Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2019-72-RC1, 2020

Earth System Dynamics
Discussions

ESDD

Interactive comment

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

Anonymous Referee #1

Received and published: 21 January 2020

The manuscript describes an approach to the study of the role the tropical Pacific ocean has for the thermodynamics of the climate system, relying on the thermal engine concept. Starting from a previous work by the authors, a rigorous statistical testing is used in order to investigate step-like changes in the observed evolution of sea-surface temperatures (SST) in the Tropical Western Pacific (TWP) and Tropical Eastern Pacific (TEP), relating them with the evolution of global mean surface temperatures (GMST) and some indices describing inter-annual and decadal natural variability (in particular the Pacific Decadal Oscillation, PDO, the Atlantic Multi-decadal Oscillation, AMO, and ENSO). It is argued that the tropical Pacific can be seen as a thermal engine transporting heat up-gradient (i.e. from a cold to a warm region), and that this mechanism is characterized by two modes: until the second half of the 20th Century a free mode is

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associated with the natural variability at the decadal timescales, while in more recent years a forced mode is established, in which the natural variability is enhanced by the anthropogenic greenhouse gas (GHG) forcing. The authors argue that these findings provide evidence that the classical view of the response of the system to the GHG forcing as a trend-like behavior with superimposed noise-like natural variability is faulty in describing the transient climate change on a decadal timescale.

Overall, I appreciated that the authors started from a climatological point of view (i.e. from observations) in addressing the challenging issue of how climate change projects on the natural modes of variability at inter-annual to decadal timescales. However, conclusions do not seem supported by sufficient evidence from results, at least the way these have been illustrated.

In the first part, authors argue that step-like changes in TEP and TWP, in part related to changes in the phase of the AMO and PDO, as well as ENSO events, shape the decadal evolution of GMST. The emergence of step-like changes is detected through the usage of established statistical methodologies. On the contrary, the propagation of the signal (cfr. Sect. 3.1) is discussed in terms of timing across different time series, and this qualitative arguments severely undermine the robustness of the results. The authors seem to claim that conclusions similar to those based on observational-based datasets can be drawn from investigation of CMIP model outputs. A visual inspection of the model results shown in the supplementary material, though, does not seem to support this conclusion.

In the second part (Sect. 6), arguments are provided to describe the Pacific ocean thermal engine, how its changes propagate to the global climate, and how its internal variability is affected by the forcing through nonlinear mechanisms. This section is deliberately qualitative, and in some parts speculative, and this is of course absolutely fine. The problem is, that the section has the form of a review of existing literature, particularly in sections 6.2, 6.4 and 6.5, with insufficient or completely absent relation to results described in the first part. These sections alone sum up to almost 40% of the manuscript, making it dispersive and difficult to read. This is not appropriate, in my opinion, for an original research article, and shall be reconsidered.

Overall, even though the results from the first part are potentially interesting and supporting the conclusions given at the end of the manuscript, I think that a more rigorous scientific approach is needed in order to make the manuscript worth publishing. My suggestion is that the authors consider splitting it into two parts. In a first paper (approximately sect. 2-5), the statistical methodology is explicitly accounted (instead of referring to a previous paper), the shifts in TEP-TWP temperatures and their relations with GMST, AMO, PDO, ENSO are explicitly addressed through rigorous methods for causal inference detection. The network approach outlined in sect. 6.5 might also be helpful in this respect. In a second paper (sect. 6), more emphasis is given on the description of the conceptual model of Pacific thermal engine. In doing so, the heat sources and sinks, the working temperatures, the Carnot efficiency and actual efficiency have to be quantitatively addressed, possibly considering the compliance to the 1st and 2nd laws of thermodynamics via energy and entropy budget.

I pointed out in the following that the authors sometimes refer to a vocabulary which is not specific and potentially confusing for a scientific audience. I suggest that the authors make an appropriate usage of terms, avoiding expressions such as “flip-flop”, “disrupt the climate system”. Related to this, the manuscript is lengthy, and in some parts difficult to read, and it might be worth considering a significant reduction.

In the following, specific comments are provided and, where applicable, suggestions on how to address them.

Specific comments

l. 33: I do not understand why the authors oppose the approach of emerging trend-like evolution of the forced climate response, to approaches where the non-linear interactions between natural variability and forced response are explicitly taken into account. As found in the IPCC 5th AR, at sect. 11.3.1.1 (I suggest that, when possible, the authors refer to the original publication, rather than the chapter of the assessment report where the information is taken from, e.g. Hawkins and Sutton 2009, in this case), “The evolution of the S/N ratio with lead time depends on whether the signal grows more rapidly than the noise, or vice versa”. This does not rule out the possibility that the noise itself is affected by the forcing; it just says that, if the forced signal grows faster than any other change due to the internal variability of the system, the latter is treated as noise. The rationale behind the S/N approach is not to be found in the radiative-convective model, rather in the fact that the near-surface layers of the atmosphere, and particularly the land surface, are heated over timescales comparable to the inter-annual and decadal timescales of the natural variability. Furthermore, one may add that the forced response should be treated as a trend-like response specifically in the case of climate models, where the internal variability (which is related, but is not coincident, in the model world, to the concept of actual natural variability of the system) is (almost) insensitive to the forcing over a wide range of timescales. In addition to that, I believe that it is nowadays commonly accepted that the regional forced response is unavoidably affected by changes in the statistics of the natural variability due to the forcing, so that the trend-like approach alone is not a viable option.

ll. 41-42: showing that a signal responding to a monotonic trend has a not strictly monotonic behavior does not necessarily imply that there is some sort of interaction between the forcing and the internal variability of the system, contrary to what seems to be suggested here.

l. 61: it is not clear to a reader who is not aware of the 2017 paper, what “explanatory power” means, especially given that a measure for that seems to be implicitly adopted here;

ll. 63-64: is this result from the 2017 paper relevant in this context? Does it relate to the results shown in this manuscript?

ll. 81-82: in my opinion it is a bit misleading to look at the Earth’s energy im-

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balance (EEI; I think this is what the authors are referring to here) as an atmospheric energy deficit. The role of the atmosphere is determining the rate of absorption/scattering/emission of solar/infrared radiation through its chemical composition. The atmosphere has a very small thermal inertia, limiting the storage of radiative heat in its interior to (less than) 1%, whereas all the remaining EEI goes into the ocean, warming the surface, melting sea-ice and glaciers (cfr. Von Schuckmann et al. 2016).

ll. 97-98: I do not see which literature reference introduces this definition and, more importantly, which evidence is behind it. Certainly, the paper by Kjellson et al. 2014 does not address this definition.

ll. 117-118: this up-gradient flow of heat might well be compensated by subsurface return flow, to my understanding. If this is the case (although I have no reference in mind for that), does it make sense to consider the upper part of the ocean as a closed system, given also its heat exchanges with the deep layers in the subsidence and uplift regions?

l. 124: I do not agree with the statement that internally the system is not in equilibrium with incoming radiation from the Sun (if I interpreted correctly what the authors mean here). Climate is not in thermodynamic equilibrium with its surroundings, rather it is in a statistically steady state, meaning that, in the absence of any forcing, the net energy input equals the net energy output over sufficiently long timescales. More appropriately, it can be said that the system is a non-equilibrium dissipative steady-state system. The atmosphere is on average in energetic balance with its surroundings, and same can be said for the oceans, even though the time needed to achieve such a balance greatly varies depending on the subsystem that is taken into consideration.

ll. 136-139: I think that the authors might discuss to what extent the efficiency in the meridional heat transport is maximised in the climate system. One can have meridional transports simply as a consequence of the differential in diabatic heating (cfr. Lucarini et al. 2011, JAS), but energy can be transported (and transformed) in many different ways, and the moisture is a critical feature, in this respect (cfr. Yang et al. 2014, Clim. Dyn.).

ll. 170-171: are these values provided with an uncertainty range? If so, is it possible to have them shown?

ll. 174-175: given that the existence of step-like changes in the TEP and TWP and the mechanisms underlying them are at the core of the results, statements like this one shall be corroborated by more quantitative arguments. At l. 202 the authors claim that they start to track changes after 1947, because of the poor quality of data prior to WWII. Then why caring of shifts happening in 1937-1942?

Figure 2d: the authors compare here anomalies in GMST, TWP and TEP with TWP-TEP difference. The visual effect is that variability in the anomaly time series are damped by the temperature gradient offset about 2 K. I suggest that the gradient and the anomalies are shown in different panels.

l. 190: I do not understand the relevance of this statement. The GMST are roughly stationary from 1880 to 1920. If we consider the 1900-1920 instead of 1880-1899 as baseline period, the warming in TEP and TWP would probably be consistent with GMST warming.

Sect. 3.1: the title and the first sentence in this section suggest that at least qualitative arguments are provided to explain a causality mechanism connecting shifts in TEP/TWP with regime changes in natural variability (namely, PDO and AMO). The section is rather a collection of insight descriptions of step-like changes, in which the propagation of the signal is argued in terms of their coincidence with regime shifts and impacts over various aspects of climate variability. I also struggled with the definition given of “tracking model”, given that the approach here shown seems to me rather an interpretative framework of the observed time series. It is interesting to infer causality links locally and remotely (the latter is partly accomplished in sect. 5). Nevertheless, there are several rigorous methods that might better serve the scope (e.g. Granger causality and derivatives) and I think that they should be addressed here. Figure 3: I found this figure very difficult to read. This is probably because it spans two pages, and also because the captions are not very informative, especially referring to panels (c)-(g). Please consider expanding the caption and/or splitting it in several figures.

ll. 231-257: the description of selected shifts is linked to several events that happened in different regions of the Earth. The connection seems basically motivated by the timing, but there is no specific argument for these links to be descriptive of large-scale processes occurring into the system, so I wonder why it is relevant to consider them.

l. 274: do the authors refer to an increase in the incident solar radiation, or are they referring to net solar radiation? This shall be specified.

Sect. 4.2: as in my comment about ll. 231.257, the authors propose here a connection between changes in TEP, TWP and TEP-TWP gradients with the AMO. They provide a survey of results available in literature about AMO phase shifts and link them to the step-like changes found in sect. 3.1, but nowhere is suggested that the two changes are correlated, nor any process responsible for it is indicated. While I understand the motivation for the arguments about PDO (in sect. 4.1), I do not see the reason for this discussion about AMO.

ll. 372-376: the statements in this paragraph also seem to suggest that the arguments about the timing in AMOC changes, PDO and TEP/TWP/TEP-TWP changes are rather speculative and a rigorous analysis is missing, relating these evolutions. Given that the AMO and AMOC are barely mentioned elsewhere in the manuscript, the authors might consider simply withdrawing these paragraphs.

l. 388: I am a bit confused about what “tightening of the heat engine” mean in this context.

ll. 389-390: this could be related to the definition of forced and free fluctuations described in Lorenz, 1979 (the authors cite it elsewhere in the text).

Sect. 5.2: given that the authors refer many times to the co-variability of GMST, TWP and TEP, I do not understand why this section is only at this stage of the manuscript, and not before (same for Figure 4b). I would suggest that it is moved before sect. 3.1, after Figure 2 is described.

ll. 433-435: I wonder why reporting the number of detected shifts is important here, especially given that the authors claim at l. 437 that many models show a "pronounced decadal variability". Typically, the multi-model analysis is ill-posed, if one does not provide a weighting scheme or qualitative arguments discerning the models. This shall be addressed when providing multi-model averages.

Sect. 5.4: the aim of this section is supporting results shown in previous sections but, in order to do so, authors provide a survey of previous analyses (Andrews et al. 2015; Andrews and Webb, 2018; Dong et al. 2019). I think that it would be more appropriate if the authors would extend this discussion describing the correlation of their data with actual measures of the atmospheric feedback, such as OLR, the cloud radiative effect (CRE). These are available either as satellite data (e.g. CERES EBAF), Reanalyses (ERA5) or climate model outputs (CMIP5, CMIP6).

l. 462: what do the authors mean by "positive spatial variations in atmospheric feedbacks"?

ll. 533-558: the authors claim here that treating the forced response and the internally-generated nonlinear variability in a separate way has no physical foundations, and they give arguments to explain that. Provided that I tried to explain in my comment to l. 33 that this sharp separation is due to a partial misinterpretation of the theory and modelling of the climate response and forcing attribution, I will try here to problematize some of the arguments. The main underlying argument they provide is that the fact that the ocean uptake of the energy imbalance determined by the anthropogenic forcing implies that the response of the system is modulated by the regime changes in oceanic variability. The authors claim at ll. 505-507 that the warming ultimately determined by the greenhouse gases is not affected by their conceptual model of the Pacific thermal engine. Rather, the model seeks to better characterize the paths through which the warming can be achieved. The S/N approach is indeed aimed at determining the overall response of the system to the anthropogenic forcing at timescales for which most of the modes of natural variability, including oceanic multi-decadal variability, is treated as noise. When looking at decadal variability or shorter timescales, the way variability is affected by the forcing is the main subject of investigation in many fields, ranging from North Atlantic weather regimes (e.g. Strommen and Palmer, 2018), ENSO regime changes (e.g. Kim et al. 2014, Cai et al., 2015, Kohyama et al. 2018), to the impact of resolution in climate models (e.g. the European project PRIMAVERA H2020). At ll. 549-550 they argue that the marginal uptake of heat by the atmosphere is soon to be uptaken by the ocean. I do not see why this should be relevant, given that the atmosphere exchanges heat mainly through latent heat and isotropic emission of LW radiation: the latter, mainly affects the exchanges of heat between the atmosphere and the continents, and is only partly emitted towards the ocean's surface. Given that, the atmospheric marginal heating is the net result of all these exchanges, thus it should not be accounted for as a heat source for the ocean.

l. 575: the authors might be more specific and explain what they mean in a scientific context when they say that the system "does not 'flip-flop"'.

ll. 578-583: surprisingly, the authors seem to ignore that significant rigorous results have been achieved on characterizing the impact of the forced variability on a chaotic system. This includes approaches based on studying the parametric smoothness of minimal QG models (Lucarini et al. 2007, Physica D), the crisis of the attractor by means of Koopman operators (Tantet et al. 2018, Nonlinearity), stochastic perturbations of edge states (Lucarini and Bodai 2017, Nonlinearity). The authors refer to Bartsev et al. 2017 to motivate the statement that the climate forcing projection over the leading modes of natural variability is inadequate to explain the nonlinear response of the system, given the inevitable presence of multistable regimes. Bartsev et al. 2017, though, use conceptual models to demonstrate that multiple stable state possibly exist in a complex system, such as the climate system. Rigorous arguments have to be provided (which is deliberately not the aim of Bartsev et al. 2017), to explain how this applies to the real system. In fact, a survey of available literature would probably convince the authors that, despite the lack of sufficient observations, precise constraints have been provided on to what extent the Hasselmann-type response dominates and when the chaotic nature of the system emerges, leading the system towards critical transitions (cfr. Ghil and Lucarini, 2019, arXiv:1910.00583 for a review).

l. 641: the authors might want to provide references on the land warming leading the oceans (if it's not just a matter of pace of the warming).

ll. 668-675: I do not understand how this mention of the maximum entropy production principle fits the remainder of the discussion in this paragraph.

l. 681-683: the authors seem to suggest that the Pacific heat engine can be treated as a Carnot cycle. Clearly, the Carnot approach only provides an ideal constraint to the efficiency of the heat engine, and this should be clarified here.

Sect. 6.5: this section contains a very long review of a few previous works using network analysis. This includes ll.715-735, where results from Tantet and Dijkstra 2014 are extensively discussed. No original analysis is provided, supporting the consistency between mentioned literature and the results here shown. Given that this is manuscript is submitted as an original research article, I would suggest that the authors either provide an application of the network analysis in this context or remove this part.

l. 769-771: again here, I think that the dichotomy "linear-nonlinear" is unreasonably emphasized, whereas the two frameworks are actually complementary. Therefore, I do not think that there is a lack of vocabulary for the nonlinear context, only because terms like "trends", "rate of change" etc. are extensively used.

l. 838: I am not really sure what the authors mean by "disrupting" the climate system.

Technical corrections

l. 390-392: the straight line the authors refer to in the text is not visible in Figure 5a.

l. 484: I believe that the authors refer here to ’CMIP5’.

l. 514: replace “additional” with “addition”

l. 544: remove “provides”.

l. 693: add “be” after “may”.

l. 831: remove “shift”.

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