EGUSPHERE-2025-6345 | Research article

Submitted on 18 Dec 2025

An Information-Based World-Earth System Resilience Index

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Special issue: Earth resilience in the Anthropocene

Reply on RC1

RC set in italics and indented
AC set in roman

We kindly want to thank the referee for their review and the helpful comments and remarks to our manuscript. Below, we aim to respond to these comments and lay out how they can be applied to improve our contribution.

> *This article presents a novel, integrative, and widely applicable framework for quantifying social-ecological resilience by infusing stochasticity and agency into classic topological resilience. The dynamic nature of the IBRI allows for updated information and agency alongside physical uncertainties, making it especially appropriate for modellingiterative policy-making. The main results, that only about 30% of cases lead to existence in the safe operating space, along with the finding that the largest "leverage point" in the system is the timing of decarbonization, are sobering and represent the need for urgent decarbonization. The authors also find the counterintuitive result that gaining more specific knowledge can decrease IBRI. This is an important social dynamic and should be investigated in subsequent literature. The explicit math formulation of the IBRI is sound and well-supported, and accepts a wide range of systems, access to information, and stochasticity, adding realism to resilience metrics that implicitly assume objective characterization.*

We are happy to read that the referee finds our approach sound and well-supported and that it adds realism to resilience metrics that often claim objectivity. We believe that the summary of results by the referee highlights their importance.

> *Specific comments:*

1. *While the paper is well-situated within basin stability literature, I recommend citing a 2024 extension of Menck et al. 2013 (basin stability for updating system uncertainties, Physical Review E, 2024) that combines basin stability with bayesian inference over uncertainty. This paper is relevant because it acknowledges the conceptual gap between classic basin stability and uncertainty-informed metrics.*

We thank the referee for pointing out this important reference and will cite and include its insights in the introduction.

2. *Please discuss the drawbacks of reducing the dimension of information variables, and how this may affect application to policy. Two pixels with identical IBRI values could have very different decarbonization rates and damage strengths. Including a variance metric for each IBRI value could give more information about the full spread of parameter combinations, including the robustness of each Tg-G0 combination.*

This is a great comment and in principle is true - two different sets of parameters could produce identical IBRI values. In our setup and the heatmap, all pixels do share the same uncertainty over decarbonization rates and climate damage strengths. Two pixels with the same IBRI thus only vary in the dimensions of climate threshold and decarbonization start point, but have the same variance over decarbonization rates and climate damage strengths.

We will include a discussion of possible drawbacks of our dimension reduction and how this is related to variance in section 4. For example, an IBRI of 100% under high input variance would indicate a very robust system, as the system would reach a safe operating space under all circumstances.

3. *The author justifies the choice of SAJOS boundaries (350 ppm and $4,000 per capita per year). However, these values are rigid and contain significant uncertainty themselves. A sensitivity analysis using a range of SAJOS threshold values could add rigor to the over findings.*

This is an important observation that in fact supports the idea of the IBRI itself. Our application example is an illustration that aims to introduce the concepts, using one particular information set. The choice of SAJOS boundaries and their uncertainties are part of this information set. While a sensitivity analysis would certainly add rigour to any particular application case, it would most likely overemphasize the certainty of our results here and understate the aforementioned point. In practice one would include even more uncertainties in the analysis.

In principle, it would be possible to conduct a sensitivity analysis (e.g. Sobol, Monte Carlo), but we would refrain from doing so and rather emphasize the points made before. Since the model attractors are clearly separated, we do expect the choice of the SAJOS boundaries in state space to be less important than the choice of time by which the SAJOS should be reached. We will thus also include a discussion of the choice of timescales.

We instead suggest adjusting section 3.1 in which the SAJOS is defined. There, we will try to make the point of boundary uncertainty and subjectivity clearer by explaining the subjectivity aspect more. Since what one deems safe and just is a very normative question, it is worth highlighting this and including the subjectivity aspect in the discussion (section 4) more prominently. We thank the reviewer for pointing out this important aspect.

TECHNICAL CORRECTIONS

- *Abbreviation SES is used without definition. Please define at first occurrence.*

Thanks for catching this. We will write out and define the abbreviation at first occurrence.

- *In table 2 the notation "420 – 620, 1 ppm steps" and "1 – 10% in 1% steps" uses inconsistent formatting (comma vs. "in"). Standardize across all rows for clarity.*

We will standardize and check for formatting in the whole manuscript.

We would like to thank the referee again for their helpful remarks and insights. Furthermore, we hope that we have addressed the suggested adjustments sufficiently.