**RESPONSE TO REVIEWERS [abq4207]**

**Towards a cohesive understanding of ecological complexity**

Dear Drs. Riva and Graco-Roza,

Thank you for sending us your manuscript, referenced above. We are pleased to inform you that we are potentially interested in publishing the paper in Science Advances. However, we are not prepared to accept it in its present form and would like you to revise your manuscript in response to reviewer and editorial comments. Your revised manuscript may be sent out for re-review or it may be re-assessed only by the Science Advances editor/s who previously handled it. Please note that if changes do not meet editorial expectations, your manuscript can still be rejected. We have provided a detailed checklist below as well as in the submission portal to help you prepare your revised submission. Please follow the checklist guidelines carefully, including those for the cover letter, manuscript, figures, and supplementary material, and for a 125-character teaser that would appear under the title if your manuscript were accepted.

**R0:** Thank you for handling our submission. We appreciate the constructive comments on our original manuscript and worked to clarify our rationale, to expand the content breadth, and to strengthen the analyses.

As agreed in a recent email exchange, we have reformatted the manuscript to incorporate extensive changes and are submitting it as a “Review”. This allowed us to provide more conceptual insights and new sections to illustrate key information and concepts to contextualize our work (i.e., history of complex system science, different philosophical views on complexity). Reading several referee-suggested citations made us realize that there was additional content worth discussing in our manuscript, but also that including this work would have meant exceeding the references limit for “Research articles” (*n* of current references = 151). We are thankful for the opportunity to update the manuscript type to the “Review” format. More specifically regarding the changes we implemented, we have:

i) Read the suggested literature and incorporated several new citations and ideas;

ii) Re-run the analysis including “chaos” as one of the features used to characterize the literature on ecological complexity. Chaos was reconsidered and included as a feature typical of complex systems after reading additional literature suggested by the referees;

iii) Included two new sections (~ 1500 words) where (i) we briefly outline the history of complex system science studies and discuss complexity beyond ecology (e.g., from physics and medicine to computer science and economy); (ii) discuss the philosophy underpinning complex system science, including differences between restricted and generalized complexity.

iv) Reworked the whole manuscript, but especially Introduction and concluding remarks, to explain the importance of accounting for complexity in ecology;

v) Included an additional topic modeling analysis using the Latent Dirichlet allocation (LDA) approach to validate our method, following a key suggestion by referee 2. We also added a new figure concerning this analysis (Figure 2);

vi) Addressed all other suggestions by the referees.

We note that a shift from “Research article” to “Review” also better emphasizes the work we did in synthesizing concepts from Complex System Science (CSS) as related to ecological complexity. Framing our manuscript as a Review, the research weaving analyses (text mining + bibliometric analyses) we propose in the original “Research article” become a support for our claim that ecologists could refer to complexity with more attention, and that CSS can aid in this direction, rather than the ultimate point of our manuscript.

Please find full information about the nature of our revisions in the point-by-point responses below. To ease the assessment of this revision, main changes to the main text are also highlighted in BLUE. Line numbers refer to the new version of the manuscript.

All in all, we are grateful that our paper was reviewed by experts in the field and in a very constructive manner, and we think that the revision process substantially improved our initial draft. We are looking forward to your response.

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**Editor Comments**

Editor One Comments: In general, I agree with the assessment of the two reviewers and think that the paper has potential, but it requires several changes and improvements, which I hope are feasible. I would like to emphasize the following aspects in addition to the ones already mentioned by reviewers:

**R1:** Thank you. We are very appreciative of the several constructive comments received in this round of review and the precise editorial guidance.

Lines 36-37. The authors say “Understanding phenomena typical of complex systems is key for progress in ecology and conservation…” I think the authors need to clarify why this is so, and what is their standpoint regarding the issue. For example, the authors could make explicit before the quoted sentence that ecological systems are a quintessential example of complex systems yet rarely tackled as such. And that part of this paradoxical situation may be related to the heterogeneous use of what a complex ecological system is. But also, a completely different narrative could fit the bill, for example, ecological systems can be studied and analyzed in many different ways and here we focus on the complexity approach because it brings useful science to the table that could be used to understand some aspects of ecological systems, specifically how they respond to perturbations.

**R2:** Thank you for these comments. We agree, and believe that both of these aspects are relevant to our manuscript and part of a broader lack of awareness of the “complex system science” (hereafter, “CSS”) perspective in the ecological community. We have now clarified this throughout the manuscript, particularly in the introduction (lines 63-85) and in the discussion (e.g., lines 334-343). Our objective was to raise awareness that a complexity science paradigm can be more appropriate than imprecise uses of the word “complexity”, as it is often invoked in ecology. We believe that using the word “complexity” more consistently with principles from CSS can reduce confusion and facilitate synthesis in ecology, and this is now specified in the abstract (lines 38-40), introduction (lines 80-82), and discussion (e.g., lines 353-354).

Lines 50-52. The authors said “Understanding complexity is becoming increasingly important in the face of accelerating global environmental change because natural systems exposed to multiple stressors often display phenomena typical of complex systems” This first sentence seems to suggest that the authors do incline themselves into the second example of narrative provided above. Is that so? Please clarify why we should at all care about the use of complexity science in ecology.

**R3:** Yes, this was certainly part of our rationale. See also **R2**. We have clarified why we should at all care about the use of complexity science in ecology in the introduction (lines 63-85), as well as more broadly in discussing our findings (lines 334-343) and in our conclusions (lines 494-509).

Lines 81-92. This paragraph makes the point that it is the complexity concept itself the one we do not understand, or we use in many different ways, and cites mostly ecological literature missing important books and papers (e.g. Murray-Gellmann 1994, Mitchell 2009). It is true that complexity means different things to a computer scientist that to a physicists or ecologists, in part this is because they work with different systems; algorithms and Turing machines versus physical or biological systems. But in general, there is a consensus in terms of what a complex system is and what people working in the field think of them (Mitchell 2009, p.4) “…an interdisciplinary field of research that seeks to explain how large numbers of relatively simple entities organize themselves, without the benefit of any central controller, into a collective whole that creates patterns, uses information, and, in some cases, evolves and learns. A different thing is how we go about and measure these attributes. A topic on which there is much disagreement. But this should not be taken to imply that we do not have a more or less agreed-upon definition of what a complex system is.

**R4:** Thank you for making this point. We agree, and have substantially revised the manuscript accordingly. The first version of this manuscript may have been unclear on two points: (i) differences between complexity vs. complex systems as defined in CSS; (ii) differences in terminology vs. ideas.

For (i), we believe the definition of complexity has been somewhat a philosophical matter and often follows definitions or highlighted aspects of complex systems as laid out by a few key scientists and papers (the purpose of the search for a network of relevant terms). We now include more detail in the manuscript in a new dedicated section (“the philosophy of complex system science”, lines 164-230), where we briefly discuss this point and other philosophical themes including differences between restricted and generalized complexity. We also introduce the difference in the second and third paragraph of the introduction. This, in our opinion, does not contradict the point that scientists have come up with a more pragmatic, general perspective on complex systems, which was the motivation for our analyses.

For (ii), terminology can vary even between experts even though we absolutely agree that the properties of complex systems are generally agreed upon. For instance, both Gell-Mann and Holland use the wording “complex adaptive system” (CAS), but do so referring to different phenomena (Gell-Mann calls CAS what Holland calls “Adaptive agents”, such that Holland’s CAS are aggregations of CASs *sensu* Gell-Mann). In this context of their preferences in terminology, Gell-Mann (1994) even quoted an unnamed colleague who stated that “a scientist would rather use someone else's toothbrush than another scientist's terminology”. We have now specified this at lines 373-375, explaining why we used a critical review instead of automatic text analysis approaches to synthesize the field of CSS into the proposed Table 1.

Ultimately, while there is overall consensus on the characteristics of a complex system, there is not consistency on how “complexity” is used in ecology and conservation, and we hope that this is now clear in our manuscript. This is one of the key reasons that stimulated us in working on this project. Experts know the characteristics of a complex system, but most ecologists seem to use the term “complexity” in a loose way. This was clear in our preliminary assessment of all reviews mentioning “complexity”, of which none was centered on CSS (see, e.g., lines 86-100). Therefore, we propose introducing CSS to anchor studies referring to ecological complexity (see also **R2**).

Lines 93-100. “Furthermore, even within complexity science, different definitions of complexity exist due to subjective preferences, philosophical views, and peculiarities of different subfields (15, 17, 18).” First, I do not think these references really back up the statement about complexity science in general. I would suggest revisiting this paragraph after reading Gell-Mann 1994, Mitchell 2009, and for the philosophical part the work of Edgard Morin (who makes a distinction between restricted and generalized complexity, see Morin 2008), there are no real complexologists in the cited crowd (e.g., Krakauer, Kauffman, West, Gell-Mann. Holland, Mitchell) to name a few. Further the claim “ but ecology and conservation have lagged behind recent developments in this field (9, 22).” It is true to a certain extent only. If you look at the pioneering work on nonlinear dynamics and chaos, critical transitions, biocomplexity, scaling, resilience, and Panarchy theory, all of those are contributions early made by ecologists such as May, Holling, West, Brown, Carpenter, Folke, Scheffer ,etc. It is true that not all subdisciplines of ecology embrace the Complexity framework, but that is not a symptom of a bad thing, it happens in physics, mathematics, biology in general, and computer science.

**R5:** We agree that the quoted sentence was misleading (see also **R4**).We re-wrote this paragraph after reading the suggested literature and have incorporated work from these authors throughout the text (e.g., in the methods, at lines 548-550). More specifically on the points raised in this comment:

First, as stated in **R4**, we do believe that there are differences among authors in how complexity is defined (e.g., Allen et al. (2018) outlines Rosen’s and Tainter’s views, and Morin (2008) discusses restricted and generalized complexity) and we also do believe that some differences exist within CSS (e.g., Gell-Mann and Holland use the term “complex adaptive system” with different meanings; Gell-Mann 1994; see lines 255-257). However, we also agree that most authors thinking about CSS conceptualize complex systems similarly, and edited the text to better reflect this view.

Second, we appreciate your insight that the relationship between ecology and CSS is unique and dates back to the dawn of CSS. We incorporated this point in the discussion. We also highlighted the contributions you referred to, e.g., at lines 425-435 (but more broadly across the whole Review). We largely retained our original classification of 23 features (except for including “Chaos”), and made an effort to clarify why we made this choice and its limitations (lines 355- 380). For instance, some of the concepts you proposed, including “panarchy”, were mentioned very rarely in the corpus of papers assessed (see lines 355-368). This does not mean that they are not relevant, but given the target of this article – ecologists not specialized in CSS, but interested in learning about this field – we still believe it is fair to focus on the 23 more general features that we propose.

Lines 102-104. How can you be sure that you reviewed the Complexity science literature? And not just any literature that mentions complexity?

**R6:** We initially used some pivotal papers, books, and special issues to define the general themes of CSS, and then built from these papers and from papers cited in their bibliographies. We focused on literature in ecology, but not exclusively (e.g., Ma’ayan 2017, Allen et al. 2018). Some of the papers we used explicitly refer to “complex system science” in the title (e.g., Anand et al. 2010 and Filotas et al. 2014), other to “complex systems” (e.g., Ladyman et al. 2013 or Ma’ayan 2017) or “complex adaptive systems” (Levin 1998), other to “complexity” (Allen et al. 2018, Lohele 2004, Green et al. 2020). We do not believe that mentioning complexity in the title is always a signature of papers related to CSS (indeed, we make this point in explaining why we decided to work on this paper - we found no review addressing complexity following the CSS paradigm, see lines 86-100, and therefore are attempting to fill an important gap with this work). Instead, these papers were selected after reading them because they were in tune with themes commonly discussed in the context of CSS.

In our revised version, we integrated work suggested by the reviewers that also define more or less explicitly the characteristics of complex systems as defined in CSS. These include, e.g., Brown (1995), Holland (1992), Mitchell (2009), Marquet (2000), Krakauer (2011), and many others. Please see the new reference list counting 151 documents. After reading several new documents, we still feel that the features we proposed in our initial analysis cover fairly well the description of complex systems provided also by these scientists. We recognize that one could disagree on the literature and words that we chose to identify and represent different features, which is ultimately an arbitrary choice and is now acknowledged in the discussion at lines 355-380. We now discuss more explicitly this aspect of our work, but also added to table 1, at lines 1066-1071, a new column including a series of terms which will clarify to the reader what the features are thought of.

Ultimately, while it is difficult to define the boundaries of “complexity science literature”, after reading the manuscripts suggested by the referees we believe that our original sample was related to such papers.

Line 143? In the Diversity of complexity articles, how sure are you that these features (Table 1) are typical of complex systems? In the methods section, the citation to books and articles used to obtain them are mostly ecological (but for Wolframs and Holland) and misses most of the physics associated with features such as criticality, scaling, and pattern formation. I do not think that they are representative of complex system science. Further, it is not correct to use features derived from ecological articles on complexity and then compare how well-used are they in articles of ecological complexity to find that there is a large use of these features and more than in the control group. This is tautological.

**R7:** In our initial draft we referred to papers written by ecologists and conservation biologists, and this was a choice to provide the readers targets of our paper (i.e., scientists interested in “ecological complexity”) with material relevant for additional readings in their fields. However, these papers typically cited scientists at the forefront of CSS (e.g., Ma’ayan (2017) cites Mitchell (2009), Holland (1992), and Kauffman (1993)). In other words, the papers to which we referred were selected because they were already syntheses of CSS pertaining to subfields in ecology and conservation. In our new version of the manuscript, we read and incorporated the documents recommended by the referees. Note that based on the additional documents we read and cited, we introduced chaos in the features we selected, which is an important phenomenon to be considered when dealing with complex systems.

Regarding potential circularity in the analysis, we note that the papers we use to identify the features of complex systems are not the same papers used in our text mining analysis. The first category of papers was identified with a critical review (i.e., we read many papers, including the one you suggested, and made an effort to synthesize this body of literature into the 23 features we selected and reported in Table 1), whereas the second category of papers was searched in the web of science and automatically assessed in relation to the features identified with our critical review. The articles on CSS that we initially selected were framed around the usefulness of CSS in ecology and conservation, but more importantly those papers were explicitly centered about concepts developed in CSS. In other words, in compiling Table 1 we did not synthesize “ecological articles on complexity”, but rather “articles making the point that CSS has important applications in ecology”. Note that in our revised manuscript we now included an additional analysis based on topic modelling (lines 272-277[[1]](#footnote-0) and a new Figure 2), to verify that the features we selected are meaningful to characterize the 180 documents mentioning “ecological complexity” in their title. This analysis confirmed that the selection of features was relevant, i.e., the features we selected with our critical review were in 22 out of 23 cases are more important in characterizing the topics from *complexity* articles than *control* articles.

More specifically on how we broadly addressed this comment, we now (i) added more documents to the list we provided as a reference in the methods, (ii) revised the analysis including chaos, because it was clear from our additional readings that it is historically a very important concept in CSS (see, e.g., the new section “A Brief History Of Complex System Science” at lines 115-163, and reference to the work of Henri Poincaré), (iii) included a new column to table 1 (“Related concepts”), where we provide synonyms and words related to our features to make more explicit that the features we propose are general concepts that have been defined differently in the literature; and (iv) explain in the discussion why some key concepts (e.g., criticality and pattern formation) are missing from our synthesis of features (Table 1). Regarding this fourth point, we think that some of these concepts are more specific and describe phenomena that occur when many of the features we propose co-occur. We now specify this aspect in the discussions, clarifying our goal of providing a general guide to ecologists interested in orienting their work on complexity along the lines of CSS at lines 355-380; see also R5.

A better source of complexity terms could be the Complexity series of the Santa Fe Institute and other similar publications.

**R8:** We apologize, but we were unsure about the exact literature to reference here. We were able to find some of the books in the series by Allon Percus et al., but they are numerous and it is unclear which of the books would be of best use to this project. That being said, in incorporating additional literature we cited several authors connected with the Santa Fe Institute (e.g., Krakauer, Flack, Mitchell, Evans, Gell-Mann, Holland, Solé, among others). If some key insight from this Complexity series is missing, we would greatly appreciate guidance on how to find specific documents/literature.

Please notice that the network of co-citations section has a different citation format.

**R9:** Thank you for pointing this out. We have addressed this oversight.

In addition to the literature cited above the authors should consider the following papers:

Jim Brown’s Essay An Ecological Perspective on the Challenge of Complexity (https://www.nceas.ucsb.edu/ecoessays/brown) This essay and replies (especially Kareiva’s) make a great example of the tension between theory and the search for general laws versus solving the present ecological problems right now, with the tools we have.

Brown, JH. 1995. Organisms and Species as Complex Adaptive Systems: Linking the Biology of Populations with the Physics of Ecosystems. In Jones , C and JH Lawton (eds) Linking species and ecosystems. Chapman and Hall.

Marquet, P. A. “Scaling and Power-Laws in Ecological Systems.” Journal of Experimental Biology 208, no. 9 (May 1, 2005): 1749–69. https://doi.org/10.1242/jeb.01588.

Levin, S:A (1999). Fragile dominion. Complexity and the commons

Murray Gell/Mann The quark and the jaguar

Morin, E. (2008). On complexity. Hampton Press (NJ).

Melany Mitchell, 2009. Complexity a guided tour.

**R10:** Thank you for these suggestions. In reading the revised version, you will see that we have read these documents and incorporated insights through the text.

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Comments from reviewers

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:

**R11:** We thank the reviewer for spending time in assessing our work, their positive attitude towards it, and their constructive suggestions.

- 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?

**R12:** We revised the manuscript to better explain our rationale in writing this paper, and in particular have reorganized the concepts in the paper around Complex Systems Science (CSS) (see new sections “A Brief History Of Complex System Science”, “The Philosophy Of Complex System Science”, and paragraph 3 in the introduction for an explanation). In response to your comment, we have clarified that: (i) disorganization in the field is suggested by the fact that the 71 reviews in ecology and conservation referring to complexity in their title address very diverse, disconnected themes, and none actually addressed CSS; and (ii) ecology and conservation are lagging behind especially in the conceptual aspect. Confusion in ecology and conservation was certainly highlighted by the fact that none of the reviews identified in our preliminary assessment of the literature that referred to complexity focused on CSS (lines 86-100 and specifically lines 94-97, Table S1, and in the discussion, at lines 328-332), but also that only 23 papers captured by the Web of Science sample of literature on “ecological complexity” referred to CSS (lines 329-332). Concurrently, we now specify that the view typical of CSS is a perspective on ecosystems that is still rarely used in ecology and conservation, despite the availability of approaches to seeking it. See, e.g., lines 461-471 but more broadly we made an effort to clarify this point throughout the review.

- 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.

**R13:** Thank you for pointing out this term. We have now clarified what we meant by ”control” (e.g., at line 242, but see also lines 235-237 in the introduction and lines 582-589 in the Methods). We are not referring to control as a feature of a complex system, but rather a set of articles composed of randomly selected peer-reviewed articles published in the domain of ecology (i.e., as in “experimental group” and “control group” when conducting experiments) for our study. Our wording choice in the previous version may have led the reviewer to misunderstand the definition of the term “Control” in the original manuscript. Note we also included “control” as a related term to “homeostasis” in Table 1.

Regarding the citation of Proctor and Larson (2005), we clarified in lines 66-78 in the introduction, and particularly in the new section “The Philosophy Of Complex System Science”, that there are differences between the study of complexity in the broad sense and complex system science. The two are related endeavors, but it seems like consensus on terminology and properties of complex systems is higher in CSS than in the definition of complexity in the broad sense.

- 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”.

**R14:** We have completely re-written this section, integrating the most salient parts throughout discussion and conclusions. See also **R15** and **R16**.

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”.

**R15:** We have provided more details on these case studies, adding a citation to each example and elaborating on the advancements at lines 454-464 in the discussion.

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.

**R16:** Thank you very much for this comment. We have been reflecting on this comment and agree. We removed the sentence and now cite Brown (1995) and Liu (2007) in our conclusions, reporting ideas consistent with your comment about integrating ecology and conservation with broader disciplines such as politics, ethics, and governance at lines 501-528.

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**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.

**R17:** We thank the reviewer for the time dedicated in assessing our work, their positive attitude towards it, and for constructive feedback and suggestions.

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.

**R18:** Our revised manuscript is based on two parts: (i) a critical review, which is not based on the “complexity papers” (i.e., paper including “ecological complexity” in title or keywords), but rather on literature in CSS that we deemed relevant to the critical review objective, and (ii) a text-mining analysis, which evaluates in the “complexity papers” how often the features identified in the critical review were used. The bibliometric analyses were used only for part (ii), to assess what the typical ecologist means when referring to “ecological complexity.”. This is now clarified at lines 86-114, in the introduction, and explained in more detail in the methods.

The referee is correct in pointing out that not all papers on complexity will have “ecological complexity” in title or keywords, however, all papers with complexity in the title or keyword should be (more or less tangentially) about complexity. Therefore, we believe that step (ii) allows us to assess a random sample of literature on complexity in ecology, and thus our inference should be, in principle, unbiased. We draw a statistical inference for the entire body of “ecological complexity studies” by taking samples that should be relatively unbiased, because we fail to find a way in which searching for “ecological complexity” in the title or keywords of a paper would bias our results in a certain way (e.g., a higher or lower similarity with control papers, or the disproportionate use of any of the features).

We note that in an era of information overload in scientific literature, there is virtually no single systematic literature review able to capture the entirety of papers on any given topic. Given the fluid definition of complexity and the inability to objectively define "complexity papers", any set of keywords to capture complexity papers (no matter how broad) will leave out some studies and aspects of the field (see, for example Farrell et al., 2022 available at: https://royalsocietypublishing.org/doi/10.1098/rspb.2021.2721). Our keyword search, although simple, captured a coherent set of papers that can be analyzed to draw general and meaningful inferences. Pragmatically, this is something we archived as demonstrated by the emergence of well-known clusters of theories and other patterns (e.g., see Fig. 2, 3, 5) in our results.

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.

**R19:** Please see responses at **R6** and **R7**. When we chose the 23 features, we did so by prioritizing terms that encompass a broad range of meanings. We have revised the manuscript substantially, explaining our strategy and its limitations at lines 355-380. Our approach (i) allows us to have broader concepts in the way we define complex systems, but (ii) it increases the risk of false positives when conducting our bibliometric analysis.

To address point (i) we added, in table 1, examples of words to contextualize what we referred to with the most general “features” (column “Related concepts''). For instance, “entropy” is often used as a measure of diversity, collective intelligence can be seen as an emergent property, food webs are networks, niche construction is related to adaptation. We needed a balance between the hundreds of theoretical constructs in ecology and general, core themes in complexity system science that could be useful to orient ecologists interested in embracing this paradigm.

To address point (ii), we ran a topic modelling analysis using the Latent Dirichlet allocation (LDA) method to assess whether the 23 features we selected through the critical review (Table 1) are meaningful to describe complex ecological systems. LDA showed our selected terms do matter for the papers more than should be expected by the average term (see the new Figure 2 and lines 154-159). We also checked whether our features were among the 0.5% of most important words in the analysis. We found that all our features matched this criteria for several topics, not only considering their occurrence across different topics, but also their average importance. This result highlights that the features were more important in characterizing *complexity* than *control* articles. All in all, additional analysis suggests features in Table 1 are representative of the topics discussed in our papers, demonstrating the reliability of our method. However, if the referee has additional suggestions of a more appropriate analysis that could further validate our point, we are happy to test more strategies.

In addition, the paper should interact more with the broader science of science literature and methodology (e.g. the work of James Evans).

**R20:** We agree with the reviewer that the science of science literature is integral to this type of inquiry, and have made our prior reliance on it clearer in the revision (see, e.g., line 102 in the introduction). In this work, we heavily relied on science of science analyses, including co-citation networks and bibliometric mapping (and, after the revision, also a topic modelling analysis). To make this link more apparent, we have now cited the seminal works by James Evans and colleagues (DOI: 10.1126/science.aao0185) which defined *science of science* as a discipline. Again, if there is a more specific point that the referee feels like is missing from the text, we are open to incorporate relevant suggestions.

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.

**R21:** We thank the referee for this feedback. However**,** without additional details, we found it challenging to fully address this comment. First, we are left wondering which clusters were expected to emerge that are missing. We would like to point out that our interpretation of the cluster is very broad, in the sense that there is within cluster variability. It could also be that some of the ideas that the Reviewer has in mind are less evident but present as subclusters in the clusters we identified. Second, this is an unsupervised analysis and hence we cannot influence or decide the outcome (i.e. the number of clusters emerging from the analysis). Importantly, this analysis is not even affected by the features – it is just based on the literature cited within the 172 papers, and thus is subjective only in how we interpret the clusters (not in how many clusters emerge). We now clarified in the methods at lines 698-701.

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.?

**R22:** In our opinion, what is lacking is synthesis – which motivated us to work on this review. We think the former results from the latter (a lack of synthesis leads to too many definitions) or possibly it is a feedback loop (by having too many definitions, more efforts are needed for synthesis and the hurdle to do so is higher). This is why we believe our bibliometric analysis (while recognizing limitations and caveats) is a good approach to lower the hurdle. We made this more clear in the introduction (lines 86-114), also in the lines 319-343, and (lines 510-520).

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.

**R23:** Thank you for this insight, which we have added this aspect at the end of paragraph one in the new section “The Philosophy Of Complex System Science”(lines 178-182).

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?

**R24:** Please see **R6** and **R7** for more context on how we selected the 23 features. First, we do agree that there are important theories related to complexity that should be discussed in our manuscript (which is a point now more explicit; see lines 436-449). These are not, in our view, features of complex systems, but “efficient theories” (sensu Marquet et al. 2014) that can be used to represent laws that characterize phenomena occurring at least at some levels of complex systems. Maximum entropy theory of ecology, metabolic theory of ecology, the recent general theory of temperature dependence in biology or the synthetic theory of biodiversity based on fractals are all examples of these sorts of efficient theories, and are now discussed in a new paragraph in the discussion as an example of successful integration of CSS and ecology (lines 436-449). Regarding the comment on keywords, please see **R18** and our comment on how we see our sample of papers referring to “ecological complexity” as a random sample of papers in ecology that deal with complexity.

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.

**R25:** Thank you, this is a very insightful comment and we agree that the previous interpretation of our analysis was misleading. There are indeed cases where complexity emerges from simple rules. We kept this point in mind while revising our draft and now discuss this aspect in the context of natural computation, at lines 461-471.

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.

**R26:** We have added the section “A Brief History Of Complex System Science” to provide more contextual background on CSS beyond ecology, as well as several examples in the lines 422-435 of how ecology and CSS have been benefitting for advances in both fields.

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.

**R27:** Thank you for clarifying this point. In reworking the conclusions of the paper we have excluded the citation to Feynman.

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

**R28:** (i) “*How can you tell that papers aren't simply mentioning the terms as analogies or adjacent work or disputing other work*?” It is difficult to tell the context in which each paper mentions the different features. Whether the feature is mentioned positively or negatively should not matter, as papers debating a feature typical of complex systems should be in principle more appropriately related to complexity than those not mentioning that feature. It is certainly possible that a paper mentions a feature when referring to other work, but this should be buffered by a focus on the relative frequency of feature use (i.e., it seems highly unlikely that the paper repeats over and over a feature if that feature is not central to the paper message).

(ii) “*How can you tell how many features the model in those papers actually addresses*?” We are unsure about what this comment refers to, in the sense that we did not discuss models within the complexity papers. For instance, some papers are likely only conceptual (no formal quantitative analyses). We are open to address this comment if the referee feels like the revised version of our manuscript still lacks clarity in this regard.

(iii) “*How many other concepts are in the non-complexity papers*?” We do not know how one would quantify how many topics there are in each paper, as common topic modelling approaches require setting a fixed number of topics in each corpus. Our LDA analysis was run with 100 topics and we found that the features identified in Table 1 were typically less important in non-complexity papers than in the complexity papers.

Regarding the comment on differences between controls and complexity papers - we stressed in the manuscript that the two types of papers are actually not that different (e.g., lines 389-399). This is actually a result which suggests the need for a paper more clearly synthesizing the literature on complexity, as we attempted to do in our work.

1. [↑](#footnote-ref-0)