Reviewer: 2

I remain supportive of this paper and think that it will make an important contribution to the field. The authors have done a substantial amount of work since the first version and responded to all of my previous comments. However, I think that more revision is needed before publication.

R1: We thank the referee for their time and this constructive feedback. We have addressed additional comments below. We believe this second round of review aided us in clarifying important aspects, e.g., with the addition of a new Figure 1 and with clarification of caveats of our analysis. We are, overall, sincerely thankful for how the clarity of our manuscript has improved thanks to the comments we received during the review process.

Major comments:

Overall the paper struggles a bit from a clarity of presenting the results. There is a lot to track in terms of which group of papers is being discussed or how it is being analyzed, and I think that the methodology needs a diagram (hence my own confusion in the first round of reviews about term selection). There are several sets of papers being used here and it is hard to keep track of which is being used for which table or figure and for what purpose. There are 1) the complex systems science consensus papers used in part to generate Table 1; 2) the 71 review papers of ecological complexity; 3) the identified control group; and 4) the identified ecological complexity group. It is easy to confuse (1), (2), and (3) especially when the terminology is not always precise and is always fairly similar. This should be spelled out in a tiered figure showing what papers were used for which analyses, and the authors should work on disambiguating terminology.

R2: Agreed; we addressed this comment in two ways. First, we have included a new diagram in figure 1 to summarize the structure of our analysis. This figure also offers a map to navigate our paper, linking contents with figures and tables. Second, we have edited throughout the manuscript many details to clarify sections that could have been misunderstood by the reader (see below).

The authors argue that the overall goal of complex systems science is to seek laws and generalities rather than simply to state that a problem is "complex" while really meaning "complicated". Thus the authors should add a caveat related to their result that the "ecological complexity" group of papers address more features (e.g. Figure 3). After all, so-called kitchen-sink modeling papers would address more features but would count as complicated rather than complex in that those models are not seeking general theories of emergent laws. Similarly, many macro ecological patterns, such as the species-area relationship, do not necessarily address many features, in fact those papers often discuss simple models of a single relationship. The reader needs to be convinced that the number of features is a meaningful metric for the goals stated by the authors. More caveats in the discussion and introduction would be helpful.

R3: We agree. We have edited the manuscript to clarify these concepts and explain caveats of our analyses, e.g., at lines 393-395 and 639-640. In general, we agree that it is difficult to evaluate whether a paper truly attempts to engage with complexity depending on the number of “features” (in the acceptation of our manuscript) it mentions and agree that macroecological papers that engage with complexity typically do so seeking simple rules.

A key result of this paper is Figure 4. As it stands this figure is aesthetically pleasing and gets across the point about the clusters. However, I immediately want to know more about the labeling of every node. Is there a way to provide more labeling or publish the adjacency matrix as supplementary material?

R4: We have included the adjacency matrix as supplementary material (Figure S3 and S4). We have also edited the node labels to report the references of the most important nodes (i.e., manuscripts) in the figure, because we felt like the update version presenting only the reference number has lost important information in comparison to the first version of the same figure. This was agreed upon with the editorial team of Science Advances.

Minor comments:

The authors did a good job of adding a review of the history and philosophy of CSS, but these sections could now be trimmed down a bit and made more concise and clear, especially for this journal. The history section should omit the discussion of institutions since it is a bit tangential and is bound to miss important players in the field.

R5: We have worked to sharpen the manuscript and agree that the old section discussing institutions would miss important organizations, hence we have removed it. More generally, we edited the entire review, particularly introduction, discussion, and sections on history and philosophy of CSS, to make the narrative more fluent and concise.

Figure 1b should have a Logarithmic y-axis for easy comparison of the two curves at all scales.

R6: We have created a new figure that presents the axis in the logarithmic scale. This is now provided as Fig. S1. We have preferred to retain the original figure 2 because we felt that it communicates more easily the exponential growth of articles on “complexity” in the last decades, but we are open to replace 2B with fig. S1.

On page 4: "...from religion, philosophy, and ethics" should be changed to "comparative religion" since I don't think the authors want to presume that CSS is the basis of a new religion as the sentence could indicate.

R7: We have made the requested change.

On page 8: I do not agree that "criticality" rarely occurs in CSS papers. Self-organized criticality was a core topic in early CSS research, and it goes beyond "self-organization" which the authors state later. In the grand scheme of things I think that this is fine, but I do have slight ongoing concerns about how terms were selected and what concepts/papers might be missed.

R8: We have clarified that criticality “rarely occurs in the complexity papers *we retrieved*” (line 341), i.e., it rarely occurs in papers mentioning “ecological complexity” in title and keywords. We also refer here to the new Fig. 1 to aid the reader in understanding us. In other words, we agree that criticality – like other concepts that we cited in the discussion, such as panarchy, brittleness, heterarchy – is an important aspect of CSS, but ecologists seem to have neglected these concepts. We introduced this aspect of our method in the introduction, stating that “*[Our] empirical approach allows us to face the historical challenge of understanding complexity in a novel way: instead of defining the study of complexity from first principle reasoning, we quantitatively assess the literature to understand how complexity has been conceptualized by ecologists*” (lines 111-114).

To clarify the section, we now also refer to the new figure 1 and to the complexity papers found by our search. We also specify that “*criticality [relates] to dynamicity, fractality, scaling, and attractors (Table 1)*”, not just to self-organization; this is a simplification of a more nuanced and difficult concept, but to synthesize a field as difficult as “ecological complexity” we had to make some difficult choices. See line 338-352 for more details on caveats of our analysis.

More generally, we understand concerns on the “features” we propose. We had extensive conversations also within our group and with other scientists, and indeed we could not to converge to a set of features on which all of use agreed (see, e.g., the inclusion of “chaos” as a feature in the last version of the manuscript). We know we are attempting a very difficult synthesis exercise, and we hope that our work will be an important stepping stone to raise people awareness of the importance of CSS in ecology and conservation. We think that the general message that wording around “ecological complexity” is often too loose is well supported by our analyses, and hope that our manuscript will be a step towards a unified view of complex systems that is better integrated also in the environmental sciences. We have substantially edited the manuscript to further explain this point and to clarify many sentences (e.g., lines 353-355). We hope that such edits make even more clear that we do not see our work as a definitive, “law-like” list, although we still believe that for most ecologists reading our work will clarify many important concepts.

Pink and red should not be used as distinguishing colors in Figure 4 as they are too similar.

R9: We believe that the reviewer might be referring to Figure 5, which has pink and red as colors describing different clusters. Therefore, we have changed the previous red color to gold, so there should be no confusion with the color palette in our revised version. In figure 4, there is a continuous gradient from gray to red representing the number of feature occurrences across papers. Difficulties in distinguishing values along such gradient (if any) are perhaps due to the continuous nature of the data, rather than to the color palette, but we slightly edited the figure to also clarify this plot