Reviewer: 1

The manuscript performs a bibliometric analysis of the scientific literature in “ecological complexity” to evaluate the conceptual and methodological structure of this field and examine the current uses of concepts of complexity in the ecology and evolution literature.

The analysis is careful and quite complete, though it relies on a number of keywords and selection criteria spelled out by the authors. The main 22 themes (as formal features) that the authors identify from the literature (Table 1) and associated methods and metrics (Table 2) help make sense of the field, as does the organization of influential papers into clusters corresponding to basic quantitative theory, scaling and macroecology.

The main issue I have with the paper deals with a number of normative statements and conclusions, including some of their “five prescriptive principles”. I recommend that the paper could be published in Science Advances after the authors consider the following questions/comments:

- It would be good if the authors said more specifically why they feel the “field is disorganized” (l 90) and also more specifically what makes them conclude that “ ecology and conservation are lagging behind recent developments in complexity science”. Are these lags methodological? Or conceptual?

- The discussion between the similarities between “complexity” and “control” leaves the reader wondering: Control is always a feature of complex systems (as in e.g. homeostasis) but as a necessary rather than a sufficient condition. For example, the concepts/methods of control are mainly used in engineered systems, which are not typically considered complex systems. The authors show that there is a strong overlap between the two literatures, but it would be important to know if there are also distinguishing features that actually separate the two. References to evolution, open-ended adaptation or emergence may be possible distinguishing features: I’d recommend that the authors consider this analysis. In particular, the statement that “The term complexity seems therefore to have been often used loosely, confirming the intuition of Proctor and Larson (2005) that it is often “a placeholder for the unknown” “ seems possibly a bit sloppy, may or may not apply in this context, if there are distinguishing features that separate the two literatures.

- The “five prescriptive principles” read a bit casual, I’d recommend that the authors revise them to better reflect what they see as specific gaps and opportunities, while recognizing that any fast-developing field of enquiry is still grasping for conceptual bridges and improving methods and metrics, not yet being “normal science”. Specifically:

o Please say more specifically what you mean by “These approaches have already provided fresh perspectives on traditional dilemmas including the stability-diversity relationship, critical thresholds in habitat loss and fragmentation, the evolution of maladaptive characters, and more”.

o “it will be important to carve a specific niche within ecology and conservation for studies of complexity” – Is that really what is to be expected of complexity science, a niche within ecology and evolution? Or, perhaps instead, given its breadth of applications, to create a new common platform for the understanding and joint study of ecological systems interfacing with other complex systems, such as earth sciences and human societies? The latter seems to be more in line with the general motivation stated by the authors of dealing with climate change and sustainability.

Reviewer: 2

The authors embark on a very important and admirable effort, that of defining ecological complexity in order for the field to make more coherent progress. This paper has a lot of potential for impacting the field. However, I think the paper needs major revisions owing to both the methodology and broader conceptual context, and at present cannot be published.

Below I have listed a few major concerns.

Major Comments:

I am worried that the bibliometrics tools employed are not sophisticated enough to avoid false negatives for complexity papers or address a variety of conceptual and statistical issues which I detailed throughout this review. For example, what percentage of papers cited in this paper's own reference list would be identified as complexity papers? In the Marquet et al. review, which is citing mostly complexity papers, what fraction of those have "complexity" in the keywords? Does the Marquet et al. paper itself use ecological complexity in the keywords? Anecdotally, much of my work is on ecological complexity and yet I have never used those keywords.

Similarly, I am worried that many of the keywords in this field are missing (e.g. maximum entropy, collective intelligence, energy, top-down causation, robustness, food webs, niche construction, etc.) so the methods should expand in a way that these terms all naturally emerge. The goal here is to be comprehensive and so it warrants expanding and digging deeper so that the most utility emerges from the paper. For example, could a topic-modeling approach be added to analysis? This would alleviate both of my concerns that many papers are not being included in the complexity category and would also expand the identified terms. In addition, the paper should interact more with the broader science of science literature and methodology (e.g. the work of James Evans).

Furthermore, the emergence of several famous clusters of theories in ecology is impressive. But other clusters are clearly missing which makes me worry about the bibliometrics tools being employed.

Detailed comments:

Line 37: "However, myriad definitions of complexity hamper conceptual advancements and synthesis." Is it that there are too many definitions or not enough theoretical synthesis, as argued by Marquet et al.?

Lines 68-80: A counterargument here would be that the lack of single definition within or across disciplines has not always hampered progress (e.g. computer science has been

enormously successful with this concept). The authors should add this caveat. In fact, it might be even more powerful to highlight that a single definition or metric in computer science allowed for progress thus setting up the call for ecology to do the same in the following paragraphs.

Lines 201-203: I am worried about the claim that a core set of concepts has been converged on. How can this be true when many concepts are not listed in the table, as mentioned above, where whole theories (e.g. METE or MTE) might even be excluded, and how can this be true if many papers which work on ecological complexity do not list the term as a keyword or use it anywhere in the paper?

Line 251: I don't think that the "co-occurence of multiple phenomena" is one to one with ecological complexity. Think about fractals generated from the simplest generative processes, or bird flocks with only a few rules. Emergence may be an orthogonal dimension of complexity to that of simply include many phenomena in a single context.

Lines 287-296: The concepts and definitions of complexity that cut across fields have been debated for a long time in complex systems science and it would be nice to see more of those references here.

Line 333: "As Richard Feynman (92) eloquently proposed, the difficult words we use to refer to natural phenomena rarely inform us about nature itself." But his reasoning is often that we should mathematize things because language can lie in ways that mathematics cannot. His point is often that mathematics is not just language but "language plus reasoning". I think this is a bit of misquote in terms of his broader philosophy, Feynman would not be in favor of agree on new terminology, but in mathematizing and using the simplest possible language. I am not sure that he would support this sort of effort. there are other philosophers that would be better to point to here for the concept.

Lines 364-397: I am very concerned about the identification of the number of features. How can you tell that papers aren't simply mentioning the terms as analogies or adjacent work or disputing other work? How can you tell how many features the model in those papers actually addresses? How many other concepts are in the non-complexity papers? This last question could totally explain what the non-complexity papers don't have a tail extending to the right (also, I am less convince that the two paper types are really that different). Also, at a philosophical level, are "kitchen sink" models the best example of complexity? They would address the most features of phenomena, but most people in complexity science would say that those papers are simply complicated but not complex.