

Earth Syst. Dynam. Discuss., author comment AC1  
<https://doi.org/10.5194/esd-2021-61-AC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Reply on RC1

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-AC1>, 2021

---

The Pacific Ocean heat engine

Roger N Jones, James H Ricketts

Response to Referee 1 Reviewer's overall comment

The manuscript "The Pacific ocean heat engine" by R. Jones and J. Ricketts (ESD-2021-61) follows from a previous submission by the same authors: "The Pacific Ocean heat engine: global climate's regulator" (ESD-2019-72). It is an in-depth description of the so-called "Pacific Ocean heat engine", a climatological figure acting as a heat pump, delivering heat from a cold reservoir (the tropical Eastern Pacific) towards a warm reservoir (i.e. the Western Pacific, and in particular the warm pool). The heat engine is affected by external forcing and at the same time acts on the way the climate system responds to forcing, in terms of a heat-release mechanism. This is used to explain why the historical warming is better described as a succession of steady states and sudden jumps than as a trend-like behavior. This manuscript is the first of two twin papers, the second one, according to the authors, providing a link between the heat engine and the dynamics of the large-scale circulation.

Overall, I think that the choice of dividing the analysis from the previous paper in two parts has greatly improved the readability of the manuscript. I have also appreciated the fact that a comparison of different methods has been included in order to discuss the network of local and remote impacts of sudden jumps in the features of the heat reservoirs. I still think that, even though it is conceived as a mainly descriptive analysis, aimed at grounding the interpretation of the heat engine mechanism on observational evidences, the take-home message might be conveyed in a more incisive way, clearly distinguishing main results from those that are less relevant.

Provided that the authors take this into account, and address the (few) minor comments stated below, I believe that the manuscript can be accepted for publication.

### Authors' response

Thank you for the appreciation of the additional work and your time and effort that went into the extensive review comments that informed it. Although much of this work had originally been planned for subsequent papers, it makes for a much more comprehensive description and has greatly informed our interpretation of the existing and new evidence.

We are a little unsure as to how to respond to the request for a more incisive take-home message, so apologies if the following discussion is overly lengthy.

When revised, the two discussion papers (2021-61 and 2021-62, (Jones and Ricketts 2021)) were to be submitted together. They were written as a complementary pair, but due to their length needed to be reviewed separately. If both are ultimately accepted, they will be published separately, so will need to be more self-contained than they are now. Addressing the aim and outcome of this paper more directly may help do that.

The aim of this paper (and part I of Jones and Ricketts (2019)), has been to identify the mechanism behind the storage and release of heat, forming steady state regimes punctuated by rapid shifts in temperature. This began with the identification of heat engine itself before moving onto the broader climate network. With the additional analysis carried out this provides a much more comprehensive picture.

The main aims of the following paper 2021-62 are:

- To identify how well the heat engine is represented in models and to test whether variables that do not have long observational records (e.g., satellite-based measurements) also show regime-like behaviour and
- To provide a conceptual model for how the climate behaves as a complex system, where thermodynamic forcing produces regime shifts, the climate system behaving as a self-regulating heat engine that automatically shifts gears under sustained forcing.

In the revision for 2021-62, we concentrated on complex systems science and the presence of emergent features to justify using a deductive and descriptive approach rather than the analytic-hypothetical approach associated with classical applications of physics. This is because we believe the emergent mechanisms need to be well understood and described in order to develop a sound hypothesis as to why they behave the way they do. This then provides a focus for further work that aims to understand the underlying physics leading to the observed behaviour.

In response to review comments to paper 2021-62, we propose to add an additional section on thermodynamics, describing the similarities and differences with existing hypotheses. This is also consistent with some of the specific comments below. In terms of this paper, the following points are important:

- The climate system viewed externally behaves as a conventional heat engine with incoming shortwave radiation leading to a surplus of heat at the equator and deficit at higher latitudes, with entropy being created by the emission of longwave radiation at the top of the atmosphere.
- Internally, the dissipation of heat from the equator is an independent process to the behaviour of the external heat engine. It is highly inefficient, with only 2% of the radiative energy available to the atmosphere being converted into kinetic energy. This internal system is very far from equilibrium.
- When the climate is externally-forced, the response time is very lengthy, so the climate internally and externally becomes far from equilibrium.
- The identification of nonlinear forced response in Jones and Ricketts (2017), shows that internally, there are mechanisms at work that produce a complex system response which is not a simple analogue of the external response, and which needs to be understood.

The take home message then becomes:

- These responses are due to a reverse heat engine, or heat pump in the Pacific Ocean linked to a broader network of teleconnections that act together to produce nonlinear

responses to forcing.

- The resulting regime changes affect planetary health in ways that will not be predicted through understanding the climate as a conventional heat engine and as represented in energy balance models.
- Historically, climate has been in two modes, free and forced, where the free mode allows temperature to shift up or down and follow random walks influenced by decadal variability and forced mode is much more tightly coupled and responds directly to the build-up of excess heat in the tropics.

The characteristics and behaviour of the heat engine and network itself are less relevant, although important. The next steps then follow points and 1 and 2 above.

### **Change to manuscript**

The introduction and Section 2 will be modified to take in the above points, along with Sections 3.4 and the conclusions (Section 4).

Specific comments

**Comment 1. Line 32:** talking about scaling, I think quite some work has been carried out (also with a focus on energetic matters, e.g. Faranda et al. 2018 JAS) on the scale invariance and multifractal structure of aspects of the atmospheric dynamics (see Lovejoy 2019 book, for a recent review). It might be suitable to address it explicitly here

### **Comment 1. Authors' response**

Yes. Lovejoy's book is interesting but considers atmosphere only for this relationship. His point about energy rate density was included in the revised 2021-62 discussion paper. We also left out palaeoclimate, where there are many such examples (e.g., Vostok temperature following the power law), due to space concerns.

### **Comment 1. Change to manuscript**

Brief additions to expand this point to cover the works suggested

**Comment 2. Line 250:** among the fundamental aspects of the climate system related to the concept of efficiency, it is particularly relevant, given its global-scale nature, to mention the Lorenz Energy Cycle, being also often referred to as a heat engine (cfr. Lucarini et al. 2011).

### **Comment 2. Authors' response**

Agreed. This is consistent with the overall response above. Interestingly, Lorenz mentioned the 2% efficiency in a 1960 publication. With respect to available potential energy, McWilliams (2019) writes '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'. We suspect this is one of the measures that the heat engine and network are regulating.

### **Comment 1. Change to manuscript**

Brief additions to expand this as suggested

**Comment 3. Lines 304–307:** I wonder if this period is really needed here;

### **Comment 3. Authors' response**

We think this indicates the periods don't need to be listed here because they are described later. If that is the case, we agree.

### **Comment 3. Change to manuscript**

Text deleted

**Comment 4.** Line 335: I think it is not clear to every reader why the distinction between free and forced modes derives from having described the whitening of the time series as we approach the forced regime. Maybe some additional explanation might be helpful here, linking these conclusions to the description above.

### **Comment 4. Authors' response**

Agreed

### **Comment 4. Change to manuscript**

We will briefly summarise the other factors referred to in the paper that distinguish free from forced. We will also suggest the physical reason is the physical closure of the Lorenz Energy Cycle/Meridional Energy Transfer limits as per comment 2.

**Comment 5.** Line 364: I can see no Fig. 9a here. Possibly a mismatch in referencing the figures?

### **Comment 5. Authors' response**

This is a typo

### **Comment 5. Change to manuscript**

Change to Fig. 8a.

**Comment 6.** Figures 9-10: describing the shift timing in the caption, rather than referring to Table S8, might be preferable.

### **Comment 6. Authors' response**

Agreed

### **Comment 6. Change to manuscript**

We will add the month of shifts to captions where relevant, and remove the reference to Table S8 where it is not needed.

**Comment 7.** Line 418: I do not have clear why the description of all these shifting events is needed and why not picking only those that are remarkably different from each other, thus helping to convey the main message.

### **Comment 7. Authors' response**

This is a brief summary of the events during the forced period noting global and regional events.

**Comment 7. Change to manuscript**

This can be shortened noting that the three largest events coincide with shifts in decadal variability. If we add the maps of spatial change, this will not be needed because it would be covered by those descriptions.

**Comment 8.** Lines 473-474: I think this sentence deserves a bit of rewording.

**Comment 8. Authors' response**

We agree

**Comment 8. Change to manuscript**

Will edit it to make it clearer

**Comment 9.** Lines 494-495: it might be better explained why the arguments below benefit from the comparison of these two different metrics, and why one needs to take them both into account.

**Comment 9. Authors' response**

We agree. Detrending the AMO index has made little difference until the mid-1990s when a regime clearly occurs. This shows up on the maps of spatial change (Fig. 3 in response to Reviewer 2)

**Comment 9. Change to manuscript**

We will clarify this, because it shows that detrending would have had limited effect prior to the mid-1990s shift, which was clearly a regime shift.

**Comment 10.** Line. 684: after all the description of the relationship between TEP, TWP and AMO, I am a bit surprised that the authors claim the take-home message is that we have to increase our focus on Pacific-Atlantic linkages, as this is nothing really new to the community. I believe this could be rephrased in a more convincing way.

**Comment 10. Authors' response**

We agree, although this point was referring to the direct involvement of linkages between the AMO and TWP and TEP in regime shifts, whereas the literature is dealing with climate variability, mostly on annual timescales, and only now really taking two-way linkages and possible involvement in the forcing process seriously. We discuss this in more detail in 2021-62.

**Comment 10. Change to manuscript**

Will be more specific with respect to the above point.

**Comment 11.** Lines 733-735: in the end, I found the section on Granger causality very hard to read. In particular, I think the relevant information is somehow hidden in a lot of other features that are barely mentioned and never linked to these conclusions. Authors might want to improve the readability of this section;

**Comment 11. Authors' response**

Although undertaking a Granger analysis was this reviewer's suggestion, the way we have

carried this out is novel for climate, much like a simplified sector by sector econometric approach. Including the nonstationary data for comparison has pushed the test beyond its design limits with respect to p-values, although the f-stat results are relevant. Many of the results are not intuitive and a full interpretation often involves also looking at lagged correlations. This work really deserves a paper of its own, so here we have tried to summarise the main results, although have also tried to provide information for interpreting the diagrams more generally.

It is hard to follow and we will revise the section, drawing out the main findings and concentrating on the results which support that. Some overall interpretation of the method will remain in the SI, but a full interpretation will need to be published elsewhere.

#### **Comment 11. Change to manuscript**

The section will be revised and better balanced with the information provided in the SI to make the results in the paper more traceable and understandable.

**Comment 12.** Lines 825-826: this figure of the heat engine as a thermostat, maintaining the system at a somewhat steady state, is really intriguing, and I wonder if it might be related to the relatively stable intensity of the Lorenz Energy Cycle in response to different forcing, despite significant changes in its components (cfr. Lembo et al. 2019, Ma et al. 2021).

#### **Comment 12. Authors' response**

Thank you for this comment. This is the type of consideration we were hoping for.

This is a topic for the other paper (2021-62), as mentioned in the overall response. It is related to the response to the comment for line 250. We think the heat engine (pump) is responsible for the maintenance of steady state. The Lorenz energy cycle is the atmospheric component of the meridional energy transport system. We know that regime shifts are emergent from the coupling of the ocean and atmosphere system as is the Pacific Ocean heat engine and that meridional heat transport shows a similar stability to forcing. We suspect this is related to the conservation of angular momentum in the presence of geostrophic heat transport, where meridional heat transport is limited by the low availability of kinetic energy. Available potential energy as hypothesised by Lorenz (1955) or maximum power limits by Kleidon (2016) optimise to maximum entropy for a given state, given a structure that has evolved to maximise both. The strong surface negative feedback provided by the TEP region and positive feedback by TWP during stable periods provides a like mechanism. It maintains steady state despite significant changes in forcing (and does so in models). This may be due to attractors within the heat engine (coupled east-west attractors) linked to a global attractor that maintains steady state in global dissipation that balances the conservation of energy and momentum with the opposing forces of available energy and available entropy (the statistical physics aspect). Once critical limits in this cycle are exceeded, regime shifts are initiated. These tend to be local when climate is in free mode and global when climate is in forced mode.

#### **Comment 12. Change to manuscript**

This will be mentioned briefly in this discussion and in the conclusions. The heat engine figure will also be amended to make the heat exchange mechanism across the tropical Pacific the physical link between the more conceptual cold and hot reservoirs. The point that it is an open system will also be emphasised. This is important for how it can be considered in classical thermodynamic terms.

#### **Technical corrections**

Line 255: TEP -> TWP;

Line 265: GSMT -> GMST;

Line 270: "which some" -> "which in some" (?);

Line 284: "This only" -> "This is the";

Line 540: remove "been";

Line 586: "a" -> "are";

Line 603: "influence" -> "influenced";

Line 654: "follow" -> "following";

### **Technical corrections. Change to manuscript**

All changes will be made.

Jones, R. N. and Ricketts, J. H. (2019) The Pacific Ocean heat engine: global climate's regulator. *Earth System Dynamics Discussions*, pp. 1-42.

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. (1955) Available potential energy and the maintenance of the general circulation. *Tellus*, 7(2), pp. 157-167.

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.