Peer Community In Ecology

From Paradigm to Publication: What Does the Pursuit of Novelty Reveal in Ecology?

François Munoz *based on peer reviews by* ***Francois Massol***, ***Matthias Grenié*** *and 1 anonymous reviewer*

Gianluigi Ottaviani, Alejandro Martinez, Matteo Petit Bon, Stefano Mammola (2025) On the quest for novelty in ecology. bioRxiv, ver. 4, peer-reviewed and recommended by Peer Community in Ecology. https://doi.org/10.1101/2023.02.27.530333

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In this study, Ottaviani et al. (2025) examined the variation in the use of terms related to ”novelty” in 52,236 abstracts published between 1997 and 2017 across 17 ecological journals. They also analyzed the change in the frequency of terms related to ”confirmatory” results. Their findings revealed a clear and consistent increase in the use of ”novelty” terms, while the frequency of ”confirmatory” terms remained relatively stable. This trend was observed across all the ecological journals, with the exception of Austral Ecology. Furthermore, the greater use of ”novelty” terms was correlated with higher citation counts and publication in journals with higher impact factors. These findings should prompt further reflection on our research practices and may be connected to ongoing discussions in the philosophy of science.

Thomas S. Kuhn’s seminal work, *The Structure of Scientific Revolutions* (1962), challenged traditional views of scientific progress. Central to Kuhn’s argument is the idea that science progresses through periods of adherence to a dominant ”paradigm”—a framework that provides scientists with puzzles to solve and the tools to solve them. A scientific crisis arises when the paradigm fails to address emerging anomalies, leading to the replacement of the old paradigm with a new one, a process Kuhn calls a ”scientific revolution.” Kuhn’s perspective stands in stark contrast to previous views, which held that science progresses through the steady accumulation of truths or the gradual refinement of theories, often guided by the scientific method. One might wonder if the growing emphasis on ”novelty” in ecological research mirrors the idea that theories are gradually refined until an exceptional discovery sparks a paradigm shift. In ecology, such a shift could be seen in the transition from niche-based theories of biodiversity dynamics (1960s-2000) to the radical neutral theory (Hubbell, 2001), which posits that diverse ecosystems can exist without niche differences. This paradigm was initially met with fierce opposition but eventually led to more integrative theories, recognizing the combined influence of both niche-based and neutral processes (Gravel et al., 2006, among others).

What, then, is the current paradigm in ecology? Kuhn's theory of scientific progress suggests alternating periods of "normal" and "revolutionary" science. Normal science is characterized by cumulative puzzle-solving within established frameworks, while revolutionary science involves major shifts that can invalidate previous knowledge, a phenomenon Kuhn terms "Kuhn-loss." Kuhn rejected both the traditional and Popperian views on scientific revolutions. He argued that normal science depends on a shared commitment to certain beliefs, values, methods, and even metaphysical assumptions, which he referred to as a "disciplinary matrix" or "paradigm." This collective commitment is essential for scientific progress and must be instilled during the training of scientists. Kuhn's emphasis on the conservative nature of normal science contrasts with the heroic idea of continuous innovation and Popper's view of scientists constantly seeking to falsify theories. However, contemporary ecological research often follows the hypothetico-deductive approach championed by Popper. In light of these contrasting views, one might ask: What is the status of "novelty" in modern ecology? Is it contributing to the gradual solving of scientific puzzles, or is it focused on refuting hypotheses? Should "novelty" and "confirmatory" research be seen as opposites, or should both contribute to the advancement of science? Finally, is the increasing use of "novelty" terms a precursor to a scientific revolution, as Kuhn defined it, or merely a semantic trend driven by editorial policies aimed at attracting readers rather than contributing to real scientific progress?

In conclusion, Ottaviani's study provides compelling evidence of the growing use of "novelty" terms in ecological journals, but it remains unclear whether this trend signals the onset of a Kuhnian "scientific revolution." This work should spark further discussion on the nature of current research practices, which may either facilitate or hinder the emergence of new paradigms.

References:

Gravel, D., Canham, C. D., Beaudet, M., & Messier, C. (2006). Reconciling niche and neutrality: the continuum hypothesis. Ecology letters, 9(4), 399-409. https://doi.org/10.1111/j.1461-0248.2006.00884.x

Hubbell, S. P. (2001). The Unified Neutral Theory of Biodiversity and Biogeography, vol.1, Princeton and Oxford: Princeton University Press.

Kuhn, T. S. (1962). The structure of scientific revolutions. International Encyclopedia of Unified Science, vol.2, 1962.

Ottaviani, G., Martinez, A., Petit Bon, M., Mammola, S. (2025). On the quest for novelty in ecology. bioRxiv, ver.4 peer-reviewed and recommended by PCI Ecology. https://doi.org/10.1101/2023.02.27.530333

Reviews

Evaluation round #2

DOI or URL of the preprint: https://doi.org/10.1101/2023.02.27.530333
Version of the preprint: 3

Authors' reply, 18 April 2025

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Decision by François Munoz, posted 12 April 2025, validated 14 April 2025

Dear authors,

The reviewers and I are pleased with your revision.

One of the reviewers suggested a few changes.

I would be happy if you could take them into account in a new version.

This is a minor revision.

I think we can then complete the process quickly.

Best regards,

François Munoz

Reviewed by Matthias Grenié, 17 March 2025

I would like to take the oppportunity of this review to thank the authors for their revised work, as well as their detailed answers to my previous comments.

I think their manuscript is now stronger both methodologically and conceptually given the additional discussions, and I don't have further comments to make.

I think this manuscript is great contribution to the debate on confirmatory versus novel research.

I'd also like to thank the recommender for giving the opportunity to review this manuscript.

Reviewed by Francois Massol, 25 March 2025

The revision of the paper by Ottaviani et al. is a clear improvement – kudos to the authors for managing this so well!

I have no major remark. Since I have spotted some items that might need some rewriting, here is the list:

General minor comment: once printed on paper, the figures are difficult to read. This might be a dpi issue – my advice would be to make sure all figures are as readable as possible given that they contain quite a number of character strings (notably figs 1 and 3).

Line 48: Connor -> Conner

Lines 79-81: While the current wording is not wrong, I would probably argue that the relationship between IF and the frequency of terms has a lot to do with the link between prestige (or perceived prestige) of journals and the way the editors in those same journals encourage or discourage certain types of writing. The "journal", by itself, is not really creating this association – in my opinion, this is all in the mind of the beholders, here the beholders being the editors and reviewers working for these journals.

Line 98: The wording does not seem to imply that (scoring 1 for at least one novelty word) => (scoring 1 for novelty). Or, in other words, it is well explained how you count occurrence of words in abstracts, but not how you decide that the abstract is in the category "novel" and/or "confirmatory". My understanding is that it is the case if you have at least one mention of these words. It might be good to explain this in a more explicit fashion.

Pages 7 and 10: About the IF, I cannot find the information on the year of IF, and especially whether it was kept the same for all papers independently of their publication year or whether it was actualized – for all the papers, did you use the IF as given by Clarivate/ISI at the time of publication or as the IF was in 2017 (or 2023, or any recent year actually)?

This question has a practical consequence: on page 10, you show a positive relationship between the age of the paper and its IF. I'd suggest two completely different interpretations based on how the IF was reported in your dataset:

* if the IF reported for a paper is the one of its journal at the time it was published, then it might be that all IFs have declined over time, so that only old papers can claim a high IF.

* if the IF reported for a paper is kept fixed for all papers belonging to the same journal (and taken at the most recent value), then it might mean that IFs have generally increased with time but that journals publish more

papers when they have lower IFs (and thus there would be an over-abundance of old papers in now high-IF journals).

Page 7: Regarding the linear model on the IF – does it mean you used a Gaussian distribution? ("linear model" now means so many different things, I'm a bit lost, sorry). If so, did you use the raw IF or a transformation (like log IF) in order to correct for the asymmetry/impossibility of becoming negative? (using a Gaussian distribution with a variable that will never be negative means you are wagering some bet [in the likelihood of the model] on the possibility that the variable takes negative values, even when you know it mechanically cannot do so)

Congratulations again for the revision.

Evaluation round #1

DOI or URL of the preprint: https://doi.org/10.1101/2023.02.27.530333
Version of the preprint: 2

Authors' reply, 25 February 2025

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Decision by François Munoz, posted 09 January 2025, validated 15 January 2025

Dear authors,

Thank you for submitting your interesting work to PCI Ecology.

You will find enclose two reviews that provide useful suggestions for more in-depth analyses and discussion of the findings.

Please consider their suggestions in a revised version of the manuscript.

We look forward receiving your new manuscript for further consideration.

Sincerely,

François Munoz

Reviewed by anonymous reviewer 1, 08 January 2025

I was asked to review the manuscript entitled "On the quest for novelty in ecology". I enjoyed reading the straightfoward and clear study, that is concise and precise. I think the framework in which the authors thought about the study is clear. The methods are sounds and well explained, and the analyses easy to reproduce as all of the materials are openly shared by the authors. The paper is a good quantitative contribution to the debate of how papers are increasingly framed as "novel". I, however, have the impression that the paper falls short on its initial goal of testing to what extent papers in ecology use novelty terms. While I fully understand the appeal of a clear manuscript centered around a single question with a single analysis to answer it, I have the impression that the manuscript could be made stronger with minimal additional analyses. Also, I think the discussion could be broaden to link to more current pressing issues as well as taking stronger stances regarding the results of the paper.

The authors mainly focus on one analysis in their manuscript regarding the temporal trend of the proportion of novelty versus confirmatory terms in the abstracts of the corpus of articles. The results are clear, but I think much more could be added to papers, with minimal additional analyses and could deepen the message of the manuscript. For example, the authors evaluated the prevalence of novelty terms in abstracts, but they don't discuss its prevalence in titles. I would expect that titles are even greater vehicle to convey novelty as they are the first thing that would attract a potential reader. If not in the main text, I, and I'm sure any potential reader, would ask how these trends compare across journals. Could we see, as supplementary material, the raw

trends across the 17 journals? Are there differences across journals? Also society versus commercial journals? In particular, given that the authors mention 65% of journals having novelty as a criteria for publication, are these specific journals showing a stronger trend in novelty terms than the others? Similarly, the used linear mixed-models don't have they variance reported (see Nagakawa's R^2). I would be interested to see to how large the variance explained by the journal random effect is relative to one of fixed effects.

I also have the impression that the discussion and conclusion of the manuscript don't take any stance regarding novelty and confirmatory terms. While a neutral stance gives the impression of rising above it all, I do think that given the framework of the manuscript, a more engaged stance should be adopted by the authors. What do they think of their results? Are there concerning? What does "novelty"" really means? Should novelty matters in ecology or not? I would have liked to have a slightly longer discussion and recommendation on these points. In particular, I find the conclusion half-hearted. I understand the authors message to consider each paper in itself with its specificities without focusing on novel versus confirmatory results, but it goes against the introduction of the paper. In the introduction it is pointed out that science is cumulative with rare breakthrough, meaning confirmatory results should be more important for its advancement and novel results rarer. I do think that this should emphasize the importance of dropping the novelty criterion for journals and increase the importance of confirmatory results. Given the current debate on the science reproducibility crisis, I would also have expected some mention of the importance of confirmatory results in this regard.

Reviewed by Francois Massol, 06 December 2024

In this paper, Ottaviani and colleagues show that the use of novelty-esque words has increased in the abstracts of ecology papers since 1997, while the use of words associated with replication and reproducibility has stayed stable. Based on this analysis, they offer some interpretations of these findings, one of which being that journals are partially responsible for the increase in novelty-related adjectives.

Overall, I really appreciate the work performed in this study. Although short, this paper will be useful to the community in these times of redefinition of research assessments, journal rankings, and viability of research system models.

I have a few comments, some of them methodological, some linked to the existing literature on the topic. I hope these will help the authors improve their manuscript.

1. Regarding the myth of the "lone genius" (page 3), to me the most enlightening book on the topic has been Clifford Conner's "People history of science". I very strongly suggest this reading to the authors. It is full of counter-examples to the common myth and it also suggests that "lone scientists" (even when they are brilliant) make mistakes while groups are more likely to self-correct.

Conner, C. (2005) A People's History of Science: Miners, Midwives, and Low Mechanicks, Nation books.

2. Because your work was performed on abstracts of ecology papers, I was wondering whether you could contrast your findings with those of Weinberger et al. (2015) – they looked at whether common rules for writing abstracts were really associated to more citations (or less).

Weinberger, C. J., Evans, J. A. & Allesina, S. (2015) Ten Simple (Empirical) Rules for Writing Science. PloS Computational Biology, 11, e1004205.

3. The biggest correction I would suggest to this paper is about statistics: I do appreciate the use of linear models (with mixed effects), but I am not sure that using a Gaussian model (here, an 'lm' or 'lmer' in R) is appropriate; rather I think using a generalized linear model with a binomial distribution for the error would be better (your data is actually a collection of articles with a 0/1 response to the questions "is novelty in the abstract?" and "is reproducibility in the abstract?"). Or if you really prefer to work with proportions rather than counts, at least make the distribution "beta". However, in my opinion, there is really a large bonus to working with counts here insofar as it makes use of the information contained in the number of papers published by each journal each year (with proportions, you don't see this piece of information, and so the result might be biased by journal that publish fewer papers).

4. The last parts of the analysis (IF against novelty, citations against novelty, and IF against confirmatory) work on residuals of a previous analysis (a GAM on citations per paper corrected for age of the paper). In all statistical textbooks (and when you discuss with statistician colleagues), it is always more advisable to make a single model with all factors rather than make a second model on the residuals of the first. Could you rather do that here, maybe through a GAM that forces your linear predictor to be linear but still keeps the spline effect of paper age (if you really want to keep it that way)?

5. More generally, the M&M is too short – I had to read through the R script to really understand what was being done, this is not very friendly to colleagues allergic to stats...

6. In the reporting of the results, we don't have a clue as to what are the "beta's" and which tests were performed to get these p-values.

7. Last methodological point: can you control for the size of the abstract of each article (number of words per abstract)? Because longer abstracts could potentially contain more room for both confirmatory and novelty terms, so that might be a factor explaining part of the effect (or at least you could take that into account).