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Earth System Dynamics Discussions

Interactive comment on "The Pacific Ocean heat engine: global climate's regulator" *by* Roger N. Jones and James H. Ricketts

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Overall summary: Referee 2

This work builds upon a previous study of the authors that showed that decadal scale warming as monitored by Global surface temperature is not "trend-like" but behaves like a progression of fast warmings ("shifts") and periods of stagnation. The present paper tries to relate this concept to processes related to ENSO and the "Pacific heat engine" by statistical analyses of warm-pool and cold-pool temperature time series, their relation to climate indices (PDO,AMO,AMOC...) in the context of shifts. A conceptual model of the heat engine is presented in a second part of the paper. While I find the concept very interesting and I appreciate the effort of detailed analyses, I found this

paper very difficult to read. It is very long, wordy and not very focused.

The many statistical analyses and detailed descriptions are difficult to follow. How the conclusions made in the text arise from the Figures is mostly difficult to see.

Overall summary: Authors' response

Thank you for the review and the time taken to consider the paper. We agree that the paper did not settle on a particular focus and could be made much clearer.

In our view, the previous work (Jones & Ricketts, 2017) showed that the current paradigm of the climate changing gradually over decadal timescales could not be sustained. When time series of GMST from the CMIP5 RCP4.5 ensemble 2006–2095 were divided into shifts and internal trends and separately regressed against ECS (n=93), shifts explained 2.9 times the variance of results than trends. Shifts also dominated internal trends during 21st century warming (average 0.73 °C vs 0.43 °C 2006–2095, n=107). For observations, shift/total warming ratios of up to 100% across some regions also preclude warming being trend-like.

This is consistent with a storage and release system, where the additional heat being trapped by greenhouse gases in the lower atmosphere is being absorbed and stored by the shallow ocean. If the shallow ocean–atmosphere is accepted as being in steady-state during the above-mentioned 'periods of stagnation', then there is little scope for gradual warming, because there is no barrier to the uptake of atmospheric heat Upon release, atmospheric feedbacks respond immediately. An analysis of temperature in the central eastern Pacific (TEP) and western Pacific warm pool (TWP) led us to believe the relationship between the two was central to this process, so we developed the tracking model to assess where and when shifts occurred.

Consistent with the severe testing approached taken in JR2017, we used statistical induction to interpret test results while aiming to develop probative criteria that could be used to assess those interpretations against plausible alternatives. The above example of using ECS to test step-like against trend-like components of warming is an example. The main drawback is that by rejecting the current paradigm, we also reject the probative criteria currently in use. For example, criteria that justify the use of the signal-to-noise model as representing an underpinning physical process (i.e., the conversion of radiative forcing into atmospheric temperature mediated by ocean heat uptake).

Developing probative criteria for testing nonlinear deterministic change in climate timeseries is complicated. The usual interpretation is to combine linear response theory with a stochastic model that has a deterministic component as advocated by Referee 1 (see Ghil, 2015; Ghil & Lucarini, 2020). However, we have already rejected this as an alternative because it over-emphasises the role of gradual change. While gradual change is a potential response to some types of forcing (e.g., anthropogenic sulphate aerosols, ongoing land-use and land cover change), we conclude that this is not the case for greenhouse gas forcing.

Without a unifying principle, evidence may be drawn from a variety of different sources, which include statistical climatology, hydroclimatology, complex system dynamics, fluid dynamics, statistical physics and idealised model approaches (e.g., energy balance models, attractor models). We drew from these hoping the reader could follow, but the lack of focus was a significant barrier.

We have prepared these responses by concentrating on complex system dynamics, specifically fluid dynamics (although qualitatively). Although this focus makes sense for a heat engine dissipating energy in a coupled liquid and gas and gas environment, it was unclear in the discussion paper as to what the controlling boundary conditions were: top of the atmosphere energy deficit, excess surface temperature, some measure of Earth's energy imbalance involving both, an imbalance between TEP and TWP or something else.

Our aim is to develop a qualitative description at the global climate scale that best fits

the evidence and can be subject to further testing. This method may not appeal to those who work quantitatively from first principles but is intended as a starting point. Champion et al. (2019) argue that complex systems are not amenable to approaches based on first principles and that many of the quantifiable relationships that can be derived from data will be descriptive. For example, attractors describing ENSO or decadal oscillations.

A key insight came from work on developing fire climates, and was prompted by Chadwick et al. (2016) describing changes in relative humidity over land as a thermodynamic response. We tested relative humidity time series from two data sets (Lucas & Harris, 2019; Willett et al., 2014), identifying changes over Australia, both hemispheres and globally as regime shifts. Changes in latent heat supply are regime-like and the timing shows they usually follow temperature changes (but occasionally change together). Model output also shows that energy transport is dominated by latent heat transport and that changes in surface latent heat flux are regime-like.

The main governing principle for the global heat engine is the meridional transport of energy from the equator to the poles (See the response to Referee 1 for more detail). Under positive forcing this mainly involves the atmospheric component, especially changing latent heat measured as the divergence of moist static energy. Geostrophic limits provide extremely tight boundary limits to the transport of meridional energy, where modelling studies show it changes little between glacial periods and 4 x CO2 (Donohoe et al., 2020). This suggests that changes in variables such as temperature, humidity and latent heat transport are thermodynamically forced.

The Pacific Ocean heat engine itself is composed of stationary and transient components. The stationary components involved include the Hadley and Walker Cells and the transport of heat from cold reservoir to warm pool. These supply much of the vertical transport from the equator, and also are the source of meridional transport.

When the heat engine is working in forced mode, meridional transport is initiated by an excess of heat in the tropics. The evidence from regime shifts suggests that on decadal timescales, such forcing is nonlinear, resulting in shifts in mean between steady-state regimes that alter dissipation rates, in the same way that an engine may shift gears. Meridional limits in heat transport are maintained while the mode of transport, being comprised of dry and moist static energy in the atmosphere, ocean heat transport and kinetic energy can alter to accommodate the required rate of dissipation to maintain steady-state.

In free mode, forcing in the extratropics could therefore result in shifts being initiated in those regions and propagating back to the tropics, as probably occurred in 1920 and possible at other times in the first part of the 20th century.

Radiative feedbacks are part of this adjustment process, with shifts in surface temperature heating the atmosphere, resulting in cloud feedbacks and altering absorbed shortwave and emitted long-wave fluxes. The heat engine therefore acts as a network, where in the early 20th century the influence of the Pacific Ocean heat engine was largely tropical and subtropical with the extratropics being able to do their own thing. The increase in forcing expands the influence of the tropics, increasing the strength of teleconnections but also resulting in increased spatial feedback from the high latitudes towards the tropics and between the hemispheres. Slow modes of dissipation decrease in influence, while rapid modes increase, reducing physical circulation at the expense of teleconnections.

The first part of the paper, therefore traces the behaviour of the heat engine and its global network, especially of the regime changes responding to thermodynamic forcing. While concentrating on temperature, atmospheric moisture fluxes are important while harder to measure, and radiative fluxes even harder still. Temperature analyses can rely on observational networks but other variables also require remote sensed, reanalysis and climate model output, which are limited by the capacity of those sources to reproduce historical change.

Our proposal is to split the paper into two as advised. The first paper would include the statistical analyses and tracking models of observed climate. Observations are the primary focus.

The second paper will elaborate on the above description, taking the findings from the first paper and adding some additional analysis as illustrations of specific points. It will also draw widely from the literature in order to construct a conceptual model of the global heat engine. We will argue that the current model of radiative forcing producing a climate response is missing a key component, a ‘hidden climate’ where thermodynamic forcing produces nonlinear responses on decadal timescales. Because these responses are boundary-limited by a gradual increase in radiative forcing they produce complex trends. These complex trends are similar in overall rate and magnitude to the simple, monotonic trends being used to measure the long-term climate signal, but on decadal timescales the signal is nonlinear.

Overall summary: Change to the manuscript

Revised contents for the two papers are presented in the response to referee 1.

Referee’s comments

The following changes need to be made before it can go into a more rigorous review of the science:

Comment 1) The paper should be substantially shortened. I suggest to leave part 2 out and leave it for a separate paper.

Comment 1) Authors’ response

As mentioned above, we agree with separating the paper into two, having generated some data to add to the first and a large amount of material for the second paper. One paper will focus on the behaviour of heat engine processes and the other on climate as a complex dynamic system, where thermodynamic forcing is the driving process behind how that system behaves.

Comment 1) Change to the manuscript

Revised contents provided in response to Referee 1's overall comments.

Comment 2) Introductory material is mixed-in with the results. Please clearly distinguish between introduction/discussion of previous work and unique results from the present study.

Comment 2) Authors' response

We agree. We propose to have a short introduction clearly stating the purpose of the paper(s), restate the case for regime-like warming and briefly describe the evidence that will be presented. This will be followed by a context setting section that will cover the previous work describing severe testing and updated conclusions, how nonlinear change is measured within time series, the approach taken to complex systems in the paper, a brief section sign-posting methods and tools used and links to the supplementary information and the physical setting.

Comment 2) Change to the manuscript

Changes detailed in response to referee 1.

Comment 3) I suggest a clear re-structure of the paper: Describe in the introduction in a focused way the starting point, also the state-of-the art with respect in relation to climate indices etc (a lot of this now is mingled in with the results sections). It should be clearly stated at the end what hypotheses you are investigating or which questions you are addressing. After the introduction, there needs to be a methods and data section where the statistical methods are explained and data described. At present, this is all in the SI, and e.g. the "tracking model" and how it is used is unclear. The methods section needs to lay out how the hypotheses will be tested with the statistical methods. The results sections are overloaded with detailed information but the storyline is not clear. Methods description and results need to be delineated. The text needs to directly refer to the Figure panels so one can track the conclusions drawn.

Comment 3) Authors' response

Thank you for these suggestions – this is now part of the suggested restructure

Comment 3) Change to the manuscript

Changes detailed in response to referee 1, comprised of new table of contents and reorganised sections.

Comment 4) Use present tense when referring to the results. It is very difficult to track whether described results are from the present study or referring to some previous results.

Comment 4) Authors' response

We agree, and apologise for the confusion. The revised structure will make this more straightforward.

Comment 4) Change to the manuscript

The appropriate changes will be made to reported results.

Comment 5) There are many unclear and unscientific phrases throughout the paper: e.g.: ... "the heat engine is networked within the climate system" (abstract)... Examples from the intro: ...the process is "regulated" by a heat engine spanning ... ...shifts are "linked to the wider system of climate oscillations"... ...the difference between "this" and the standard model "in terms of energy flow" .. ...how it interacts with "broader climate"... section 2: ...heat "being channeled and made available for dissipation"... page 11: ..."because of issues with".... page 17: ...the "tightening of the system" coincided with the heat engine moving from free to forced mode... etc. There is a need to go through the paper again and make statements as clear and scientific as possible.

Comment 4) Authors' response

Agreed and accepted – the lack of a focus for the science and a lack of agreement in technical definitions and language when dealing with nonlinearity and complex systems made this difficult. By focussing on complex system dynamics and taking a conservative approach to thermodynamics we hope to make this much more straightforward.

The context setting planned as an extended introduction will provide a guide as to the general approach and define important terms. For some of the phenomena we will be describing, the thermodynamics as to how they distribute energy is uncertain. Our interest is in the effect in terms of measurement and how that might flow onto risk.

The additional work done to clarify points raised by both referees has helped to increase our understanding of how the climate system is distributing energy, which can flow through to clear descriptions of those processes. Some of the phrasing will remain descriptive, and therefore 'unscientific' because the system is complex, but such descriptions will be standardised and defined where needed.

Comment 4) Change to the manuscript

The text will be gone through with this in mind and language and meaning clarified.

Structural comments:

Comment 5) Figure 1a is something you find in any climate/ocean dynamics textbook. Instead of this rather basic Figure it would be better to redraft something related to the heat engine hypothesis, recharge-discharge theory with focus on your hypothesis.

Comment 5) Authors' response

We felt it useful to start with a fairly basic description because that's what people generally understand. We will add some details

Comment 5) Change to the manuscript

Some minor details added but we want to keep this fairly straightforward at the start

Comment 6) Section 1: Bullet points 1-6 read like the conclusions section from the

Jones and Rickets 2017 paper and are not needed in this way here. Line 77: what is meant by subsequently and elsewhere? Line 84: here, hidden in a side sentence, it is stated for the first time what is the subject of the paper. It is not clear though what is meant by "processes" because I find this paper to be mainly about statistical analyses and not necessarily the underlying processes.

Comment 6) Authors' response

We have discussed both referee's comments with regard to our failure to convey why these findings are so important and have proposed changes. Line 77 is just not clear and line 84 is not well sign-posted in the preceding description.

We intend to make this clearer in the context section, but the approach we took to testing the difference between gradual and abrupt change in JR2017 is error testing. Mayo (2018) defines an error probability as the probability that a method commits an erroneous interpretation of data, and error statistics the practice of exploring this. Error testing is described as both a philosophical and statistical approach to statistical induction (Mayo, 2010; Mayo & Spanos, 2011). In JR2017, we concluded that the evidence for steplike change was overwhelming. We will emphasise the key results here rather than repeating bullet points 1–6.

The intention of the paper is to use statistics to explore the underlying processes resulting in change on decadal timescales. The non-standard nature of the testing (i.e., testing for shifts rather than trends) is also because we are working with a complex dynamic system and trying to infer the mechanisms behind specific behaviours. This falls in to the area of mechanistic philosophy (Craver & Tabery, 2019), which has largely arisen due to the problems with understanding complex systems, drawing on and developing methods for understanding mechanisms that may not be tractable to the use of more traditional deductive methods.

Accordingly, we will be much clearer about processes, using more precise terminology that makes use of the concepts of emergence and organisation. Many of the tests used are detection tests designed to measure timing and effect and we will be more precise about what those are. From those we wish to infer various mechanisms associated with complex system dynamics and how they may be influencing the climate system.

Comment 6) Change to the manuscript

Some of the material in the previous paragraphs will be incorporated into the context section describing the general approach, and how the statistical methods and tools will be used to inform the analysis and results.

Comment 7) Section 2 and the beginning of section 3 are introduction material. What exactly do you mean by heat engine "behaviour" in the title section 3? Line 158 suddenly results start. Lines 169 ff: too many numbers, difficult to follow.

Comment 7) Authors' response

We agree that the mixing of introductory material and results is confusing and the introductory material would go into an expanded context section. The proposed new section 3 is to be called results. The results from Line 169 will be woven into more of a narrative that discusses the timing of the various changes.

Comment 7) Change to the manuscript See above

Comment 8) Section 3.1: first sentence: where do you show this? explain tracking model in methods, it is otherwise very difficult to follow section 3.1

Comment 8) Authors' response

This first sentence belongs later on. We have other reasons for hypothesising that shifts in warming may originate in the Pacific due to the additional accumulation of heat from forcing, but the tracking processes needs to be shown before we can argue this properly. Agreed, the tracking model should be explained in methods and the reasons for constructing it described better.

Comment 8) Changes to manuscript

The tracking model and attached hypotheses being tested will be described in methods and tools. The delineation of free and forced, which influences how the tracking model works will be described in the new proposed Section 3.1 describing TWP, TEP and their relationship with the broader climate.

Comment 9) Section 4: First sentence: where do you show this? Lots of material and references to previous studies to be moved and discussed in introduction.

Comment 9) Authors' response

This was meant to be a mini abstract for this section but clearly does not work. Changes will be made as suggested, similar to those recommended for Section 3.

Comment 9) Changes to manuscript

The methods used for testing whether decadal oscillations may be linked to shifts in temperature will be introduced in the context section describing methods. The evidence for their being linked to the climate network and hypotheses that arise from this (shifts being propagated through teleconnections) will be introduced in Section 2.3, describing climate as a dynamic complex system. Section 3.3 would present the results.

Comment 10) Section 5.3: is this needed? It seems like a proper analysis and description of climatological data is enough.

Comment 10) Authors' response

There were three main motivations for doing the model comparison: (1) Because regime shifts are an emergent phenomenon, we wanted to see whether the basic functions of the Pacific Ocean heat engine were present in climate models, and if so, to what degree. (2) We are proposing that one of the most widely-held views about how the climate changes is incorrect – if our proposal is accepted then we would like to avoid charges of the science being fundamentally wrong. If models sustain our thesis by representing the basic characteristic of a heat engine, then only the interpretation of their output is misplaced, not the basic science itself. (3) Climate models are promoted as the primary source of information about future climate risk which, as we have argued in the past, understates that risk (Jones et al., 2013). If observations capture a degree of risk not present in models, then it is important that these are captured and used to inform future risk planning. There is also a line of argument in some assessments that if a mechanism is not present in climate models under specified boundary conditions, its existence in the real world is in doubt Mann et al. (2020). To address this, it is important to identify whether or not a particular mechanism exists in a model, especially if it is being overlooked.

In response to comments from referee 1, we have investigated model performance in more detail, extending to variables where observations are relatively brief (e.g., energy fluxes). Although these are preliminary as we have only looked at several models, they show where important links can be made when comparing observations, reanalyses and earth system models. These are now to be incorporated in paper 2.

Comment 10) Changes to manuscript

A revised section on the performance of the heat engine in models is planned for the second paper, where a brief summary of energy fluxes will be added. This will also support the discussion on the heat engine and self-regulation.

Comment 11) Conclusions: here I suggest to clearly distinguish what are new hypotheses and interpretations rather than conclusions drawn from the statistical analyses in this paper. A clear summary of the statistical results needs to be presented first.

Comment 11) Authors' response

Thank you for this suggestion. The proposed revision to the research context should set this up better. The conclusions from the first paper will present the results of tracking the evolution of warming through the system, key network interactions and put the case for there being free and forced modes during the 20th century.

The second paper will address the new hypotheses and interpretations using the find-

ings of the first paper, with additional analyses from climate models backed up by more analyses from observations and reanalysis models. These hypotheses are now much clearer and focus on how thermodynamic forcing influences dissipation in the coupled ocean-atmosphere system. This system forms a self-governing network with the Pacific Ocean heat engine at its centre. Powerful boundary conditions, such as the geostrophically-limited meridional heat transport curve between the equator and the poles, the ocean subsurface and top of the atmosphere set the physical and energetic limits of the system, and changes in radiative forcing determine the amount of heat available for dissipation.

The behaviour of the whole system is based on the idea that as a complex system, the climate autonomously settles on the preferred modes and pathways for dissipating energy. These can be crudely lumped into what we consider to be 'climate variability'. When changes in forcing are very small, climate is dominated by dynamic processes. When forcing changes, thermodynamic processes become more prominent and climate will pass through a series of thermodynamically-forced steady-state regimes until it achieves a relaxed state. This model suggests that energy imbalance leading to thermodynamic forcing is an excess of energy within the meridional dissipative system, moderated by as yet poorly understood thermodynamic rules. In a positively-forced system, this will be excess of heat at the equator. Earth's energy imbalance is therefore a symptom of dissipative imbalance. Rebalancing the deficit (or excess) at the top of the atmosphere is an outcome, rather than a causal factor driving climate change.

Comment 11) Change to the manuscript

The two-paper proposal in the final response to Referee 1 should address these concerns. âĂČ References

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