal signal related to the PDO may have been mixed within the inter-annual variability signal correlated with the MEI. The authors don't clarify what they mean by inter-annual variability (i.e. period/frequency range) even though EMD should produce IMFs with quite distinct frequency signals, unless combined in post-processing. Similarly, the high-frequency IMF appears to be in fact a mix of at least two IMFs corresponding to the seasonal and intra-seasonal signal. More detailed results of EMD need to be presented and described, at least as extended Data Figures.

You are right. We did combine IMFs in post-processing, details of which have been provided above and in the revised manuscript, as suggested. Specifically, we do not take into account the Pacific Decadal Oscillation (PDO) prior to Lines 178-180 because the time series of the CRT area here in this study only covers 20 years, shorter than a complete cycle of the PDO (Mantua et al., 1997; Zhang et al., 1997), so that it is not reasonable to link the low-frequency IMFs to the PDO. We have now provided the details of the EMD method used in this study (lines 413-422). We also included a new Extended Data Fig. 1 to present the original decomposed IMFs. See below for the revised text and the figure of IMFs.

"Here, in the sifting process of the EMD, the S stoppage was used as the stoppage criterion, producing five sets of IMFs with different S values ranging from 2 to 6 (Extended Data Fig. 1). The IMF1 and IMF2 with an average period < 2 years were combined into the seasonal component of the CRT area. The IMF5 and IMF6, although emerging as low-frequency signals, are most probably a result of over-sifting for such a time series of a limited span (20 years)[25]. Thus, the IMF3 to IMF6 with an average period > 2 years were all combined as the interannual component. Finally, the residual components from these five IMFs were averaged to derive the residual trend of the CRT area. A linear regression was then applied to this residual trend component to obtain the long-term changing rate of the CRT area."



Extended Data Fig. 1. | The original IMFs generated from the EMD analysis of the 20-year time series of the CRT area. The Mean (black lines) and standard deviation (gray shading) of each IMF were obtained from 5 different siftings corresponding to the S stoppage criterion from 2 to 6.

References:

Mantua, N. J. et al. A Pacific interdecadal climate oscillation with impacts on salmon production. Bull. Am. Meteorol. Soc. 78, 1069-1080 (1997).

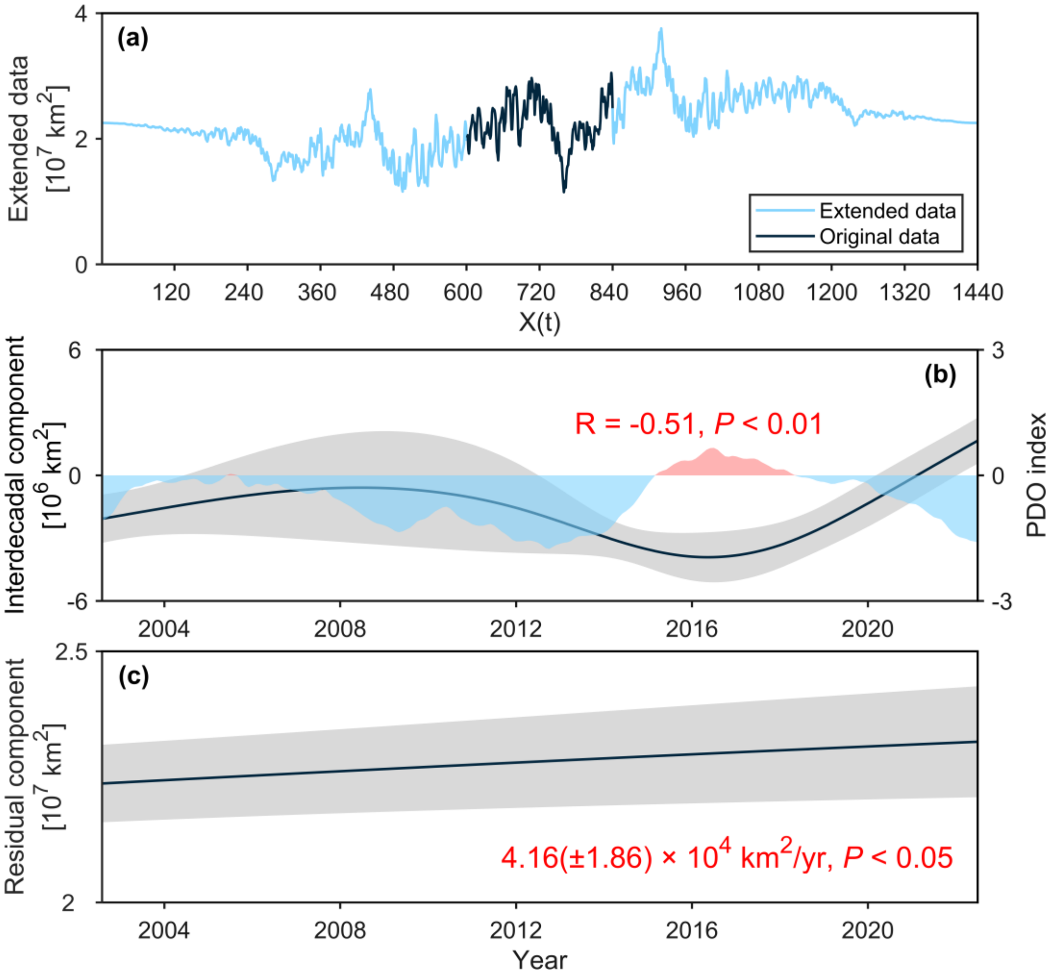
Zhang, Y., Wallace, J. M. & Battisti, D. S. ENSO-like interdecadal variability: 1900-93. J. Clim. 10, 1004-1020 (1997).

While the authors speculate on the potential residual trend being in fact a component of decadal oscillation, they also don't use the full potential of the EMD method to test such a hypothesis. Wu et al. (2007), cited by the authors (ref. #34), talks about the value of evaluating multi-decadal trends as opposed to looking at simple linear trends or the overall adaptive (residual) trend as applied in this manuscript. Additional analysis in that direction appears critical considering the authors' choice of the title which points at a clear long-term trend, even though elsewhere, in lines 175-180, the authors admit that the observed trend is "(...) likely a result of naturally occurring, internal variability (...)"

We agreed, and have now tried to use the full potential of the EMD method to test if the potential residual trend is part of a decadal oscillation component. It is established by following the approach of Stallone et al. (2020) (suggested by Review #3) to generate a long time series of the CRT area that is five times the original span. Then the EMD was carried out on this prolonged time series. The interdecadal component is defined as all the IMFs with an average period greater than 8 years, and is found to be negatively correlated with the low-frequency (>8 years) components of the PDO index ($R = -0.51$, $P < 0.01$) (see the figure below). The remaining residual term ($P < 0.05$) still shows an expansion of the CRT, but with a lower expansion rate (~4.16×10^4 km^2/yr) compared to the EMD analysis of the original time series (~1.87×10^5 km^2/yr). This suggests that the original trend contains multi-decadal variabilities that are highly correlated with the PDO. Specifically, we have added the temporal span of 2002-2022 in the title. In addition, it is important to note that the extended data out of the original time series boundaries is produced from a mathematical approach (Stallone et al., 2020) and thus lacks physical meaning. Therefore, we use these new results only as supporting evidence for our conclusions and for discussing the link of CRT changes to the PDO and climate change. See below, lines 205-218 of the revised manuscript and the Extended Data Fig. 5.

"On the other hand, it is noticeable that our study period of 2002-2022 aligns with a mostly negative phase of the Pacific Decadal Oscillation (PDO)[45-47]. To explore potential links of the low-frequency variabilities of the CRT to the PDO, we adopted a mathematical approach of Stollen et al. (2020)[48] to symmetrically extrapolate the CRT area time series out of the boundaries, generating a new time series that is five times longer than the original one (see Methods). The decomposed interdecadal component of the CRT area after applying the EMD to this prolonged time series shows a significant negative correlation with the PDO in 2002-2022; meanwhile, there exists a weaker residual trend at a rate of $4.16 (\pm 1.86) \times 10^4$ km^2/yr ($P < 0.05$) (Extended Data

Fig. 5) compared to that calculated from the original time series (Fig. 2d). It suggests that the original 20-year CRT residual trend shown in Fig. 2d contains multi-decadal signal to a large extent. Therefore, the observed westward expansion of the CRT in 2002-2022 looks like mostly a result of naturally occurring, internal variability related to the PDO."



Extended Data Fig. 5. | Decomposition of the extended time series of the CRT area. (**a**) The extended monthly time series of the CRT area (blue line) and the original data (black line). (**b**) The interdecadal and (**c**) residual trend components. The red and blue colors in (**b**) represent the positive and negative values of the PDO index, respectively. The gray shading represents the 95% confidence limit.

Detailed Comments:

More detailed comments and suggestions for improvements are listed below under Data and Methodology.

DATA & METHODOLOGY

Please add significance values (p-values) to all the trends plotted in the paper to see if they are significant.

The p-values of all calculated trends have been added to related figures.

Please add a plot/plots to see gradual evolution of CRT area through the years. Your conclusions can be a part of inter-annual variability, or a method artifact which is difficult to track with the way you visualise the data.

We have drawn a figure similar to the Arctic sea ice evolution (Francis and Wu, 2020) as you suggested. The new figure shows that the CRT is expanding from year to year (see the figure below). An inserted figure in the new figure shows a comparison between the two periods of 2003-2007 and 2018-2022 of Fig. 4 in the original manuscript. This new figure has been added to the revised manuscript as Fig. 4a.

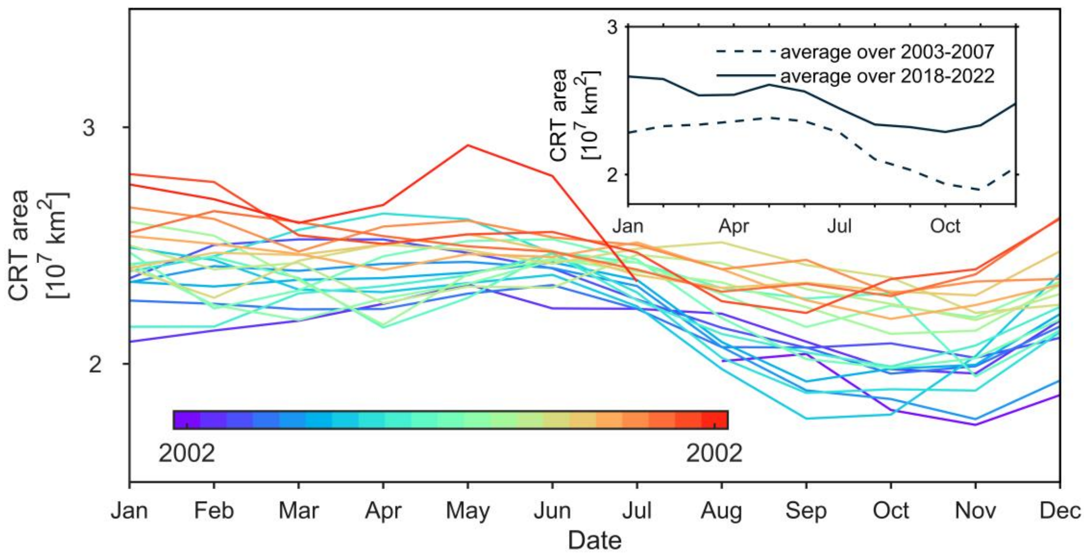


Fig. R4. Evolution of the monthly CRT area from 2002 to 2022. The insert figure shows the comparison between the average annual cycle in 2003-2007 (the black dashed line) and that in 2018-2022 (the solid black line).

Lines 34-35: Please add some references. There are past global or regional studies on temporal evolution of chlorophyll in the equatorial Pacific (e.g. the already mentioned Cael et al., 2023).

The following references are now properly cited in the revised manuscript (see Lines 30-32).

"The long-term variation in the CRT remains unclear, despite notable changes in atmospheric circulation, ocean currents, and ecosystems observed in the equatorial Pacific[13-19]."

References:

Cael, B.B., Bisson, K., Boss, E. *et al.* Global climate-change trends detected in indicators of ocean ecology. Nature **619**, 551–554 (2023). https://doi.org/10.1038/s41586-023-06321-z

Zhai, D., Beaulieu, C., & Kudela, R. M. (2024). Long-term trends in the distribution of ocean chlorophyll. Geophysical Research Letters, 51, e2023GL106577.

https://doi.org/10.1029/2023GL106577

Lines 47-48: delete "if any" or expand what you mean by having no forcing behind such an observed change. "Ocean currents data" instead of "current data." What about the effects of cooling/warming? Have you considered looking at parallel changes in satellite sea surface temperature? If not, why?

Thank you! We deleted "if any" as suggested.

We did not consider looking at the changes in sea surface temperature (SST). The CRT forms owing to upwelling and the westward advection. Also, the Chl-SST relationship is complex and nonlinear (*e.g.*, Cael et al., 2023; Dunstan et al., 2018). We think it is more straightforward to look at the driving factors, *i.e.* the ocean current data.

Reference:

Dunstan, P. K. et al. Global patterns of change and variation in sea surface temperature and chlorophyll a. Scientific Reports 8, 14624 (2018).

Lines 53-54: The reader might be convinced that westward advection of "nutrient-rich water" stimulates phytoplankton growth in the region, however, it is not nitrate which controls phytoplankton growth in much of the equatorial Pacific. Silicate and iron are the growth-limiting nutrients, whose supply via upwelling and passage of Tropical Instability Waves moderates much of the production in the area. Also, the role of the upwelling of nutrients via the Equatorial Undercurrent has been clearly established on top of the redistribution via the SEC. See for example Nelson & Landry (2011) and the references therein to several related articles in that special issue. Please clarify why you are looking at nitrate in this study, and not any other nutrients, especially since the presented nutrient climatology data is not subject to any further analysis.

We agree that Fig. 1c could be misleading. Also, considering that it is not subject to any further analysis, we have removed the nutrient climatology from Fig. 1 (see the figure below as well) and revised the manuscript accordingly (lines 24-28 in the revised manuscript). The references you suggested (Nelson & Landry (2011) and the references therein) are properly cited now (see lines 53-55).

Lines 24-28:

"Specifically, the formation of this chlorophyll-rich tongue (CRT) is sustained by the local supply of nutrients facilitated by upwelling caused by the trade-wind-driven divergence of the Ekman flow at the equator[5,6], as well as the westward advection of nutrient-rich water via the Southern Equatorial Current (SEC) originating from coastal upwelling off Peru[7]."

Lines 53-55:

"Clearly, within the boundary of the CRT are the poleward equatorial surface water divergence and the westward SEC (Fig. 1b)."

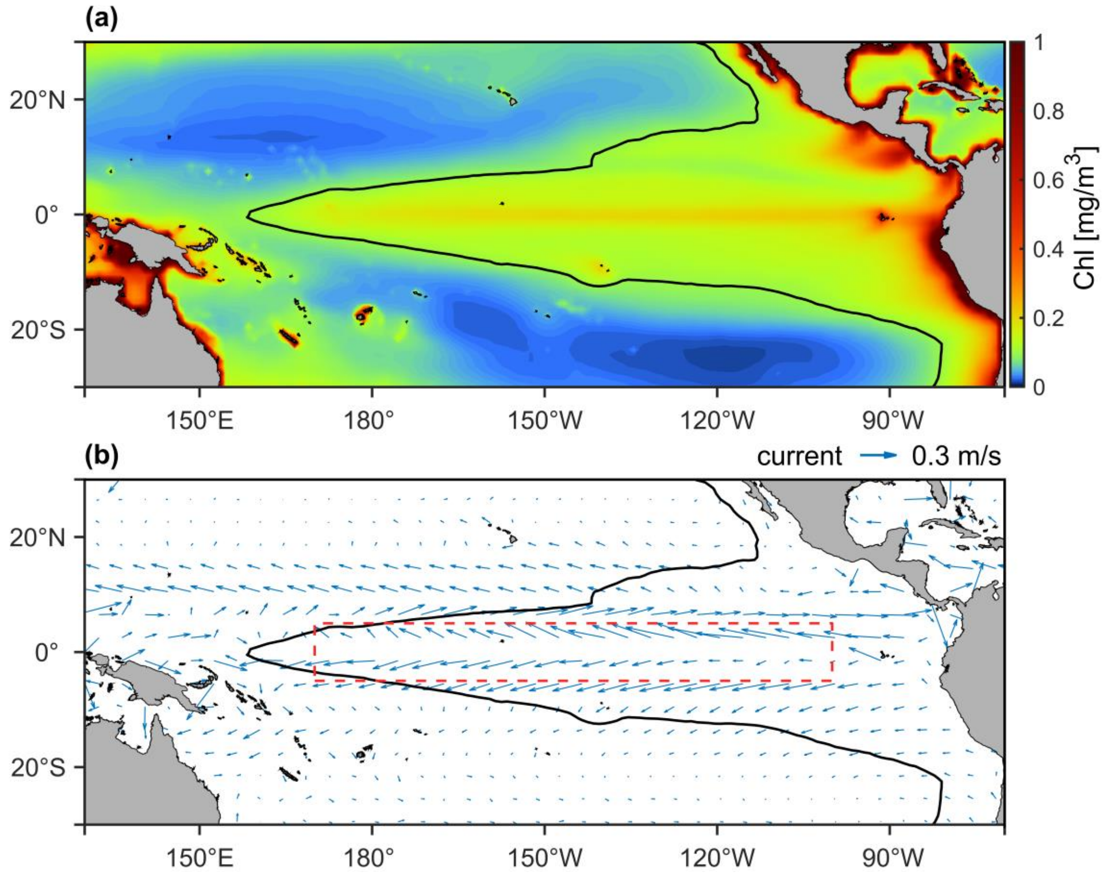


Fig. 1. | Illustration of the chlorophyll-rich tongue (CRT) and spatial distributions of ocean current fields in the equatorial Pacific. Climatological mean (**a**) surface chlorophyll concentration (Chl, mg/m^3) and (**b**) current fields (m/s) between August 2002 and July 2022. The CRT climatology boundary is delineated with the black curve, representing the Chl isoline of 0.1 mg/m^3. The area enclosed by the red dashed lines in (**b**) denotes the central positions of the South Equatorial Current (5° S-5° N, 170° E-100° W).

Figure 1: Caption title mentions nutrients but you are only showing nitrate concentrations.

The nutrient climatology has now been removed from Fig. 1, as have replied above.

Figure 2: Please add labels (years) to all panel x-axes. All panels have different scales on the y-axes. Panels b and c should have the same ranges, or if not, explain why in the caption. It is not clear whether the presented EMD results are the original IMFs or combined IMFs with a broader frequency range.

Labels (years) have been added to all panel x-axes. Panels b and c have been modified to the same y scale. We have added a figure to show the original IMFs generated directly from the EMD results in the Extended Data Fig. 1. More details of combined IMFs have been added in the Method section (see lines 415-422 of the revised manuscript).

Line 91: The rate of increase does not include any uncertainty estimate. Fig. 2 includes confidence limits which should be used to report uncertainty along with the final result.

Also, the choice of MODIS-Aqua vs OC-CCI dataset and period considered (extended Data Fig. 2) very strongly affects the magnitude of the trend. Please take that into account when reporting the final result and associated uncertainty.
Uncertainty estimates have been added in the text and relevant figures. The uncertainties are calculated as the standard deviation of the computed trend from the five sets of EMD analysis with various S stoppage criteria.

Lines 109-114: Why is this analysis limited to the boreal summer? Would be helpful to know the seasonal evolution of the extension of CRT. Is it expanding uniformly throughout the year? Why/why not?
As suggested by the other two reviewers, we have changed Fig. 4 to show the comparison of the annual mean CRT area between two periods (2003-2007 vs 2018-2022), instead of the comparison of the mean CRT area in boreal summer. Previously we chose to present the result of boreal summer simply because the CRT is the largest in boreal summer. We thought presenting the results of boreal summer may facilitate a clearer illustration of the expansion of the CRT.

Figure 4: Why do you only show these specific time frames (2003-2007 and 2018-2022)? Based on visual inspection of Figure 2a), choosing CRT area for e.g. 2010-2014 vs 2016-2020 would result in an opposite conclusion, i.e we will see the CRT area decreasing. It would be helpful to see the gradual evolution of the CRT area through the years. I suggest to make the graphs and/or maps showing evolution from year to year or one map with isolines showing i.e. evolution of maximum CRT area from year to year, like in studies of i.e. Arctic Sea Ice evolution - see for example Figure 2 in Francis and Wu (2020) or Figure 2 here (https://ccin.ca/ccw/seaice/future).
Many thanks for this constructive suggestion! As suggested, we have drawn a figure similar to the Arctic Sea ice evolution (Francis and Wu, 2020). Note that **we remove the dominated interannual component** (it explains 59% of the CRT variability as estimated above in our response to Reviewer #3) **from the time series to highlight the evolution of the CRT throughout the year**. This new figure has been added to the revised manuscript as Fig. 4a, and the original Fig. 4 in the manuscript has been replaced with the results using the annual mean CRT area as Fig. 4b (see also the figure below).

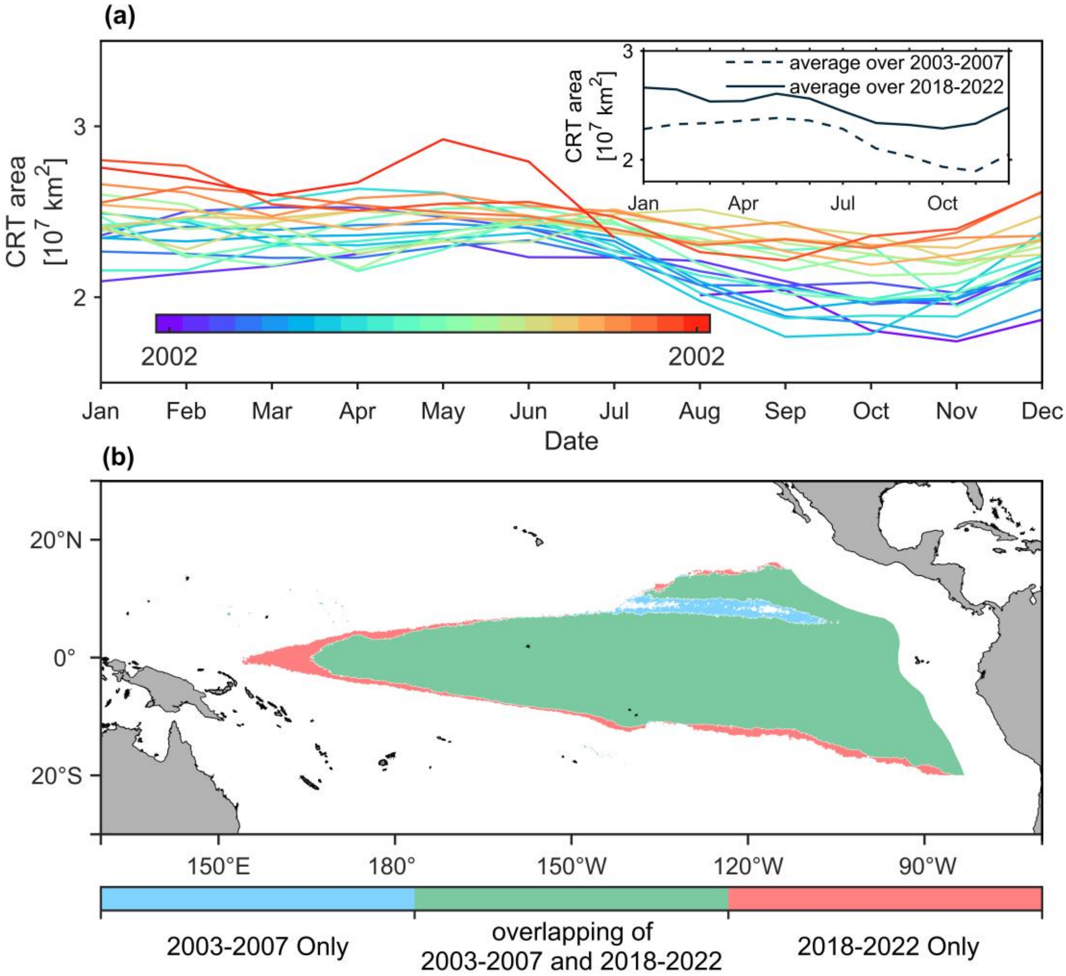


Fig. 4. | Illustration of the westward extension of the chlorophyll-rich tongue (CRT). (**a**) Annual cycle of the non-interannual component of the CRT area time series. The colored lines are for years from 2002 to 2022. The black dashed line averaged over 2003-2007, and the solid black line averaged over 2018-2022 are shown in the subfigure. (**b**) Comparison of the CRT area spatial distribution. The CRT area averaged over 2003-2007 and 2018-2022 are highlighted in light blue and magenta, respectively. Green areas represent the overlapped CRT area in these two periods. The monthly Chl products over the respective five years were first averaged and then used to obtain the CRT area for the two periods.

We agree that selecting different temporal spans for the comparison may lead to the opposite conclusion, as **the increase of the CRT area is not monotonic throughout the 20 years** from 2002 to 2022. Selecting the first and last five years for the comparison was somehow arbitrary, as our main purpose was to illustrate the changes in the CRT area better and more straightforwardly. Interestingly, when we removed the contribution from the interannual components, the average CRT area in 2016-2020 was still greater than that in 2010-2014 for almost every month (see the figure below), suggesting the overall expanding trend of the CRT. This result, on the other hand, confirms your suggestion regarding the necessity of including a figure showing the gradual evolution of the CRT area throughout the years.

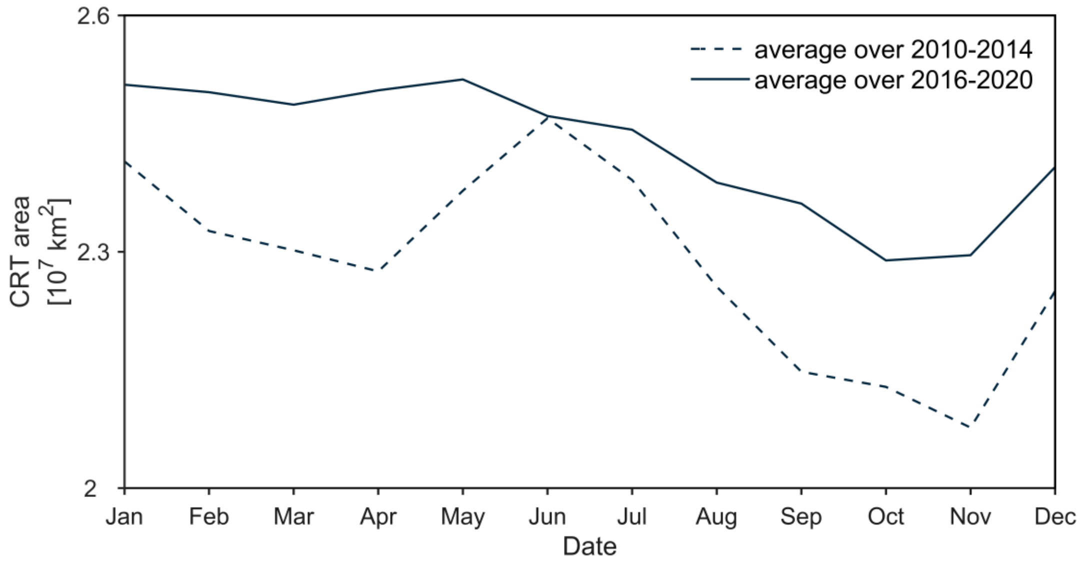


Fig. R5. Annual cycle of the non-interannual component of the CRT area time series in 2010-2014 (the black dashed line) compared to that in 2016-2020 (the solid black line).

Lines 132-133: the statement "reveals an increasing trend" sincere there seems to be a decreasing trend in Figure 5b.

Negative values in Figure 5b indicate that the zonal South Equatorial Current (SEC) is westward. Thus, a decreasing trend in Figure 5b indicates an increasing trend of westward zonal SEC.

Line 163: I would delete "detailed"

Revised as suggested.

Line 174: heat? Could you give examples from previous studies of how much change in chlorophyll-rich areas can influence ocean heat budget?

Phytoplankton biomass can significantly impact the way in which incident solar radiation is absorbed in the mixed layer and the vertical penetration down into the subsurface layers. By using a simplified coupled atmosphere-ocean model, Timmermann and Jin (2002) showed that phytoplankton can reduce La Nina amplitude by about 1.5 - 2 °C through negative biological feedback. Zhang et al. (2011) revealed the opposite effect of phytoplankton of the penetrative solar radiation flux out of the bottom of the mixed layer (Q_{pen}) in the central and eastern equatorial Pacific, where the interannual Q_{pen} variability can be significantly enhanced by more than 20% and reduced by less than 10%, respectively. We have expanded this sentence accordingly. See below for the revisions as well as lines 199-204 in the revised manuscript.

"Also, as chlorophyll in phytoplankton absorbs solar light, a higher abundance of phytoplankton increases the absorption of solar radiation, leading to an enhanced heating rate at the ocean surface[43]. Therefore, changes in the spatial distribution of the CRT would lead to a redistribution of heat in the surface layer, influencing tropical climate, especially the amplitude and asymmetry of the ENSO[24, 44]."

References:

Timmermann A, Jin FF. Phytoplankton influences on tropical climate. Geophys. Res. Lett. **29**, (2002).

Zhang RH, Chen DK & Wang GH. Using Satellite Ocean Color Data to Derive and Empirical Model for the Penetration Depth of Solar Radiation (H_p) in the Tropical Pacific Ocean. J. Atmos. Oceanic Technol., **28**, 944-965 (2011).

Lines 177-178: "It is difficult, if not impossible, to predict if the CRT will continue to expand beyond 2022" - if you want to mention further predictions please cite the studies forecasting spatial chlorophyll evolution after 2022. Numerous modelling studies like this exist with varying degrees of certainty.

We have deleted this sentence which indeed does not add much to this study.

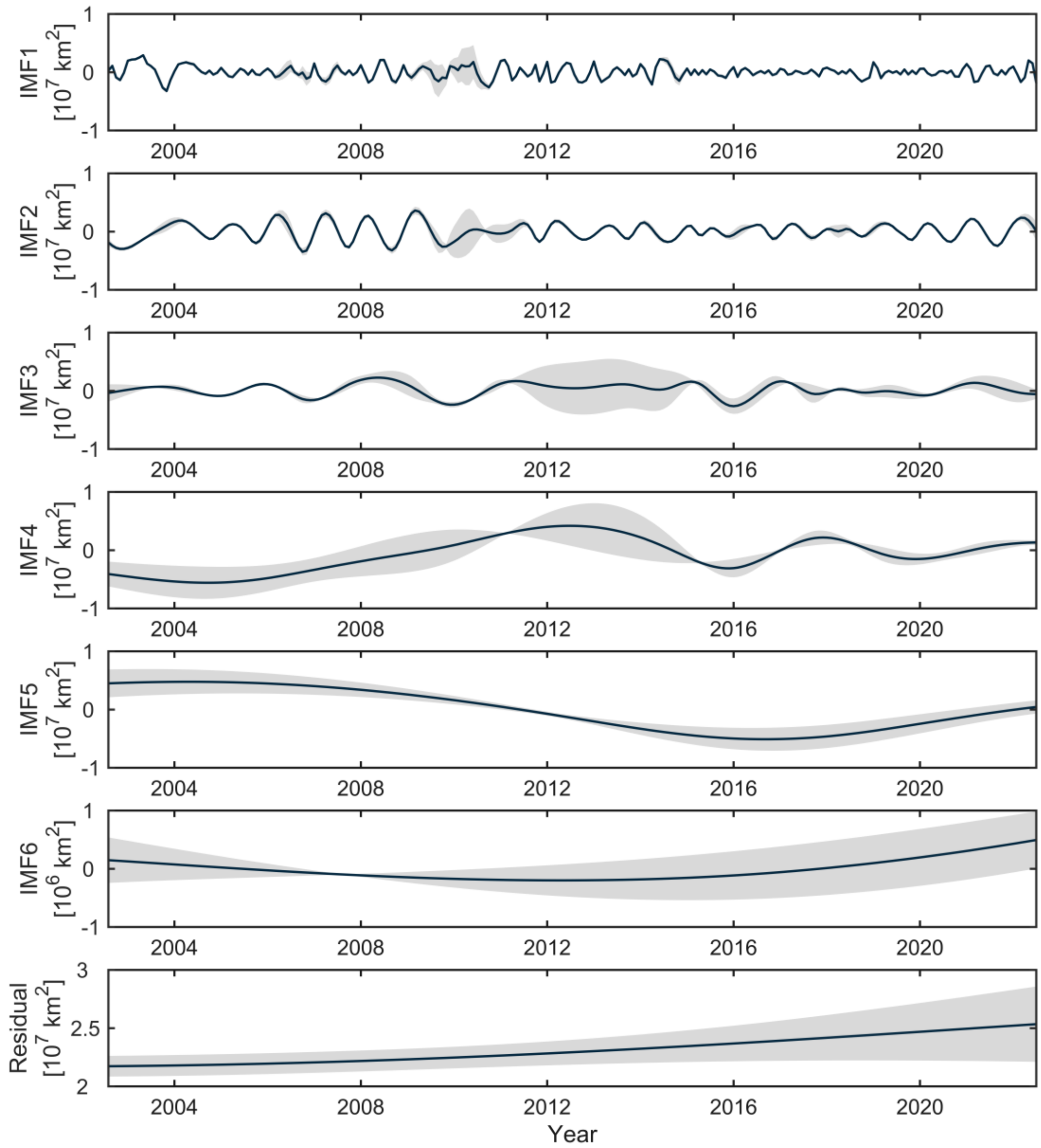
Line 336: efficiency instead of efficacy

Revised as suggested.

Lines 338-349: This is a general description of EMD but not a description of the exact procedure applied by the authors in this study. In particular, how many IMFs were generated for each time series using a single S criterion, and at what characteristic frequencies? Was there any mixing of IMFs prior to presenting the results in Fig. 2? If so, how and on what grounds? This information is crucial to ensuring reproducibility of the analysis.

The description of the exact EMD procedure applied in this study has been provided in the Method section (lines 413-427, also see below). We have added a figure to show the original IMFs generated from the EMD decomposition (see the Extended Data Fig. 1 in the manuscript, and the figure below).

"Here, in the sifting process of the EMD, the S stoppage was used as the stoppage criterion, producing five sets of IMFs with different S values ranging from 2 to 6 (Extended Data Fig. 1). The IMF1 and IMF2 with an average period < 2 years were combined into the seasonal component of the CRT area. The IMF5 and IMF6, although emerging as low-frequency signals, are most probably a result of over-sifting for such a time series of a limited span (20 years)[25]. Thus, the IMF3 to IMF6 with an average period > 2 years were all combined as the interannual component. Finally, the residual components from these five IMFs were averaged to derive the residual trend of the CRT area. A linear regression was then applied to this residual trend component to obtain the long-term changing rate of the CRT area. In addition, the confidence limit serves as a standard measure for assessing the outcomes of EMD[56], which is defined as a range of standard deviations from the equally valid IMF sets. In this effort, the standard deviation was computed from five residual trend components, each generated with a different S stoppage criterion, with two standard deviations equivalent to a 95% confidence limit."



Extended Data Fig. 2. | The original IMFs generated from the EMD analysis of the 20-year time series of the CRT area. The Mean (black lines) and standard deviation (gray shading) of each IMF were obtained from 5 different siftings corresponding to the S stoppage criterion from 2 to 6.

Lines 352-353: Wu et al. (2007), your reference #34, includes a very useful discussion on other types of trends which might be relevant for this case, with large likelihood that the presented residual trend is in fact a component of the long-term natural variability related to the PDO.

Agreed, as have replied to your previous comment.

Lines 354-358: Consider also the white noise assisted EEMD method (Wu and Huang, 2009) as an alternative to provide explicit confidence intervals around each set of IMFs, even for a single S criterion.

The EEMD (Wu and Huang, 2009) analysis was attempted as suggested. The decomposed components with confidence limits are now shown in the Extended Data Figure 4, and the corresponding descriptions have been added in the manuscript (lines 101-105& 408-411, see also below). Note that the estimated expanding rate of the CRT area from EEMD (1.31×10^5 km^2/yr) are quite comparable to that of EMD (1.87×10^5 km^2/yr).

Lines 101-105:
"Furthermore, application of the ensemble empirical mode decomposition (EEMD) method[34] to the CRT area time series in 2002-2022 results in a similar expanding rate (1.31×10^5 km^2/yr) (see Extended Data Fig. 3). Hence, there is a high degree of certainty on the expansion of the CRT from 2002 to 2022."

Lines 408-411:
"There is an upgraded version of the EMD called the Ensemble Empirical Mode Decomposition (EEMD), which is relatively complex, aiming to solve potential drawbacks of the EMD by sifting an ensemble of white-noise added signal[34]."

Line 360: Did I understand it right that World Ocean Atlas 2018 has nitrate data for 2002-2022 that you use in the analysis (see your Fig.1 caption)? Also, it is not nutrient data, but nitrate data only.
We have removed nutrient data from our revised manuscript as have replied above.

REFERENCES USED IN THE REVIEW COMMENTS

Cael, B.B., Bisson, K., Boss, E. et al. (2023). Global climate-change trends detected in indicators of ocean ecology. Nature 619, 551–554; doi: 10.1038/s41586-023-06321-z

Francis, J. and Wu, B., (2020). Why has no new record-minimum Arctic sea-ice extent occurred since September 2012? Environmental Research Letters, 15; doi: 10.1088/1748-9326/abc047

Nelson, D. M., & Landry, M. R. (2011). Regulation of phytoplankton production and upper-ocean biogeochemistry in the eastern equatorial Pacific: Introduction to results of the Equatorial Biocomplexity project. Deep Sea Research Part II: Topical Studies in Oceanography, 58(3-4), 277-283; doi: 10.1016/j.dsr2.2010.08.001

Wu, Z., & Huang, N. E. (2009). Ensemble empirical mode decomposition: a noise-assisted data analysis method. Advances in adaptive data analysis, 1(01), 1-41; doi:10.1142/S1793536909000047
Thank you for your help! These papers are now properly cited in the revised manuscript.

Comments from Reviewer #5

"I co-reviewed this manuscript with one of the reviewers who provided the listed reports (Dr Artur Palacz). This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts."

Thank you for your supports!

Responses to Comments on Manuscript

Preface:

We thank the reviewers for their positive comments on our revisions, and their additional comments/suggestions for further improving the quality of this manuscript. As follows, we provide our point-by-point responses to address each of the concerns by the reviewers. The reviewers' comments are presented in black fonts, with our replies in light blue.

Point-by point responses

Comments from Reviewer #3:

The manuscript has been significantly improved from the its first draft and my previous suggestions have been correctly answered, as well as those from the other reviewers. The EMD is more precisely explained in the method section, specially regarding frequency combinations.
The importance of ENSO influence (through MEI) is better discussed.
I thank the authors for having tested the EEMD method on their data set (extended Fig. 3.), that shows a similar trend, although 30% lower and with a higher theoretical uncertainty.
We appreciate the positive comments from the reviewer.

I personally regret that the various contributions of the seasonal, interannual and trend components (respectively 32, 59 and 9%) are not explicitly mentioned, at least in the results because this is just a very interesting quantification, independently of the scope of the paper, more about trends.
The relative contributions of the seasonal, interannual, and trend components are now explained in the text. Please see Lines 73-76 in our revisions.

Maybe because of the better timing, the Western/southern expansions of the CRT also look spatially more "regular".
Indeed, timing is important, as Wu et al. (2007) stated, "the trend should exist within a given data span".

Title: I suggest: "Ocean's largest …. westward (2002-2022)" (not "in"). Alternatively replace "in" by "during".
The title is now revised as "Ocean's largest …. westward (2002-2022)" as suggested.

Abstract:

Because the nitrate mapping part (and "enrichment" considerations) have been removed, I do not think that the mention of "Our findings imply a broader cover of productive water along the equator" it still relevant. This sentence is acceptable but a bit general...You be the judge.

Here, productive water simply refers to the water with a higher Chl concentration (Chl > 0.1 mg m^{-3}), not the water enriched with nutrients. So, we decided to keep this statement.

Second point for the Abtract: I personally think that the simultaneous extension of the Pacific gyres, expanding poleward (as already described in the literature) is an important point that should be briefly added in the abstract, because this result is not so straightforward in this new context.

Added as suggested. See below and Lines 14-16 in the revised manuscript.

"Interestingly, EMD analysis on central locations of the Pacific gyres suggested simultaneous extension of the gyres and the CRT during 2002-2022, with the gyres extending poleward."

Introduction: OK, a bit improved from the last version

Thank you!

Westward extension

The fact that the surface of the CRT is computed for all months of data (not only in summer) improves the quality and stability of the results, numerically and graphically. Alternatively it shows a much higher positive extension trend and I imagine that you were satisfied by this result.

Yes, thanks a lot for your very constructive comments to help us improve the quality and stability of our results!

Strengthening of trade winds and upwelling:

I do not have any particular comment, the Ekman transport is now more adequately explained (thanks to another reviewer).

Thank you!

Detailed corrections (Note: line numbers refers to the last pdf version of the document)

- Figure 1b: it would be great to place the current names (SEC, NEC, NECC) and corresponding main stream (arrows).
- Extended data Fig. 2: move the Y axis of Fig. (b) on the left.

Revised as suggested.

- Extended data Fig. 4: I do not understand why you increase the length of the arrows, they do not represent the actual changes! (sorry I did not paid attention to that before).

Indeed, the length of the arrows does not represent the actual changes. We simply use the arrows as an illustration of the extending direction. The arrows in Extended Data Fig. 4 have now been shortened.

- In the whole text: it would be more precise to replace “Equatotial current” by “South Equatorial Current”

We use the term of “equatorial current system” when the description/discussion is not solely related to the South Equatorial Current.

Line 77: Reference 27 (Wang et al. 2013) is not dealing at all with the Eq. Current system. It must be removed or replaced.

Reference 27 has been removed.

About EMD decomposition:

A personal question: the trend of the Westward extension has more than doubled from (8.46 × 104 km2/yr to 1.87 (± 0.82) × 105 km2/yr), Is it only explained by the fact that you consider now all months of the year instead summer months only?

By the way, the interannual component, that also is higher than in the previous calculation which makes sense because the MEI is supposed to be stronger in boreal winter.

The trend obtained previously (0.85×10^5 km^2/yr) was calculated from CRT data of “August 2002 – September 2022”, while the latest result ($1.87 (\pm 0.82) \times 10^5$ km^2/yr) was obtained from CRT data of “August 2002 – July 2022”. Data of all months of the year was considered either in the previous trend decomposition or in the latest one. This change of data span was in response to your previous comment regarding our methods:

“By choosing the 20-year period "August 2002 -September 2022" period you added two extra months to the period, compared to a multiple of 12 months. This introduces a seasonal bias in the time series, especially when computing the trend. You should consider the "August 2002 -July 2022", or any number of exact yearly period, for example the "January 2003 -December 2022" period”.

Lin 117: I would remove “ considering that 1° at the equator corresponds to ~111.2 km” (quite well known)

Removed as suggested.

Discussions and implications

I am sorry but “the CRT in the equatorial Pacific did not shrink but expanded from 2002 to 2022” is not exactly a question but an assertion. Please correct it, in one way or another.

Revised as suggested. See below and Lines 197-199.

“The above analysis offers a compelling answer to the proposed question regarding the changes of the CRT in the equatorial Pacific, that the CRT did not shrink but expanded from 2002 to 2022”

Line 208: Stallone (instead of Stollen)
Corrected. Thank you for reminding!

Comments from Reviewer #4

I would like to thank the authors for a careful and detailed response to the original comments provided by all reviewers, and for implementing the suggested modifications. I think that the authors have done a very good job in providing additional information about their methods of analysis, and presentation of results. The manuscript has in consequence improved in quality significantly.
We appreciate the positive comments from the reviewer.

However, I still have concerns about the overall interpretation of the time series analysis results, and in particular, about drawing consistent conclusions about the allegedly observed westward expansion of the CRT vs concluding that the expansion is part of a lower frequency oscillation related to the PDO. A clear and consistent message is missing from the current version of the manuscript. Below please find detailed comments focusing on identifying those sections of the manuscript where there are major inconsistencies or lack of clarity in such interpretation.
I recommend that these issues be resolved prior to publication.
Thank you! We provided point-to-point responses to your concerns in the following.

Throughout the manuscript there appear inconsistencies in referring to interannual, interdecadal (e.g line 434, Extended Data Figure 5) and multidecadal (e.g. line 431) signals. For instance, in line 70-71, an abritrary frequency threshold of >8 years was selected for an internannual signal. Should such a signal not be called "interannual to decadal"? On page 19, lines 418-419, interannual is defined as signal >2 years…. In lines 446-448 authors talk of extracting low-frequency (>8 years) signal from the PDO index. I'm convinced that early on in the manuscript the authors needs to clearly explain how they define these different frequency signals and whether they are applied consistently across the different methods of time series analysis.
We defined different frequency signals based on a comparison of the frequencies in the IMFs and the conventional definitions of climatic variabilities. Climatic variabilities are often featured by multiple time scales. Interannual signals may refer to variabilities with frequencies of >1 yrs, or 5-8 yrs; quasi-decadal/decadal/multidecadal signals are with frequencies > 8 yrs, and these terms are often used interchangeably, generally referring to low-frequency variabilities (Keerthi et al., 2022; Serio et al., 2023; Ji et al., 2024). The IMF5 and IMF6 with an average period > 8 years emerge as low-frequency signals. However, they are most probably a result of over-sifting for such a time series of a limited span (20 years) (Wu et al., 2007). Thus, the IMF3 to IMF6 with an average period > 2 years were all combined as the interannual component. Later in the EMD analysis of the prolonged time series (120 years), the IMFs with an average period > 8 years, which contain physically meaningful signals, were extracted as decadal component. In summary:
For the original 20-year time series analysis, seasonal component: < 2 years; interannual component: > 2 years (the IMFs > 8 years were combined into it);
For the prolonged 120-year time series analysis, seasonal component: < 2 years; interannual component: > 2 years; decadal component: > 8 years;

We have now tried to clearly explain how we define these different frequency signals in the caption of Fig. 2 and in the Method section. We note that they are applied consistently across the different methods of time series analysis. See below as well as Lines 432-438 and 453-457 of the revised manuscript:

Fig. 1. | Decomposition of the monthly time series of the CRT area using empirical mode decomposition (EMD). (**a**) The original 20-year monthly time series of the CRT area (the solid black line), its linear fitting trend (solid red line), and its decomposition into (**b**) seasonal, (**c**) interannual, and (**d**) residual components. Variabilities of high-frequency (< 2 year) were combined as the seasonal component; the components of low-frequency (> 8 years) were combined into the interannual component, because the 20-year time span of the CRT area time series inhibits the decomposition of decadal variability with physical meaning. The seasonal pattern of the CRT area is inserted in (**b**), with the solid line and error bar representing the mean CRT area and the standard deviation of each month, respectively. The temporal variation of the Multivariate ENSO Index (MEI) is also included in (**c**), with the red and blue shadings indicating El Niño and La Niña, respectively. The gray shading represents a range of two standard deviations, equivalent to a 95% confidence limit (see Methods).

"The IMF1 and IMF2, with an average period < 2 years, were combined into the seasonal component of the CRT area; the IMF3 and IMF4, with an average period > 2 years, were combined into the interannual component; and the IMF5 and IMF6, with an average period > 8 years, emerging as the low-frequency signals, are most probably a result of over-sifting for such a time series of a limited span (20 years), and thus were combined with the IMF3 and IMF4 as the interannual component[26, 28, 55]."

"The IMFs with an average period > 8 years contained physically meaningful signals for such a 120-year time series; they were no longer combined with the IMFs with an average period > 2 years (interannual component), but were extracted as decadal component (Extended Data Fig. 5)."

Reference:

Keerthi, M. G., Prend, C. J., Aumont, O. & Lévy, M. Annual variations in phytoplankton biomass driven by small-scale physical processes. *Nat. Geosci.* **15**, 1027-1033 (2022).

Serio, C., Montzka, S. A., Masiello, G. & Carbone, V. Trend and Multi-Frequency Analysis Through Empirical Mode Decomposition: An Application to a 20-Year Record of Atmospheric Carbonyl Sulfide Measurements. *J. Geophys. Re. Atmo.* **128** (2023).

Ji, K., Yu, JY., Li, J. *et al.* Enhanced North Pacific Victoria mode in a warming climate. *npj Clim. Atmos. Sci.* **7**, 49 (2024).

p. 4, lines 41-43: "The changes in CRT may have consequences for ecosystems, heat exchanges, and air-sea CO2 fluxes, which is especially important to understand amidst ongoing global climate change." - If possible, please specify the details of such consequences, better with numerical approximations i.e. "potential expansion of CRT was shown to increase (or decrease) the heat budget in xx area xx times and increase (or decrease) the CO2 uptake from the atmosphere xx times". Please put relevant numbers instead of "xx" from the literature you cite.

Here we just wanted to highlight the significance for investigating a changing CRT in the tropical Pacific. Its potential consequence is discussed to some extent in the manuscript though being qualitative (Lines 195-204 in the revised manuscript). A quantification of the exact impact on the heat budget or CO_2 fluxes using numerical simulations or models is out of the scope of this study. Nevertheless, we included one sentence as follows (see Lines 216-217),

"Certainly, further quantification of these consequences of expanding CRT would desire more dedicated efforts."

p.4 line 67: the phrase "relatively easy to implement" is not precise and does not add anything meaningful to the justification for choosing this method. I suggest to remove or revise this statement.

Removed as suggested.

p.4 lines 69-77: This text includes many contradictions. What is the difference between "long-term" and "multi-decadal" in the context of this manuscript? I suggest that here the authors only mention that the focus of the analysis is on distinguishing between seasonal and interannual variability and the residual trend which may or may not be part of a longer, multi-decadal cycle - something which cannot be unambiguously determined from a 20-year time series. Parts of this text are also already included in the Fig. 2 caption. Information about combining IMFs can also be moved there I think.

We removed the use of "long-term" and "multi-decadal" to avoid confusion, and revised this paragraph and moved the information about combining components to Fig. 2 caption, as suggested. Thank you!
See below, and Lines 69-73 as well.

"The EMD, which is powerful in detecting trend[25], was then carried out to decompose the CRT area time series into seasonal and interannual components as well as a residual trend component, as the focus of this analysis is on distinguishing between these variabilities and the residual trend (see Methods and Extended Data Fig. 1)."

Fig. 2. | **Decomposition of the monthly time series of the CRT area using empirical mode decomposition (EMD)**. (**a**) The original 20-year monthly time series of the CRT area (the solid black line), its linear fitting trend (solid red line), and its decomposition into (**b**) seasonal, (**c**) interannual, and (**d**) residual components. Variabilities of high-frequency (< 2 year) were combined as the seasonal component; the components of low-frequency (> 8 years) were combined into the interannual component, because the 20-year time span of the CRT area time series inhibits the decomposition of decadal variability with physical meaning. The seasonal pattern of the CRT area is inserted in (**b**), with the solid line and error bar representing the mean CRT area and the standard deviation of each month, respectively. The temporal variation of the Multivariate ENSO Index (MEI) is also included in (**c**), with the red and

blue shadings indicating El Niño and La Niña, respectively. The gray shading represents a range of two standard deviations, equivalent to a 95% confidence limit (see **Error! Reference source not found.**).

p. 6 line 96: Please define the "remaining residual component" which is not defined by the authors until this point.
It has now been defined in Lines 69-73:

"The EMD, which is powerful in detecting trend[25], was then carried out to decompose the CRT area time series into seasonal and interannual components as well as a residual trend component, as the focus of this analysis is on distinguishing between these variabilities and the residual trend (see Methods and Extended Data Fig. 1)."
Also, to keep consistency, we removed the word 'remaining'.

p. 6 lines 101-105: Why is there no error bar on the EEMD-derived residual trend reported here if it is given in Extended Data Fig. 3? The phrase "high degree of certainty" seems inadequate considering that the presented EEMD results reveal possible lack of trend within the 95% confidence limit. The p-value of <0.05 only refers to the average trend.
The error bar has now been added on the EEMD-derived trend. Also, the phrase "high degree of certainty" here is not related to the EEMD result itself, but to conclude that results from different methods and different data all confirm the expanding of the CRT. To avoid confusion, we have changed wording. See Lines 113-114 of the revised manuscript, and as below:

"Taken together, there is a high degree of confidence on the expansion of the CRT from 2002 to 2022."

Figure 4: both colorbar labels are for 2002, I would suggest the last one should be "2022"
Thanks for pointing it out! It was a typo. Corrected.

p. 12 lines 214-223: Up until this point the reader is left under the impression that the detected trend is that of steady increase across the two decades under study. This paragraph contradicts the results presented in the previous sections. In my view, the conclusion of this paragraph must be reported much earlier in the text.
The conclusion of this paragraph is based on a mathematically extended CRT time series that is five times longer than the original 20-year time series, following the approach of Stallone et al. (2020). According to Wu et al. (2007), the original 20-year time series is too short to enable physically meaningful separation of decadal signals; thus, in the previous sections there is no way to explore whether the detected CRT trend is related to a natural variability index such as PDO. We think this paragraph does not contradict the results presented in the previous sections. The detected trend is indeed that of seemingly steady increase across the two decades under study, but this steady increase contains decadal signal according to our new analysis on a mathematically prolonged time series. The detected trend here is as defined in Wu et al. (2007): "the

trend is an intrinsically fitted monotonic function or a function in which there can be at most one extremum within a given data span”. To avoid confusion, we have now added two sentences in the previous sections (Lines 114-116 in the revised manuscript), and revised the text in this discussion paragraph correspondingly (Lines 218-221 in the revised manuscript), also as below:

“Of course, it must be noted that whether the residual trend may be part of a longer decadal cycle cannot be unambiguously determined from a 20-year time series.”

“On the other hand, it is noticeable that our study period of 2002-2022 aligns with a mostly negative phase of the Pacific Decadal Oscillation (PDO)[45-47], suggesting that the detected expansion trend of the CRT may possibly contain low-frequency variabilities.”

p. 19 lines 408-412: I don’t understand what is meant by phrases “relatively simple” or “relatively complex” when describing EMD and EEMD. Also, to be precise, EEMD was only applied to one dataset (CRT expansion) while EMD applied to all time series used in the study. Differences between the results of EEMD and EMD are not sufficiently explained in my view.
Thanks for the suggestion! We have removed the phrases “relatively simple” or “relatively complex” when describing EMD and EEMD, and have clearly reported that the EEMD was only applied to one dataset (CRT expansion) while EMD was applied to all time series used in the study. See Lines 424-429 of the revised manuscript and below:

“There is an upgraded version of the EMD called the Ensemble Empirical Mode Decomposition (EEMD), aiming to solve potential drawbacks of the EMD by sifting an ensemble of white-noise added signal[34]. In this study, the EMD was attempted for all the time series involved in the CRT analysis. The EEMD was only carried out with the CRT area time series for comparison.”
In addition, since the EMD analysis here in this study has been efficient in trend detection, and the results of EEMD are simply used as a support to the EMD results, differences between the results of EEMD and EMD, which are out of the focus of this study, are not explained.

Extended Data Figure 1: I wish that all panels used the same scale of 10^7 km2. Otherwise, it is difficult to compare IMF6 to the rest of the signals. Based on this figure, I still struggle to understand why the authors focus on reporting a residual trend rather than talk about a very pronounced interannual to decadal signal, with a clear westward expansion of the CRT over the last 10 years, coincident with a negative phase of the PDO.
Corrected as suggested. The same scale of 10^7 km^2 has now been used for all panels. Thank you!
As mentioned above, according to Wu et al. (2007), **the 20-year time span of the CRT area time series inhibits the decomposition of decadal variability with physical**

meaning, and thus any further discussion of potential decadal signals. Also, we indeed seek for a changing tendency, as indicated in the Introduction: "It thus prompts a crucial question: is the CRT in the equatorial Pacific, located right in-between the expanding North and South Pacific Subtropical Gyres, inevitably shrinking?" And that is why we choose the EMD method which is powerful for trend detection.

Extended Data Figure 3: The authors fail to comment on the wide confidence interval over the last 7-8 years of the study period, including uncertainty over the direction of the residual trend. Is this related to over-sifting when using certain S stoppage criteria?
The wide confidence interval over the last 7-8 years of the study period is not related to over-sifting when using certain S stoppage criteria. It is related to the added white noise to the original data.
The confidence interval is two standard deviation of the 800 times ensemble EMD results of the original data with random white noise added. Therefore, a wide standard deviation emerged almost throughout the whole data span, but only narrowed down around 2015/2016 (a strong ENSO event) when the interannual signal is strong enough to cover the effect of white noise. We have now added one sentence for a brief discussion. See Lines 110-112 of the revised manuscript and below:

"The confidence interval appears wide over most of the study period due to a higher standard deviation, which results from random white noise added in the EEMD analysis and therefore is not material to the direction of the residual trend."

Extended Data Figure 5: Why not report the trend in the same unit as above, i.e. in 10^5 km^2? That would make comparisons much easier. Why would the average residual trend be >4 times smaller than reported e.g. on Extended Figure 2 (a)? Were the IMFs combined differently here? In fact, these should render exact same results. I don't understand where the difference comes from.
As suggested, to facilitate comparison, all the estimated CRT trends are now in the same unit of 10^5 km^2. Thank you!
The trend shown in Extended Data Figure 5 is the result of applying EMD to a mathematically prolonged CRT area time series. The prolonged time series enables separation of decadal signals with frequency > 8 years. As have indicated in the Method section (Lines 453-456 of the manuscript), **"the IMFs with an average period > 8 years contained physically meaningful signals for such a 120-year time series; they were no longer combined with the IMFs with and average period > 2 years (interannual component), but were extracted as decadal component".**
On the contrary, in the analysis of the original 20-year CRT time series (results shown in Fig. 2), according to Wu et al. (2007), **the 20-year time span of the CRT area time series inhibits the decomposition of decadal variability with physical meaning.**
Therefore,

1) In one part, "the IMF5 and IMF6, with an average period > 8 years, emerging as the low-frequency signals, are most probably a result of over-sifting for such a time series of a limited span (20 years), and thus were combined with the IMF3 and

IMF4 as the interannual component".

2) Another part of decadal signals may be contained in the residual component by EMD of the original time series, as indicated in Lines 114-116: "Of course, it must be noted that whether the residual trend may be part of a longer decadal cycle cannot be unambiguously determined from a 20-year time series".

In one word, the IMFs were indeed combined differently in the two EMD analysis, and a weaker residual trend in Extended Data Fig. 5 compared to that calculated from the original time series (Fig. 2d) suggests that the original 20-year CRT residual trend shown in Fig. 2d contains decadal signal to a large extent.

Comments from reviewers #5:

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Thank you for your support!

Responses to Comments on Manuscript NCOMMS-24-19672

Preface:

We thank the reviewers for their positive comments on our revisions, and their additional comments/suggestions for further improving the quality of this manuscript. As follows, we provide our point-by-point responses to address each of the concerns by the reviewers. The reviewers' comments are presented in black fonts, with our replies in light blue.

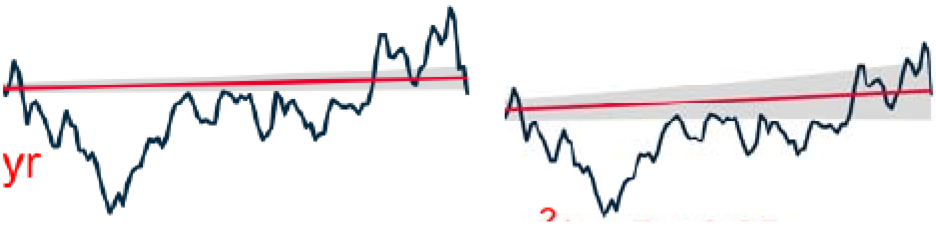
Point-by-point responses

Comments from Reviewer #3:

I am satisfied with all the corrections done.
My detailed comments are in the document enclosed "498598_2_rebuttal_9887878_smsjf9_Answer-Reviewer3.docx"
Thank you for your valuable comments! We have moved your detailed comments in the document enclosed below and provided point-by-point responses.

I think that you can do the same for the Extended Fig. 3; Sorry.
Following your suggestion, we have moved the Y axis of Fig. (b) to the left.

OK, the data span is now corrected, as suggested. I thought you used previously only summer months but this was not the case, sorry.
So, the difference in the trend is effectively **ONLY** due to the new (correctly balanced) period (August to July), by removing the two last months.
A closer look to your initial results (with the two extra months), shows that the new trend remains moderate and unchanged from 2002 to 2014 (a 12-year period), cf figure bellow. At the opposite, the last third of the time series (2015-2022) shows a much stronger trend increase (allowed by its non-linearity). The second point is the strong increase of the 95% confidence area.



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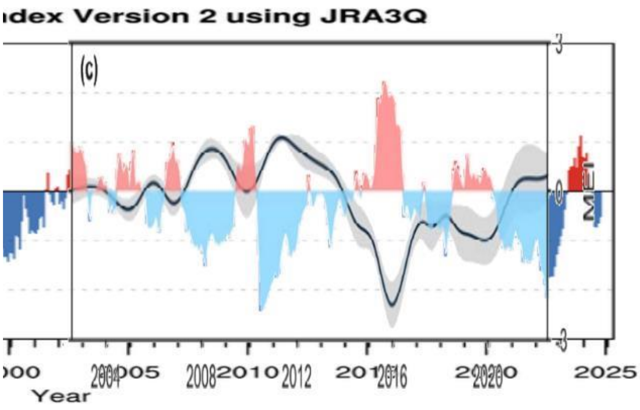


I think that this particular point (increasing uncertainty) must be pointed out because it is very likely (see the last MEI data above) that this uncertainty significantly decrease (and probably the trend of the CRT as well) in the last two years i.e. after your time series.

We appreciate very much that you take a very careful look into the details of the data. We, however, to avoid possible impact of a further discussion about the increasing uncertainty at the end of the time series on the integrity of the whole story, currently chose not to introduce new data out of our study period. We will certainly keep updating this time series of the CRT and MEI, watching the changes of uncertainties with time. We agree that this is a very interesting phenomenon deserved further investigation.

In the other hand, we are now in a very strong phase of the PDO.
CAUTION: This remark may not requires changes in the MS except if you want to highlight the importance of the queue distributions effect in a time series when estimating trend.
I see that you added a new sentence (lines 114-116): "Of course, it must be noted that whether the residual trend may be part of a longer decadal cycle cannot be unambiguously determined from a 20-year time series." also better discuss the results in this respect.
Agreed!

Line 219: Suggestion: "mostly negative phase" could be replaced by "more negatives phases". In particular we are at the moment (2024) in a very strong negative phase of the PDO (cf last data at ncei), probably at a maximum of the strongest ever observed. Furthermore, the phasing already observed between ENSO and PDO is rather decreasing in the recent years, adding variability.
Whatever you do, the discussion is already now quite clearer in this respect.
Revised as suggested.

Also you new sentence (lines 216-217) "Certainly, further quantification of these consequences of expanding CRT would desire more dedicated efforts." is useful.
Thank you!

Also add at line 463: "v2" to ‘The bi-monthly Multivariate ENSO index’
Added as suggested.

Comments from Reviewer #4:

I would like to thank the authors for carefully considering my remarks, and for the detailed responses. Overall, I'm very satisfied with the presented responses, and the corresponding changes to the manuscript. Additional explanations and edits to some sections improved the clarity of the manuscript in my view.
At this point I only have very few comments, most of which are of editorial nature. The authors may consider them to further improve the text. Otherwise, I recommend the manuscript for publication.
Thank you very much for your help!

----- Detailed comments -----

Lines 9-10: Please change "chlorophyll" to "chlorophyll-a" as in line 49, and add "satellite" after "MODIS-Aqua", as in line 50.
Revised as suggested.

Lines 127-130: I suggest to move these two sentences till after line 162, as a concluding statement of that entire section which encompasses also the pixel-based analysis of spatial expansion. Otherwise, such a broad conclusion appears premature.
Agreed! Moved as suggested.

Lines 213-215: This sentence is awkward and could possibly read better if "namely" and "rather" or similar words were added. E.g.:
"The above analysis offers a compelling answer to the proposed question regarding the changes of: the CRT in the equatorial Pacific, **namely** that the CRT did not shrink but **rather** expanded from 2002 to 2022.
Revised as suggested.

Lines 255-258: Very complex sentence, hard to follow. Consider rephrasing as for example this way:
"The ENSO-CRT area correlation observed in this effort and in prior studies [2,24,29] suggests that, if the occurrences of consecutive La Niña events increase under global warming as projected [14], the CRT will probably further extend to the west."
Revised as suggested. Thank you very much!

Comments from Reviewer #5:

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Thank you for your support!

Responses to Comments on Manuscript

Preface:

We thank the reviewers for their positive comments on our revisions, and their additional comments/suggestions for further improving the quality of this manuscript. As follows, we provide our point-by-point responses to address each of the concerns by the reviewers. The reviewers' comments are presented in black fonts, with our replies in light blue.

Point-by point responses

Comments from Reviewer #3:

The manuscript has been significantly improved from the its first draft and my previous suggestions have been correctly answered, as well as those from the other reviewers. The EMD is more precisely explained in the method section, specially regarding frequency combinations.
The importance of ENSO influence (through MEI) is better discussed.
I thank the authors for having tested the EEMD method on their data set (extended Fig. 3.), that shows a similar trend, although 30% lower and with a higher theoretical uncertainty.
We appreciate the positive comments from the reviewer.

I personally regret that the various contributions of the seasonal, interannual and trend components (respectively 32, 59 and 9%) are not explicitly mentioned, at least in the results because this is just a very interesting quantification, independently of the scope of the paper, more about trends.
The relative contributions of the seasonal, interannual, and trend components are now explained in the text. Please see Lines 73-76 in our revisions.

[Rev.3] Thank you for that, perfect.

Maybe because of the better timing, the Western/southern expansions of the CRT also look spatially more "regular".
Indeed, timing is important, as Wu et al. (2007) stated, "the trend should exist within a given data span".
OK, yes.

Title: I suggest: "Ocean's largest …. westward (2002-2022)" (not "in"). Alternatively

replace “in” by “during”.
The title is now revised as “Ocean’s largest …. westward (2002-2022)” as suggested.
OK, good for me if publisher also agree.

Abstract:
Because the nitrate mapping part (and “enrichment” considerations) have been removed, I do not think that the mention of “Our findings imply a broader cover of productive water along the equator” it still relevant. This sentence is acceptable but a bit general...You be the judge.
Here, productive water simply refers to the water with a higher Chl concentration (Chl > 0.1 mg m^{-3}), not the water enriched with nutrients. So, we decided to keep this statement.
No problem for me.

Second point for the Abtract: I personally think that the simultaneous extension of the Pacific gyres, expanding poleward (as already described in the literature) is an important point that should be briefly added in the abstract, because this result is not so straightforward in this new context.
Added as suggested. See below and Lines 14-16 in the revised manuscript.

“Interestingly, EMD analysis on central locations of the Pacific gyres suggested simultaneous extension of the gyres and the CRT during 2002-2022, with the gyres extending poleward.”
Perfect.

Introduction: OK, a bit improved from the last version
Thank you!

Westward extension
The fact that the surface of the CRT is computed for all months of data (not only in summer) improves the quality and stability of the results, numerically and graphically. Alternatively it shows a much higher positive extension trend and I imagine that you were satisfied by this result.
Yes, thanks a lot for your very constructive comments to help us improve the quality and stability of our results!

Strengthening of trade winds and upwelling:
I do not have any particular comment, the Ekman transport is now more adequately explained (thanks to another reviewer).
Thank you!

Detailed corrections (Note: line numbers refers to the last pdf version of the document)

- Figure 1b: it would be great to place the current names (SEC, NEC, NECC) and corresponding main stream (arrows).
- Extended data Fig. 2: move the Y axis of Fig. (b) on the left.

Revised as suggested.

OK, checked.

I think that you can do the same for the Extended Fig. 3; Sorry.

- Extended data Fig. 4: I do not understand why you increase the length of the arrows, they do not represent the actual changes! (sorry I did not paid attention to that before).

Indeed, the length of the arrows does not represent the actual changes. We simply use the arrows as an illustration of the extending direction. The arrows in Extended Data Fig. 4 have now been shortened.

OK, this is a detail, but this is better.

- In the whole text: it would be more precise to replace “Equatotial current” by “South Equatorial Current”

We use the term of “equatorial current system” when the description/discussion is not solely related to the South Equatorial Current.

OK, you are right. Good for me.

Line 77: Reference 27 (Wang et al. 2013) is not dealing at all with the Eq. Current system. It must be removed or replaced.

Reference 27 has been removed.

OK.

About EMD decomposition:

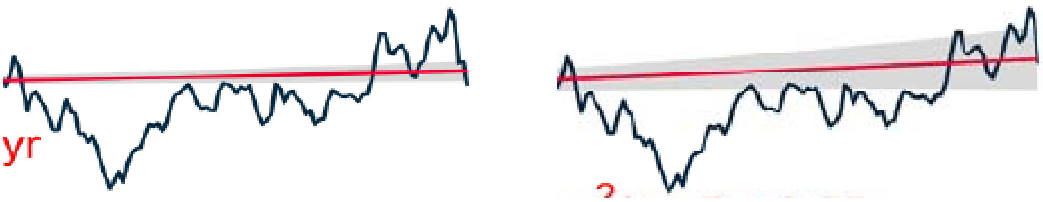
A personal question: the trend of the Westward extension has more than doubled from (8.46 × 104 km2/yr to 1.87 (± 0.82) × 105 km2/yr), Is it only explained by the fact that you consider now all months of the year instead summer months only?

By the way, the interannual component, that also is higher than in the previous calculation which makes sense because the MEI is supposed to be stronger in boreal winter.

The trend obtained previously (0.85 × 10^5 km^2/yr) was calculated from CRT data of “August 2002 – September 2022”, while the latest result (1.87 (± 0.82) × 10^5 km^2/yr) was obtained from CRT data of “August 2002 – July 2022”. Data of all months of the year was considered either in the previous trend decomposition or in the latest one. This change of data span was in response to your previous comment regarding our methods:

“By choosing the 20-year period "August 2002 -September 2022" period you added two extra months to the period, compared to a multiple of 12 months. This introduces a seasonal bias in the time series, especially when computing the trend. You should consider the "August 2002 -July 2022", or any number of exact yearly period, for example the "January 2003 -December 2022" period”.

OK, the data span is now corrected, as suggested. I thought you used previously only summer months but this was not the case, sorry.
So, the difference in the trend is effectively **ONLY** due to the new (correctly balanced) period (August to July), by removing the two last months.
A closer look to your initial results (with the two extra months), shows that the new trend remains moderate and unchanged from 2002 to 2014 (a 12-year period), cf figure bellow. At the opposite, the last third of the time series (2015-2022) shows a much stronger trend increase (allowed by its non-linearity). The second point is the strong increase of the 95% confidence area.



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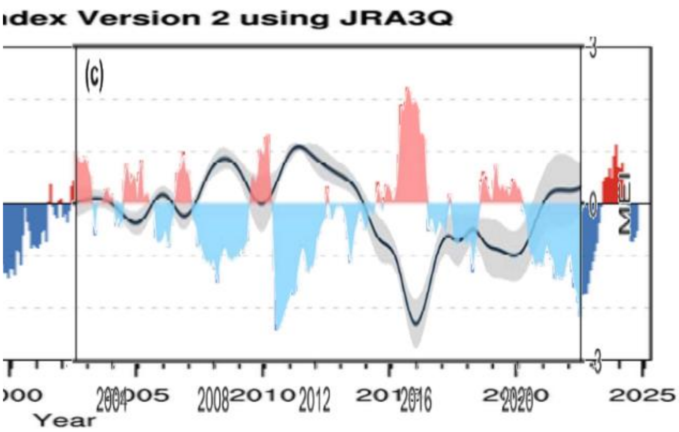

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Whatever you do, the discussion is already now quite clearer in this respect.

Also you new sentence (lines 216-217) "Certainly, further quantification of these consequences of expanding CRT would desire more dedicated efforts." is useful.

Lin 117: I would remove " considering that 1° at the equator corresponds to ~111.2 km" (quite well known)
Removed as suggested.
OK

Discussions and implications
I am sorry but "the CRT in the equatorial Pacific did not shrink but expanded from 2002 to 2022" is not exactly a question but an assertion. Please correct it, in one way or another.
Revised as suggested. See below and Lines 197-199.

"The above analysis offers a compelling answer to the proposed question regarding the changes of the CRT in the equatorial Pacific, that the CRT did not shrink but expanded from 2002 to 2022"
OK for me.

Line 208: Stallone (instead of Stollen)
Corrected. Thank you for reminding!