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# Reply on RC1

Roger N. Jones and James H. Ricketts

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Author comment on "Climate as a complex, self-regulating system" by Roger N. Jones and James H. Ricketts, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-62-AC1, 2021

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**General comments**

This paper poses the question as to whether the climate is complicated or complex by examining the performance of tropical Pacific variability characterised as "the Pacific Ocean heat engine", and associated teleconnections. A vast array of literature from complexity and dynamical systems approaches to linear response theory, statistical dynamics etc are cited but how that literature relates to the sparse analysis provided eludes me.  Many general, qualitative and or speculative, statements e.g. "climate as a self-regulating system governed by the principle of least action" are made without support, with little or no quantitative work provided to back them up.

The authors rely in large part on reviewing existing literature to the extent that the manuscript reads like an unstructured essay. Previously presented simple multistep bivariate tests are relied upon extensively but the new results included are only cursorily discussed e.g. the multiple figures 3 on pages 17 & 20.

In short, I can't find anything rigorous that corresponds to providing an answer to the question posed regarding complexity in the climate system. Moreover, I'm not sure who would not regard the climate system as complex?

**General comments. Authors' response**

This paper was originally a single paper with two parts that was divided into two following reviewer's comments. They were to be submitted as a pair. The first was Jones and Ricketts (2021), and this was the second. They followed up on earlier work that severely tested decadal regime (step-like) change in temperatures, applying gradual (trend-like) change as a rival hypothesis. Passing a severe test for a hypothesis means that its rivals and/or null are highly unlikely to pass the same tests.

Developed by (Mayo 1996, Mayo 2005, Mayo 2018, Mayo and Spanos 2010), the inspiration was to use statistical inference to explore both sides of the Popperian dilemma, where a hypothesis could be accepted or rejected based on tests that probe the evidence, rather than either prove or disprove it probabilistically; e.g., by using a particular p-value. This overcomes methodological underdetermination, where more than one hypothesis can meet a specific test standard, often with high likelihood (Mayo 1997).

The highly probed aspect of these tests was to relate regime shifts to radiative forcing. If they were due to climate variability, they would be stochastic. Our tests showed that an ensemble of climate models (CMIP5 RCP4.5, n=107) produces a similar distribution of regime shifts to observations during the historical period. When time series of global mean surface temperature in models are separated into fast and slow modes of change, the rapid changes explain equilibrium climate sensitivity with 2.9 times the power of gradual change (n=94). For historical temperatures, internal trends between break points are insufficient to explain the minimum estimate of the forced component of warming (Jones and Ricketts 2021).

Identification of a heat engine structure in the Pacific Ocean, and the timing of shifts in the western Pacific warm pool suggested it was playing a role in regime shifts. The first paper analysed the Pacific Ocean heat engine and its interactions with the broader climate network, mainly using observed temperature. We concluded that the heat engine may be acting as a thermostat and that both the heat engine and wider network may be self-regulating. However, if that is the case, the governing rules remain unknown. The second paper aimed to address the heat engine's presence and performance in a selection of CMIP5 RCP4.5 models (the r1p1 cohort), investigate emergence of regime-like behaviour in climate models and to develop an explanation for its behaviour.

Delays and their length meant they were submitted and reviewed separately. This paper was not self-sufficient, and needs to rely more on its own content. We accept that the presentation of the model analyses, repeating those carried out on observations were very spare and the figure captions inadequate. Assessing this paper relied on being familiar with the methods and results of the first paper. These were briefly summarised on Lines 40–64 but provide only a sketch. The analysis was sparse. However, there is a rich array of evidence from previous work, which these analyses aimed to build on. A revision would ensure this evidence is better presented as a starting point.

One reason for focusing on emergence and complexity is because classical thermodynamics currently has no explanation for forced shifts between stable-state regimes, largely because they are unrecognised. The current focus is on linear response theory, which failed the severe testing outlined above. Model experiments show that these nonlinear responses to forcing are emergent (Sections 3.2.1, 3.2.3). They are absent in mixed layer models, atmosphere-only models, energy balance models, but emerge in coupled ocean-atmosphere models.

Although these responses have a thermodynamic origin, the underlying rules giving rise to them are unclear, so the paper aimed to deduce these from a variety of evidence generated by the previous work, selected tests on models and observations and the literature. A mixed methods approach is used. The paper resembles an old-fashioned natural philosophy paper rather than the hypothetico-deductive approach that now dominates the physical sciences. Several methods are used including statistical inference, experimental approaches, analogues of complex system behaviour and the philosophical application of scientific practice to inform how these methods may be applied. A revised paper would include guidance and a framework in the SI to show how and where these different methods have been applied.

By not accepting the complicated versus complex argument at the beginning, the reviewer does not endorse how the paper is framed. The paper's introduction directly addresses the rhetorical question asked by the reviewer ('I'm not sure who would not regard the climate system as complex?'). The introduction outlines the conditions for complexity (lines 30–39) while maintaining that the existing approach – supported by many who would indeed consider climate to be complex – is complicated, with complex often used as a synonym. In the first lines of the paper, Rind (1999) is cited, asking whether climate was complex and whether this was a matter for concern. Rind concluded climate was complex but could not answer whether it matters – we respond yes and yes on Line 39 and reconfirm that climate variability is complex in the conclusion.

If the constructionist approach, which follows the simple to complex model hierarchy first articulated by Schneider and Dickinson (1974), cannot be supported because of the presence of emergent features not represented in that hierarchy, the appropriate course is to identify and understand emergent mechanisms. Supporting theory can then be developed from that understanding.

Many complex systems need to be approached this way, particularly biological and socio-economic systems. For climate there is no existing theoretical framework that predicts regime shifts in response to forcing, so if such shifts are identified and proven, existing theory must be incomplete. Nor could we find in the literature any supporting theory for shifting between non-equilibrium steady states in fluid systems more generally. Especially for those systems far from equilibrium such as climate, where the underlying theory is not well understood (Altaner 2017, Singh and O'Neill 2021).

**General comments, changes to the manuscript**

We accept that the analyses presented in the paper are underdone and would improve these considerably, establishing more direct links with their related analyses in the revision of Jones and Ricketts (2021), providing more detail in figure captions and better support in the SI. We apologise for the mislabelling of figures.

The paper would retain the complicated/complex framing in the introduction but would add much greater continuity with the previous work to better delineate the goals of the paper. These are to investigate how well the heat engine is represented in the models, to explore regime-like behaviour in other variables, to address the issue of emergence of nonlinear behaviour in models, to explore the rules and limits within the climate system at the surface and top of the atmosphere and to explore the boundary-limited nature of dissipation. Thermal equivalence, consistent with that described by Kleidon (2016) will be used to inform the concept of steady-state regimes and regime change.

A revision would provide a more detailed definition of a climate regime. Two types of climate regime were identified in the paper, the first oscillating with no overall change in energy (measured as a combination of sensible and latent heat) and the second where there was a forced component. Identifying the limiting factors that precipitate regime shifts is something we are aiming to explore, even if this has not been made clear enough in their introduction and presentation.

Thermodynamics were not dealt with directly or strongly enough in the discussion paper. A revision would briefly introduce the nature of heat engines in the climate system, distinguishing between the planetary-scale heat engine and the internal, dissipative heat engine containing the heat pump and climate network. The open nature of the Pacific Ocean heat engine will be stressed to emphasise that it does not conform with the idealised Carnot heat engine. A comprehensive thermodynamic description of the climate system is provided by Kleidon (2016) who describes the major forms of energy and entropy in the earth system. They are thermally equivalent, so both energy and entropy will balance each other out if a steady state is maintained. Thermal equivalence is also required for switching between different modes of energy transport.

The relationship between geostrophic controls and work available for meridional transport will be briefly described. This will position the section describing meridional heat transfer much better. The outcome will be an improved structure for Section 4.2, especially where it fits into the existing literature and the speculative aspects that need further research. Much greater detail is given in the response to reviewer 2.

**Comment 1:** Figures are incorrectly labelled and only cursorily discussed.

**Comment 1: Author's response**

We agree and they all would be corrected, described more fully and better supported in the SI

**Comment 2:** Most of the paper refers to results in an upublished work "JR21" which has an incorrect reference – or at least one with a doi that is incorrect – but that I am assuming refers to https://esd.copernicus.org/preprints/esd-2021-61/esd-2021-61.pdf.

**Comment 2: Author's response**

Apologies for this. The submission process was difficult to follow and we did not have a link when the paper was submitted.

**Comment 3**: References to a number of papers e.g. those with O'Connor as a co-author are missing.

**Comment 3: Author's response**

Apologies for this. The omissions are due to the way that apostrophes were recorded in the reference manager. This has been corrected.

**Comments 1–3: Changes to the paper**

All these points will be addressed.

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