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Originality in scientific practice. Is it clear enough or should we continue discussing it?

Luis Marone[1,✉] & Martín R. Aguiar[2]

[1] *Desert Community Ecology Research Team (ECODES), Instituto Argentino de Investigaciones de las Zonas Áridas (IADIZA), Consejo Nacional de Investigaciones Científicas y Técnicas (CONICET). Mendoza, Argentina.* [2] *IFEVA-Ecología, Facultad de Agronomía, Universidad de Buenos Aires, CONICET. Buenos Aires, Argentina.*

Abstract

The guidelines for publishing articles, awarding grants, or obtaining a doctorate at an academic institution all stipulate originality. But how is it measured in practice? Research aimed at testing a creative solution (i.e., a novel idea) to an interesting, unsolved problem is undeniably original. But is the presence of a novel hypothesis a standard requirement or condition for evaluating the originality of a scientific proposal? It seems that such a condition would set the bar too high. While we value both plausible and original hypotheses in scientific proposals, the assessment of originality in day-to-day academic practice should hinge on whether the proposal addresses a research problem or question that remains unresolved. We highlight the balanced contributions of observation (e.g., natural history) and theory in inspiring and justifying scientific problems of significant interest. Identifying well-founded problems constitutes —or should constitute— the best way to evaluate originality in scientific practice.

[Keywords: epistemology, original hypothesis, original question, problematization, research problem]

Resumen. Originalidad en la práctica científica. ¿Está suficientemente clara o debemos continuar debatiéndola?
Las instrucciones para la publicación de artículos, la concesión de subsidios o la obtención de un doctorado en una institución académica exigen originalidad. Pero, ¿cómo se mide la originalidad en la práctica? La investigación que pone a prueba una solución original (i.e., una idea novedosa) a un problema o pregunta intrigante, sin resolver, es innegablemente original. Pero ¿es la presencia de una hipótesis novedosa un requisito o condición cuando se evalúa la originalidad de una propuesta científica? Nos parece que tal condición pondría la vara demasiado alta. Si bien valoramos tanto las hipótesis plausibles como las originales en planes, proyectos y artículos, la evaluación de la originalidad en la práctica académica cotidiana debería centrarse en si la propuesta aborda un problema o una pregunta de investigación que permanece sin tener una respuesta satisfactoria. Destacamos las contribuciones equilibradas de la observación (e.g., la historia natural) y la teoría a la hora de inspirar y justificar problemas científicos interesantes. Identificar problemas bien fundamentados constituye —o debería constituir— la mejor manera de evaluar la originalidad en la práctica científica.

[Palabras clave: epistemología, hipótesis original, pregunta original, problema de investigación, problematización]

The originality

Something is original if it enriches the world with something genuinely new, which did not exist before the creative act (Bunge 2001). Among the objectives and scope of Ecología Austral is "the publication of scientific research results that represent an original contribution to ecological knowledge". Our universities, for example, also require that a doctoral thesis "be an original contribution to a field of knowledge" (Ordinance 49/2003, National University of Cuyo) or that "it reveals the originality of the student's contribution to existing knowledge" (Doctorate in Agricultural Sciences, University of Buenos Aires). In our field of ecology and evolutionary biology, any thorough analysis of the instructions for publishing articles, awarding projects or obtaining a doctorate in an academic institution will surely show that originality is mandatory. But how is originality measured? Some might think this question already has an answer: after all, there are now numerous peer reviewers, committees and commissions that evaluate the originality of research proposals. Despite that, we believe the question still requires reflection and we sustain that any assessment of originality should be inspired by the scientific method. But how does the scientific method relate to human curiosity and creativity?

Editora asociada: María Semmartin
✉ lmarone@mendoza-conicet.gob.ar

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In simple terms (see Figure 1), and based on theory and previous knowledge, the scientific method consists of formulating interesting and intriguing questions (i.e., research problems) and proposing reasonable solutions (i.e., plausible working hypotheses), and then looking for evidence to show whether the hypotheses are (or are not) provisionally true. In this context, the originality of a scientific proposal is often related to the originality of its working hypothesis. Research focused on testing a creative solution (i.e., a novel idea) to an unsolved problem is undeniably original: it is the ultimate expression of originality. But is the presence of an authentic novel hypothesis a typical requirement or condition imposed by publishers, funding agencies and universities when evaluating the originality of a scientific proposal? It seems that they might be setting the bar too high.

Before continuing, a brief digression on the roles of hypotheses in scientific research. Although the inclusion of original and disruptive hypotheses may not be the rule in everyday research proposals, there are other reasons to include plausible —though not necessarily original— hypotheses in them. First, hypotheses enable the reader to understand how a researcher believes the reality under investigation to be. Second, hypotheses are the genuine guide that drives the experimental or comparative design and data gathering of any study. Third, a priori hypotheses connect the general theoretical framework with the specific predictions to be tested, lending plausibility and explanatory power to the inferences made from the empirical results (Marone 2024). Still, since this essay aims to discuss only the criteria used to evaluate the originality of research proposals, we will not evaluate in depth the broad role that hypotheses play in scientific research.

In our experience as authors and reviewers, the originality of a hypothesis is rarely the primary criterion used to evaluate the overall novelty of a proposal. Were this the case, the majority of research proposals would fail to meet the standard. However, the aforementioned framework for the scientific method includes the hypothesis, but also the data or evidence and the research problem or question (Figure 1). So, we wonder whether this broader perspective sheds light on the sometimes-opaque ways in which the originality of manuscripts, projects and theses is assessed.

It seems controversial whether simply collecting new data (e.g., in a new location, in a different year, on a similar species) can bring authentic originality to a study. The typical justification "we are going to

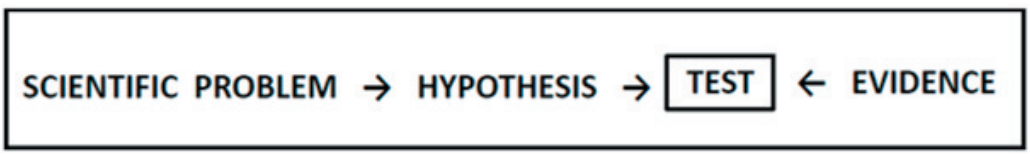


Figure 1. Simplified scheme of the scientific method. Although in everyday practice the process of knowledge creation is non-linear and admits certain variability, it often starts —departing from prior knowledge— by establishing a scientific question or problem (i.e., a gap in knowledge that remains largely or totally unsolved and which does not arise in a vacuum, but as a result of a disagreement with previous knowledge; e.g., theories). Then, the researcher selects and offers a hypothetical solution to the problem: the working hypothesis (i.e., a general and testable explanation for the initial problem). The final step involves gathering evidence to put the hypothesis to trial which, eventually, may show —provisionally— that the hypothetical solution solves the scientific problem. This process is surrounded by numerous assumptions or auxiliary hypotheses (e.g., general theories, other hypotheses, knowledge about the techniques used to gather data). We maintain that in everyday practice, the originality of most research proposals is or should be assessed by establishing whether the research problem they describe is intriguing enough and well-founded, because in such cases the research process will be innovative in some recognizable way, beyond including or not including a novel hypothesis.

Figura 1. Esquema simplificado del método científico. El proceso de creación de conocimiento admite variabilidad en la práctica diaria, pero a menudo comienza planteando una pregunta o problema de investigación (i.e., un hueco en el conocimiento previo que permanece no resuelto y que no surge en el vacío sino, por ejemplo, como resultado de un desacuerdo con datos o teorías bien establecidas). Para resolverlo, la investigadora selecciona y postula una solución plausible: la hipótesis de trabajo (i.e., una explicación general del problema de partida, pasible de ser puesta aprueba). Finalmente, se recoge evidencia para poner a prueba, y eventualmente corroborar, la solución hipotetizada. Para ello, se recurre a numerosos supuestos (e.g., teorías, otras hipótesis, conocimiento que justifica el uso de las técnicas de obtención de datos). Hipotetizamos que, en la práctica diaria, la originalidad de las propuestas científicas es o debería ser evaluada indicando si el problema de investigación que abordan es intrigante, significativo y está bien justificado. En ese caso, el proceso de investigación podrá ser considerado innovativo u original, más allá de que incluya o no una hipótesis de trabajo novedosa.

conduct (or have conducted) this study because there is insufficient information on the subject" might not adequately justify originality (Peters 1991). Otherwise, simply accumulating data files would amount to doing science. However, data can lend a touch of originality to a study when they are assessed in the context of previous knowledge (i.e., the complete process indicated in Figure 1). For example, some unexpected results of a well-designed sampling, simulation or metanalysis can contribute something new to knowledge when they contradict the specific predictions of previous theoretical or empirical frameworks (i.e., an explanatory theory or a well-established pattern, respectively). The disagreement does not yet refute the established hypothesis, but it allows for the emergence of a scientific research problem by questioning why an unexpected result occurred and whether we should revise what we knew until now (Marone and Jaksic 2025). A scientific proposal that correctly poses an interesting problem or question may be then original to a certain extent, although in a humbler way than a proposal with a disruptive idea.

Research problems, for example of foraging ecology, such as why most granivorous birds prefer grass seeds to forb seeds in deserts around the world, or whether seed-eater specialists are also neophobic consumers [why?], or whether context-dependent foraging behaviour hinders the manifestation of general ecological laws? [why?], could all inspire original research because they aim to solve interesting research problems. To answer them, the researcher could propose a well-known hypothesis when the degree of generality or universe of inference of the hypothesis remains unknown and deserves to be tested in a new empirical context (Cohen 2017). In other words, when the available evidence is still insufficient or controversial to resolve the problem. Certainly, the researcher could also propose a completely new idea, but this is a much rarer event. In all cases, however, the project would have a touch of originality if it triggered a creative research process aimed at solving a scientific problem that has not yet been solved. We submit that the burden of originality lies in addressing a question that still intrigues (i.e., the scientific problem in Figure 1).

Overall, in routine academic evaluation, the test of a radically original idea does not appear to be a requisite for manuscripts, projects or theses. Successful proposals could (hopefully!) include a well-known and not-necessarily new hypothesis aimed at answering a stimulating open question since the assessment of originality of the research process would depend on the degree to which the question remains unsolved. But in such a case, the proposal should carefully describe the logical steps that reveal that the peculiar problem exists, is conceptually important, and perhaps also of applied-human interest. Unfortunately, previous experience has shown us that many research proposals do not reasonably describe the specific problem that justified or would justify the research, but instead refer to overly broad problems (e.g., global or climate change, biodiversity loss and conservation, desertification, biological invasions), to which the proposal aims to contribute in some (often unspecified) way. Without a detailed articulation of the ideas and facts that give rise to the specific problem (e.g., how much is known about the problem, what are the main gaps in that knowledge, why the problem remains unsolved despite previous studies), the supposed originality of the proposal cannot be assessed. Therefore, even if the proposal does not put forward any disruptive hypotheses, the evaluation of its scientific merit will depend on whether it includes a detailed 'problematization' that shows that the proposal addresses a specific, intriguing and unresolved research problem (Marone 2019).

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