Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., author comment AC2
https://doi.org/10.5194/esd-2021-61-AC2, 2021

Reply on RC2

Roger N. Jones and James H. Ricketts

Author comment on "The Pacific Ocean heat engine" by Roger N. Jones and James H. Ricketts, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-61-AC2, 2021

The Pacific Ocean heat engine

Roger N Jones James H Ricketts

Response to Referee 2

Reviewer's overall comment

The manuscript "The Pacific Ocean heat engine" by Jones and Ricketts addresses the role of the Western Pacific warm pool as a heat supplier to the globe, of which heat is transported from the eastern tropical Pacific. This supply occurred as stepladder-like rather than gradual warming during the observational period. (The importance of step-like warming is already reported in 2017 by the authors.) It also argues the shifts are related to longer variability such as PDO and AMO. The reviewer thinks this is a good storyline to explain the global warming hiatus where rather stable conditions are maintained. The abstract reads very well by pointing to this interesting result and the reviewer was keen to read the manuscript. However, the ways of presentation in the text (analysis, figures, and description) are not well structured. The reviewer has to admit that it was quite difficult to read the manuscript as the text is too descriptive, figures are not adequately shown, and figures captions do not explain clearly what is shown. It requires concise writing and succinct figures with clear representation. Also, further analysis to support the changes of heat is required as suggested below.

Authors' response

Thank you for your consideration and interest. We note the reviewer was interested in the step-like nature of change and what it might mean, and requests a more succinct and streamlined manuscript. In an ideal world that would be welcome but experience has taught us that this thesis is seen by some as highly provocative. It is also counter to the consensus view of how the climate changes, so the burden of evidence to overturn that consensus is much greater than the evidence required to affirm it. In the description of a heat engine and its relationship with a broader network, we are describing an emergent mechanism resulting from complex system interactions. This will be novel to many, so carries the burden of unfamiliarity, which needs more comprehensive explanations that might otherwise be needed. The nature of emergent features also means they need to be described as mechanisms and processes before they can be described more formally.

This paper is also informed by extensive reviewer comments of an earlier discussion paper, where that reviewer suggested more analyses to cross-validate the results. This reviewer may be been anticipating an explanation of recent changes (e.g., the hiatus), whereas we are looking at the evolution of climate system over time and hope to be able to provide a general description about how climate change irrespective of the nature of forcing. We do not agree with all the comments made and hopefully our responses will clarify the position taken here.

We can clarify the relationship between temperature and heat by being more specific about the variations of both in the shallow-ocean atmosphere system.

Change to manuscript

We will take these comments on board in any revision of the paper – especially making the figure descriptions as expressive as possible.

Specific comments

Comment 1. SST changes include some aspects of heat transport in the climate system. However, it is difficult to conclude only from the SST (and air temperature) relationship between different locations to argue a rather bold statement of defining 'heat engine'. in certain areas. Although SST can be an indication of heat distribution, it is quite a noisy variable to specifically link heat move in the climate system. The authors even argue a mechanism of heat transport and dissipation, which is not analyzed at all. In addition to the SST, heat content (at least upper 700 m where observational (or reanalysis) data is available) and ocean-atmosphere heat exchange need to be analyzed. Particularly, heat content changes by ocean advection and net surface heat fluxes will be useful.

Comment 1. Authors' response

We are surprised at these basic assumptions being questioned in this way.

A conventional heat engine has two reservoirs at T_H and T_C with a heat flux moving from T_H to T_C (Q_H and Q_C) with some work W being done along the way, so that $Q_H = Q_C – W$. The ideal efficiency of that heat engine depends on temperature between the reservoirs independent of the energy flux. The heat flux depends on the temperature difference and the medium through which it is to be transferred. A heat engine in equilibrium will depend fully on the temperature difference because the amount of power and work produced remain constant.

A dissipative heat engine such as Earth's atmosphere requires work done to be converted back into heat to balance the heat emitted into space. Its ideal efficiency is estimated by Kleidon (2016) as being 12.9%, where the actual efficiency estimated is around 2% (Kleidon 2016, Lorenz 1960). A heat pump is slightly different in that its efficiency, the co-efficient of performance is based on incoming power (due to the Coriolis effect) mediated by temperature difference as a ratio of T_C.

The Pacific Ocean heat engine is an open system where the region between TEP and TWP acts as a heat exchange mechanism with heat coming from solar radiation and heat and work from the Walker Circulation and incoming trade winds, counterbalanced by upwelling cool water inflows, a strong negative downward flux into the ocean, before emitting 'waste heat' through Hadley Cells, meridional and return currents and the warm pool. It retains steady state despite undergoing shifts in both TWP and TEP in observations and models, so is a very robust structure. This whole structure sits on a thermocline which itself oscillates. We are satisfied that the idealised reverse heat engine structure is well conceptualised, with the relationships between TWP and TEP and with other regions being the variables of most interest.

SST integrates the energy available at the surface and if the heat content of the shallow ocean changes, SST will change. The variability of mean SST over a region is actually lower in most places than variability over land, with the TEP region being an exception. However due to strong negative feedbacks, its stability is very high. The bivariate test takes variability into account in its calculation of the likelihood of change in mean and $p<0.01$, which invariably means a change >1 standard deviation. SST, upper layer ocean temperature, and upper layer ocean heat content are all clearly regime-like showing they are closely related within the couple ocean-atmosphere system. With depth, changes in ocean heat content become more gradual, with the profile 0–2,000 m being very curve-like (Fig. 1). Individual ocean basins and the hemispheres tend to be more step-like, showing some regional variation.

It is simply not possible to use the variables suggested for long-run historical analyses from 1880. Data availability and model limitations affect reanalyses and climate models are limited in their ability to reproduce heat engine behaviour. We are interested in how the climate evolves from pre-industrial to the present time and in the ability of models to reproduce these changes and project them forward. The latter is investigated in the second paper of two 2021-62 (Jones and Ricketts 2021). These suggestions sound like they would inform an energy balance approach, which is not what we are doing.

Figure 1: Regime shifts for global ocean heat content 1955–2020 (upper) 0–700 m depth, (lower) 0–2,000 m depth.

Comment 1. Change to manuscript

We could add some analyses of upper ocean temperature and heat content to the SI show they are related in terms of regime-like behaviour, but they would not constitute additional results for the paper because it does not affect the conclusions.

Comment 2. All analyses are based on time series. Except for the global mean temperature, the choice of areas for calculating time series is subject to change the results, and having too many indices makes the comparison difficult as it is presented. It is also difficult to rationalize the movement of heat without seeing spatial distribution. A suggestion is showing spatial maps of temperature differences that occurred due to step-like changes. As there are many step-like changes (each case can be called ensemble member), the ensemble mean of those cases must show a stronger signal over the TWP. If any roles of PDO and AMO are important, some features reminiscent of the modes can be seen in the ensemble mean or in some cases with time delay. Spatial patterns of individual cases will also be useful, e.g. relative role of different climate modes, e.g. PDO, AMO. Similar analysis can be done also with the heat content and surface heat fluxes data as suggested above.

Comment 2. Authors' response

We don't agree that the different time series are different ensemble members. There are however, some sampling issues that we have addressed in our choice of method, region and dataset that have only been touched on in the SI. Firstly, the spatial data has many gaps, especially earlier and data quality issues that become very apparent at grid scale. This is especially the case in data sparse regions and those that have uneven sampling, such as the ocean away from shipping routes. Using a comprehensive set of zonal, hemispheric and global regions and analysing land and ocean separately and together allows us to see the different thermodynamic units and to test where shifts propagate from and to. The zonal region 60 °S to 90 °S was omitted altogether because of lack of data. Zonal regions are also important because this is a meridional energy transfer system. TEP and TEP are different as they cover small regions and their strong influence on the Granger analyses shows they have a large influence given their small area. We have produced spatial maps of the three main events as suggested (shown below).

The sampling issue means that often single grids do not show shifts because the data is not of sufficient quality, whereas small regions average over several grid squares will. Where we have several zonal averages in a hemisphere, the strongest shifts are preserved at the large scale. For example: 0–30 °N 1926, 1979, 1997, 2014; 30–60 °N 1921, 1988, 1998, 2015; 60–90 °N 1920, 1988, 2002, NH 1925, 1987, 1997, 2014. We have also tested these according to the distributive law, splitting regional warming into fast and slow components and adding these separately to obtain a larger regional average. If they are commensurate, then fast and slow warming at the regional scale should equal the totals added from the smaller regions. This does not always occur if the regional average is more trend-like and the smaller regions more steplike. Averaging steplike changes across regions that have a similar history does produce steplike change at the global scale, but the dates of change will vary regionally.

We don't understand the reasoning behind the assertion that TWP must show a stronger signal for each event (we call them events in the paper – they are shown for a larger collection of data sets in Figure 2a of the paper). The most important factor is the timing (see above). The working hypothesis is that this involves a shift being initiated due to a critical limit in attractor space being reached.

We are also unsure of the means by which shifts are propagated – this remains to be worked out. Some are due to circulation changes but teleconnections are also involved such as the PDO and AMO. Therefore, we might not necessarily expect their footprints in warming factors if they are acting as a trigger. Likewise, if the TWP is acting as a thermostat, it may be the source of instability but not the heat. The Granger analyses also reveal interhemispheric influences but due to the sheer quantity of results produced we have not dwelt on these.

Comment 2. Change to manuscript

We have produced some spatial maps with the timing. Draft figures for the three main global events are shown below (Figs 2–4). They do show some evidence of PDO and AMO involvement in the pattern of change. The years 1994–98 show a widening of the tropical zone with the major influence being on the midlatitudes. Final versions could be shown in the paper with some brief interpretation. Many of the continental areas in these diagrams will show shifts if area averaged.

Figure 2: Year before regime shift ($p<0.01$, the bivariate test Ti_0 date) during 1974–1978 on a 5 degree grid using NCDC v5 temperature data. The 1974 dates in the Indian Ocean may be influenced by a previous event in the SH. The locus of this change is the eastern Central Pacific. Note gaps in data have not been included but would be in a final version.

Figure 3: Year before regime shift ($p<0.01$, the bivariate test Ti_0 date) during 1994–1998 on a 5 degree grid using NCDC v5 temperature data. The locus of this change is the western Pacific warm pool and eastern N Atlantic. Note gaps in data have not been included but would be in a final version.

Figure 4: Year before regime shift ($p<0.01$, the bivariate test Ti_0 date) during 2012–2016 on a 5 degree grid using NCDC v5 temperature data. The locus of this change is the western Pacific warm pool and eastern N Pacific. Note gaps in data have not been included but would be in a final version. This is a similar pattern to Figure 2 and more shifts are likely to be detected over the next few years because of the short distance to the end of the record in 2020.

Comment 3. Years of the shift (step-like change) need to be consistent and marked on all relevant analyses. The years should be defined by global mean temperature because the main message of the manuscript is for global temperature changes. Many other years of shift based on different indexes (e.g. TWP and TEP) are introduced to track the sources of changes. However, as this is not compared with the years of global shifts, it is difficult to follow the arguments.

Comment 3. Authors' response

The response to comment 2 shows why we think regional dates are important, as are differences between shifts on ocean and land. We will continue to view these episodes as events and will look at making the text clearer on this. In response to Reviewer I's comments, we are revisiting the main take-home messages of the paper. The key finding is that we have identified the Pacific Ocean heat engine and associated network as the major mechanism regulating regime shifts.

The text and Figures 9 to 13 in the paper show that each event consists of shifts propagating throughout the system, in a process that might take up to two to three years. Bringing things back to single date would remove regional information, which is the information that most informs changing risk.

The major underlying motivation for carrying out this research is to provide better estimates and predictions of changing risk for impact and adaptation assessments at regional scale. Although GMST is an important measure for climate policy and the fact that global temperature can shift by up to ~0.25 °C, it is the regional shifts that can be more than twice that are much more important for understanding changing risk in a given location.

Comment 3. Change to manuscript

Aside from emphasising the above points in the text of paper more strongly, we will not make the change suggested.

Comment 4. Term 'steady-state regime' seems not correct as changes are happening also on a shorter time scale. A suggestion would be rephrasing the term or give a specific definition to be used in this paper.

Comment 4. Authors' Response

Apologies, we had meant to provide a more formal definition in the paper, and have one elsewhere but did not include it here. We will argue in the second paper (Jones and Ricketts 2021) that the climate regime is a basic physical unit of climate. Here, we will distinguish between stationary regime shifts of the type that may be associated with decadal oscillations such as the PDO, and non-stationary shifts associated with external forcing. Our findings show these are thermodynamically forced and represent a change in heat transport, whereas a stationary change may involve shifts in sensible and latent heat that compensate for each other; e.g., from wet and cool to warm and dry, but not involve any external forcing.

Comment 4. Change to manuscript

We will add more formal statements to define the steady-state regime along these lines to Section 2.1

Comment 5. Defining free and forced modes and how those years are defined need to be given.

Comment 5. Authors' Response

Thank you. Reviewer 1 also wanted more detail along these lines. The actual change chosen coincided with the first shift note in TWP in 1968 and shift in lag-1 GMST from red to white in 1969, noting that the ocean shifted in 1957 and land in 1972. We have a physical explanation for this related to the limits of the Lorenz Energy Cycle and Meridional Energy Transport that requires closure. 'For full kinetic energy generation to occur, the adiabatic rearrangement of parcels by the fluid motion must be complete to achieve the sorted reference state; that is, the rearrangement must be global' (McWilliams 2019). We will argue that the shift from red to white occurs when external forcing pushes the climate system to its maximum power limit. Note that this result is restricted to global average ocean, land-ocean and land temperatures and is not as coherent for regional averages, supporting the global case.

Comment 5. Change to manuscript

The text between lines 335–344 will be updated accordingly.

Comment 6. Most of the figure captions do not include a detailed explanation and it is virtually impossible to reproduce or objectively understand the analysis. For example, Figure 1 and related text do not explain how the periods for trends and shifts are not chosen.

Comment 6. Authors' Response

Thank you. There is quite a lot of support material in the Supplementary Information, so we will sign-post this a lot more closely and add to it when needed. For example, the how-to for this method in is the SI at the top of P6. We will also go through the SI to make sure it has been updated with any changes made.

Comment 6. Change to manuscript

We will go through each of the figure captions and expand the description. We will also go through the text and sign-post clearly where there are additional, supporting information in the SI, particularly that outlining methods, assumptions and caveats.

Comment 7. Section 2.4: It is too lengthy and better to be incorporated into the introduction by reducing the text. In line 227, please also check whether winds turn east really during El Niño. It will be eastward anomalously.

Comment 7. Authors' Response

Thank you. We prefer to leave this as is because the physical setting for tropical Pacific and where we start is important to ground readers in their understanding and experience before we begin to add new information. The tropical Pacific has been the focus of different research approaches since the 1980s (it can be argued that it was a focus much earlier, but this is when the research diverged into different streams). We accept this manuscript is not easy on the reader because of the amount of new material but fell that it is important to start on familiar ground.

Comment 7. Changes to manuscript

We will adjust the wording on line 227 as suggested.

Comment 8. Section 3.2/3.3: As pointed above, a spatial map of SST (or surface air temperature) during the individual shift cases will help to support the role of individual factors used. Figs. 9-13: It looks the choice of factors is subjectively chosen for each shift. To be objective, authors may find ways to combine Section 3.2 and 3.3 and use PDO, AMO to link the TWP and TEP. I suggest excluding AMOC as no direct observations of AMOC are available for the whole period of the analysis. The AMOC index used here is a reconstruction from SST, and which can be projected already by AMO.

Comment 8. Authors' Response

Thank you. We don't understand what the comment on role of the individual factors means. All the available 29 zonal, hemispheric and global averages for land, ocean and land-ocean plus TWP and TEP were subjected to the same analysis where the detection of shifts was carried out by the multi-step bivariate test. So we don't understand the subjective choice of factors, because we don't understand what the factors are referring to. All shift dates have been selected by the test at $p<0.01$, with the exception of a few that are $p<0.05$ where it was felt the timing helped explain the order of shift propagation. These are extensively reported in the SI, particularly Tables S8 and S9.

We will not merge 3.2. and 3.3 because 3.2 is about heat propagation via temperature and section 3.3 is about trying to understand the role of decadal oscillations via teleconnection. These are different mechanisms. The issue of the relationship between the AMOC index and the AMO is discussed in the literature and we cite some of those discussions here and discuss it further in paper 2021-62.

Our concluding hypothesis is that regime shifts are also showing evidence of switching from slow modes of dissipation (AMOC) to fast modes in the atmosphere and that coincident regime changes in the indices that represent these processes are evidence for that, with the caveats given between the index and actual measurement. The AMOC index is very different to the AMO index, and interestingly, the AMOC and Gulf Stream Indices diverge in 1972 showing the system has undergone a regime shift from co-dependent to independent behaviour.

Comment 8. Changes to manuscript

We anticipate no change to the manuscript.

Comment 9. Section 3.4: I have to admit that it is quite difficult to follow the argument. It is quite descriptive and the figures are quite complicated even without labeling of x- and y-axis in the figures.

Comment 9. Authors' Response

Thank you. We accept that this section is difficult to follow. To our knowledge this type of analysis has never been carried out before in climatology. There is no need to label every axis because they are all the same but the caption does need to be made more descriptive as commented more generally above, so the reader knows exactly what they are being shown.

The results show the f-statistic for Granger regressions that subtracts lag 1 to n-1 from lag n, therefore showing that new information informs the current value in the internal between lag n and lag n-1. The most prominent of these is for annual TEP where a strong lag-2 effect means a pronounced influence on the predictand in real time and lag-1. A sustained high f-statistic implies a long-term and complex relationship. However, the f-statistic does not indicate whether lagged correlations are positive or negative, so this needs to be established independently.

The combination of monthly and annual free and forced analyses using stationary (de-stepped) and observed (nonstationary) data provides 16 panels for 29 variables. We report the main findings from this while also trying to provide some interpretation on a novel analysis. Clearly, more effort needs to go into what is in the paper and what is in the SI. More sign-posting between the two as suggested above for other methods can be part of this.

One possibility is to list the main take-home messages without showing any analysis but this seems unacceptable. The correlation analyses presented are also critical because they show a transition from circulation to increased teleconnection when climate moves from free to forced mode. The increases in oscillating behaviour between TWP and TEP at the same time as they move from being linked to the tropics to the global climate is also important. These analyses are very detailed, so can be the subject for more papers than just this one.

Comment 9. Changes to manuscript

We will revise this section substantially and make much more of the link between the main manuscript and the SI to keep the reader as informed as possible.

Comment 10. Line 25: Accompanying paper is not cited.

Comment 10. Authors' Response

This was not cited because of the uncertainty about submitting two papers at the same time. This is easily corrected.

Comment 10. Changes to manuscript

We will either cite the discussion paper 2021-62 explicitly or its successor if that publication moves forward.

Jones, R. N. and Ricketts, J. H. (2021) Climate as a complex, self-regulating system. Earth Systems Dynamics Discussions, 2021, pp. 1-47.
Kleidon, A. (2016) Thermodynamic foundations of the Earth system, Cambridge University Press.
Lorenz, E. N. (1960) Generation of available potential energy and the intensity of the general circulation. in Pfeffer, R. L., (ed.) Dynamics of climate,Oxford: Pergamon Press. pp. 86-92.
McWilliams, J. C. (2019) A Perspective on the Legacy of Edward Lorenz. Earth and Space Science, 6(3), pp. 336-350.