



WHAT IS THE IMPORTANCE OF YOUR RESEARCH? A FRAMEWORK FOR CLEAR ECOLOGICAL RELEVANCE

Yuri Costa^{1,2*}; José A.C.C. Nunes^{1,2}; Charbel Niño El-Hani^{2,3}; Francisco Barros^{1,2,4}

Abstract

How to clarify the relevance of research is a challenge for ecologists since this is not a trivial question for any scientist but may be especially difficult for early-career researchers, who often find more difficulty in providing answers that are synthetic, logical, and cogent. However, a clear answer to this question is critical for obtaining funding and is increasingly required by journal editors in order to send papers to review, let alone to attract readers to the published paper. Here, we argue that relevance should in fact appear in all steps of ecological research, including project preparation (e.g., for funding requests), manuscript submissions, oral communications, and also in media releases. Herein we discuss a framework for ecological relevance based on five key elements: clear connections with theory, knowledge gap, novelty, methodological innovation, and applicability. There are different ways of combining these elements, but in order to make the relevance of a study clear, ecologists should make explicit how these elements are connected with their main research question. Journal editors and grant agencies or donors decide on the relevance of the submitted works or proposals. We argue that categorically deciding whether or not an article is relevant is a delicate issue, particularly if one considers how scientific works can range from a gradient of no relevance to extreme relevance. We hope that with this simple “must-have argument list”, ecologists, especially in early careers, can enhance and show the relevance of their work in improving the field of ecology and, ultimately, human society.

Keywords: Research relevance. Ecology. Theory. Methodology. Applicability.

¹UFBA – Federal University of Bahia, IBIO – Institute of Biology (“Instituto de Biologia”), Benthic Ecology Laboratory (“Laboratório de Ecologia Bentônica”), Salvador, Bahia, Brazil

²UFBA – Federal University of Bahia, INCT IN-TREE – National Institute of Science and Technology in Interdisciplinary and Transdisciplinary Studies in Ecology and Evolution (“Instituto Nacional de Ciência e Tecnologia em Estudos Interdisciplinares e Transdisciplinares em Ecologia e Evolução”), Salvador, Bahia, Brazil

³UFBA – Federal University of Bahia, LEFHBio – Teaching, Philosophy and History of Biology Laboratory (“Laboratório de Ensino, Filosofia e História da Biologia”), Salvador, Bahia, Brazil

⁴UFBA – Federal University of Bahia, CIEnAm – Interdisciplinary Centre for Energy and Environment (“Centro Interdisciplinar de Energia e Ambiente”), Salvador, Bahia, Brazil

*Corresponding author: yuricost@gmail.com

Submitted on: 6 Dec. 2022

Accepted on: 11 Dec. 2022

Published on: 31 Dec. 2022

© Copyright 2022



1 Introduction

During the development of research projects, we are constantly provoked, sometimes intimidated by the question: "What is the relevance of your research?" This is not a trivial question for any scientist, but may be especially difficult for early-career researchers, who often find more difficulty in providing answers that are synthetic, logical, and cogent. However, a clear answer to this question is critical for obtaining funding and is increasingly required by journal editors in order to send papers to review, let alone to attract readers to the published paper. For example, in countries like Swiss, there are laws that guide ethics committees in human health research to evaluate research projects considering that they must "answer a relevant research question" (SWISS FEDERAL GOVERNMENT, 2011). Although in most disciplines, such as ecology, this is not a legal requirement, we know that due to limited resources to meet various project funding requests, research relevance is one of the most recurrent criteria.

The question itself can be framed in different ways: "what is the contribution of your work to your research field?" or "how innovative is your research?" Or even "how is your work applicable?". Anyway, the answer must address the reasons "why this work should be done?" Some authors (e.g., SHAW; ELGER, 2013) consider that there are two research relevance types. Scientific relevance, more associated with advancing knowledge on a specific topic, and social relevance, when research provides some direct benefit to society. This conception of research relevance apparently contemplates fundamental objectives of science, but it has some limitations. Such problems arise when trying to apply these principles, since this view does not consider that scientific and social relevance are multidimensional and that different layers meet different values and aspirations of society (COREN, 1970). Thus, lack of elements to assess the studies relevance leads us to carry out a categorical evaluation of the research (i.e., relevant or irrelevant) based on these criteria. Additionally, focusing social relevance as an immediate practical ramification is disregarding evidence of innovation gained from advances from basic science. For instance, the contribution of James E. Lovelock as co-inventor of electron capture detector that eventually allowed for the sensitive detection of chlorofluorocarbons and pesticides (DA SILVA; TSIGARIS, 2023). It is important to be sensitive to the importance of basic science, otherwise we will be insisting on is merely short-term, readily visible gains (TERWILLIGER, 1970).

Another question that can be identified is an assessment of the research relevance linked to the rigor of the studies (i.e., evaluation of the design) (CARTON; MOURICOU, 2017; SHAW; ELGER, 2013), we here consider that the relevance of the study must be accessed mainly through

its research question. Since the research design belongs to the execution planning stage, it should be evaluated after the project merits. For example, when applying for a graduate program in which the project is required to participate in the selection, prioritizing the quality of the research question seems more reasonable, since knowledge in design and analysis are part of the student's training within the program.

Van Grunsven and Liefting (2015) argued that sufficient knowledge of the studied species and ecosystems is often essential to assess the ecological relevance and scientific merit of a paper. Here, we argue that relevance should in fact appear in all steps of ecological research, including project preparation (for funding requests), manuscript submissions, oral communications, and also in media releases. We consider that to communicate, for instance to peers or funding agencies, the research relevance, it is necessary to make explicit some arguments and reasons supporting such judgment of relevance. In addition, we suggest that explicit combinations among the different elements discussed here (which we may call "arguments for relevance") enable the reader/audience to fully understand how relevant a study is.

2 Methods

This framework was developed as a synthesis of the discussions carried out in the context of the experimental design discipline, where a conceptual map was created on the aspects that provide relevance to the different research projects. At the same time, a non-systematic literature review was conducted where articles related to scientific research relevance were searched using some search strings (e.g., (Relevance OR Important*) AND (Stud* OR Research*)) on the Web of Science platform. The articles retrieved in the search were selected by reading the titles and abstracts. Works that dealt with the discussion about the relevance of scientific studies in general and in ecology were selected for reading. After preliminary construction of the framework, improvements were obtained through discussions with specialists.

3 Results and Discussion

There is no universal answer to the question "What is the relevance of your work"? Therefore, how to combine several justifications that come to our minds seems to be an important requisite to give a good answer to it. Here, we aim to explore five elements that we consider central to the construction of an argument for relevance in ecological research:

1- Clear Connection with Theory: Any research must show a clear connection with a relevant theory in which the work is embedded (PICKETT; KOLASA; JONES, 2013).

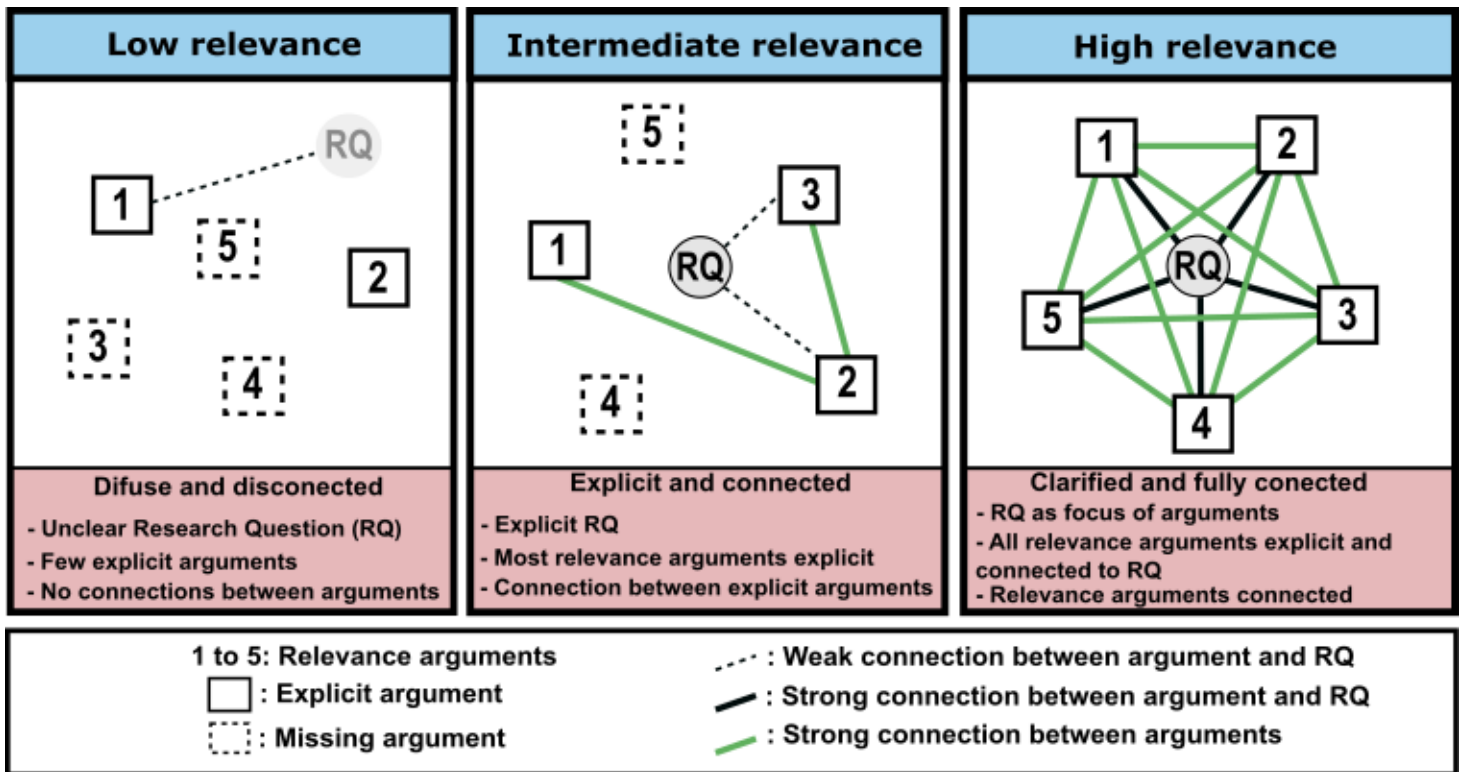


Figure 1. A framework for clear ecological relevance. Source: The authors.

Ecology without theory is simply the accumulation of situation-bound statements that are of limited predictive and explanatory power (Van GRUNSVEN; LIEFTING, 2015). There are many remarkable things to talk about your research topic, but the task is to build a clear argument that help others understand how your goals are connected to one or more ecological theories, for instance, what hypotheses or models related to such theories you will be testing.

In order to present the theoretical relevance of your work, it is important to explicitly identify the ecological theory or theories that provide the background to your specific study. Over the years of working with ecology, we concluded that many ecological researchers do not make explicit the theory (or theories) underlying their research. Not rarely do we observe the relevance of the study object (e.g. habitat, species) being used to justify research or arguments regarding a very large process (e.g. climate change) that will not be directly accessed or tested. Consider, for instance, a researcher interested in investigating the role of agonistic relationships among crustaceans that are selecting burrows in coral reefs (DALOSTO et al., 2013).

Suppose that this researcher decides to justify the relevance of the study based on the fact that coral reefs are extremely diverse ecosystems providing several environmental services.

That is, the argument follows the belief that some of the wells know the general characteristics of coral reefs is somehow relevant to the study. However, the studied phenomenon (i.e. agonistic behavior) is not directly related to the biodiversity of coral reefs or to the ecosystem services associated with them. In fact, it is clearly related to the role of competition in structuring reef communities and, thus, this study may contribute to advances in competition theory. In this case, a much better argument for the relevance of the study could be derived from pointing out its relationship with that theory.

It is also interesting to explain why the chosen biological model, or ecosystem, for that matter, provides a good setting for testing a given hypothesis derived from some specific theory (or theories). For instance, arguing why a certain species corresponds to a suitable model to test a given hypothesis is an important step towards showing the relevance of a study.

We should keep in mind that theory has an essential role in explaining ecological phenomena and also their relationship with physical and chemical variables (Van GRUNSVEN; LIEFTING, 2015).

It is important to note that some theories provide relatively simple representations of the natural world and allows researchers to make predictions that additional data can be used to test (MARQUET et al., 2014).

2- Knowledge gap: To justify a scientific study based on a clear knowledge gap in one's research field goes a long way towards showing its relevance. While its important to point out examples of papers that deal with a similar question (see SUTHERLAND et al., 2013; for examples of fundamental questions), it is crucial to be clear about what specific knowledge gap the study is filling.

In rare situations (e.g., endangered species or very unique habitats) an argument based on the uniqueness of the study site or species might convincingly show the relevance of an ecological study, but usually, this is not enough. Evidently, studies that investigate endangered species are an exception, since one in six species on the IUCN Red List are classified as "data deficient" (BLAND et al., 2017), and this information is fundamental for defining conservation strategies (SERROUYA et al., 2019). Apart from this exceptional case a weak but common argument for justifying ecological research is the absence of studies in a given site or habitat. Surely, our knowledge about virtually anything is limited, and thus there are ways some place which a given research question has not been addressed yet. Nevertheless, it assumes a cumulative wrong view of the construction of scientific knowledge in which advances would have to be made investigating hypotheses or models in every possible ecological system. It is much more interesting and productive to identify what we do not know yet in our research fields from a broader perspective, for instance, by identifying missing links in our understanding of specific ecological phenomena. If we are acquainted with the theoretical bases of our research field, we will likely be able to identify such missing links, and the hot research questions at a particular moment in the history of the field (SUTHERLAND et al., 2013). Systematic reviews are also helpful to identify knowledge gaps, and, also, to what extent advances have already been made towards filling gaps, avoiding wheel reinventions. Systematic surveys are also good to verify where the controversies lie in our field, as another way to justify the relevance of a study is to tackle a controversial point (KHAN et al., 2003). You need to show the paucity of studies on some missing theoretical link, which limits the understanding of a phenomenon, or process, and make explicit to your audience how your contribution may fill that gap.

3- Novelty: In general, all work intends to bring new information. The point here is to make it clear what kind of advance is made in the study, either theoretical or empirical, or both. Are you raising a new way of explaining an ecological relationship? Do you suspect that a variable that has not been studied before is the main predictor of the abundance of a given species? Do you believe that the

interaction of two or more factors, that have only been studied separately, is the best way to explain a pattern? Are you gathering evidence to test an untested ecological hypothesis or model? One should not miss the opportunity to highlight what is novel in the study and, even when we are replicating a study, something that is also necessary for ecological science (see, e.g., KELLY, 2019), we still have something to say about what is new, for instance, why doing such a replication may add to the support (or rejection) of a hypothesis or model. Or, if the relationship you are proposing has never, or rarely, been studied, it will be important to stress this fact and try to explain what (which mechanisms) made you think of this relationship. Constructing a good conceptual model helps to visualize the structure of such arguments. It is important to mention that for hypotheses and models to be convincingly tested, it is essential to replicate tests (see KELLY, 2019).

4- Methodological innovation: Another simple but also an important aspect that needs to be clarified is the methodological advances that the work provides (if any). If the study brings a contribution to improve scientific practices in a research field (e.g., by proposing a new data-gathering method, a new analytical method, or building a new research practice by incorporating different procedures and methods)¹ (GU et al., 2021; WADGYMAR et al., 2017), to the development of a new research tool, or else, it is necessary to make this clear.

Researchers frequently think "if I investigate the same ecological question with a better method would I find the same result?" In fact, many studies propose new methods to tackle particular research questions, and if this is the case of a study, it is of key importance to clearly explain how the novel method can substantially improve the understanding of a previously studied ecological relationship or mechanism, or, alternatively, what other reasons there may be to prefer the new method, for instance, how it might be faster, cheaper or more accurate than previously available methods.

¹Generally speaking, "scientific practices" refer to the cognitive, epistemic, socio-institutional, cultural and other processes carried out by scientific communities to build scientific knowledge. It is important to recognize that scientific practices are never simply mechanical, such that they could be simply reduced to methods, techniques, and procedures, but rather, also involve insight, creativity, imagination, serendipity, as well as are influenced by theoretical frameworks that guide, but do not determine, the questions asked, the methods employed, the analyses carried out, and the conclusions reached. Nonetheless, in any science, methods are an important element of research practices, and they can be analyzed in terms of procedures, i.e., organized actions for reaching a given goal, which are comprised, in turn, of specific techniques. Finally, there is nothing like a single scientific method that would characterize science as a whole. Sciences use a diversity of methods and are never reducible to just the application of methods.

5- Applicability: If a work has direct application in relation to socioenvironmental problems (e.g., ecological restoration or ecological pest control) (PICZAK et al., 2022; WYCKHUYS et al., 2020), it will be necessary to make clear the application context and how feasible it is, in concrete ways, to apply the outcomes of the work to the issue at stake. It can be the case, for instance, that the ecological research being reported can improve the local or global economy (reducing poverty, solving production problems, improving fisheries or fruit quality through reforestation, etc.). If so, it is relevant to clearly argue in this direction in the manuscript.

Combining the elements: The connection among the arguments for the relevance of a study (some of them discussed above) is an important element of papers reporting the results of ecological studies but is frequently obscure. The challenge is to clearly bring these arguments and their connection to the forefront such that the audience of a paper can easily perceive how important is the study. To properly combine relevant arguments is crucial because when this is achieved the audience's perception of the bearings of a study increases. There are different ways to combine these arguments; however, to make clear the relevance of a research question we need to be explicit about how these elements are connected (see Figure 1).

4 Conclusions

Journal editors and grant agencies or donors need to decide on the relevance of the received works. We argue that categorically deciding whether or not an article is relevant can be very fragile since scientific works can range from a gradient of no relevance to extreme relevance. We hope that with this simple “must-have argument list”, ecologists, especially in early careers, can enhance and show the relevance of their work in improving the field of ecology and, ultimately, human society.

CREDIT AUTHORSHIP CONTRIBUTION STATEMENT

Costa, Y: Conceptualization, Writing, Proofreading; Nunes, JACC: Conceptualization, Writing, Proofreading; El-Hani, CN: Writing, Proofreading; Barros, F: Conceptualization, Writing, Proofreading.

DECLARATION OF INTEREST

The authors disclose that they have no known competing financial interests or personal relationships that could have appeared to influence the study reported in this manuscript.

FUNDING SOURCE

FAPESB (“Fundação de Amparo à Pesquisa da Bahia”), CAPES/PrInt (Coordination for the Improvement of Higher Education Personnel/Institutional Programme for Internationalisation) program for the doctoral (BOL0077/2017) and sandwich doctorate grants (88887.363563/2019-00) granted to Yuri Costa.

REFERENCES

BLAND, L.; BIELBY, J.; KEARNEY, S.; ORME, C.D.L.; WATSON, J.E.M.; COLLEN, B. Toward reassessing data-deficient species. *Conservation Biology*, Washington, v. 31, n. 3, p. 531-539, 2017. Available from: <https://doi.org/10.1111/cobi.12850>.

CARTON, G.; MOURICOU, P. Is management research relevant? A systematic analysis of the rigor-relevance debate in top-tier journals (1994-2013). *M@n@gement*, Sceaux, v. 20, n. 2, p. 166, 2017. Available from: <https://management-aims.com/index.php/mgmt/article/view/3869>. Accessed on: 5 dec. 2022.

COREN, S. Is relevance relevant in research? *American Psychologist*, Washington, v. 25, n. 7, p. 649-650, 1970. Available from: <https://doi.org/10.1037/h0037873>.

DA SILVA, J.A.T.; TSIGARIS, P. The relevance of James Lovelock's research and philosophy to environmental science and academia. *Frontiers of Environmental Science & Engineering*, Beijing, v. 17, n. 3, art. 39, 2023. Available from: <https://doi.org/10.1007/s11783-023-1639-7>.

DALOSTO, M.M.; PALAORO, A.V.; SANTOS, S.; COSTA, J.R. Aggressiveness and life underground: The case of burrowing crayfish. *Behaviour*, Leiden, v. 150, n. 1, p. 3-22, 2013. Available from: <https://doi.org/10.1163/1568539x-00003034>.

GU, Y.; HU, L.; ZHANG, H.; HOU, C. Innovation ecosystem research: Emerging trends and future research. *Sustainability*, Basel, v. 13, n. 20, art. 11458, p. 1-21, 2021. Available from: <https://doi.org/10.3390/su132011458>.

KELLY, C.D. Rate and success of study replication in ecology and evolution. *PeerJ*, San Diego and London, v. 2019, n. 9, 2019. Available from: <https://doi.org/10.7717/peerj.7654>.

KHAN, K.S.; KUNZ, R.; KLEIJNEN, J.; ANTES, G. Five steps to conducting a systematic review. *Journal of the Royal Society of Medicine*, London, v. 96, p. 118-121, 2003.

Available from:
<https://doi.org/10.1177/014107680309600304>.

MARQUET, P.A.; ALLEN, A.P.; BROWN, J.H.; DUNNE, J.A.; ENQUIST, B.J.; GILLOOLY, J.F.; GOWATY, P.A.; GREEN, J.L.; HARTE, J.; HUBBELL, S.P.; O'DWYER, J.; OKIE, J.G.; OSTLING, A.; RITCHIE, M.; STORCH, D.; WEST, G.B. On theory in ecology. *BioScience*, Herndon, v. 64, n. 8, p. 701-710, 2014. Available from: <https://doi.org/10.1093/biosci/biu098>.

PICKETT, S.T.A.; KOLASA, J.; JONES, C.G. **Ecological understanding**: The nature of theory and the theory of nature. 2. ed. Burlington: Academic Press, 2007. Available from: <https://doi.org/10.1016/B978-0-12-554522-8.X5001-8>.

PICZAK, M.L.; ANDERTON, R.; CARTWRIGHT, L.A.; LITTLE, D.; MacPHERSON, G.; MATOS, L.; McDONALD, K.; PORTISS, R.; RIEHL, M.; SCISCIONE, T.; VALERE, B.; WALLACE, A.M.; YOUNG, N.; DOKA, S.E.; MIDWOOD, J.D.; COOKE, S.J. Towards effective ecological restoration: Investigating knowledge co-production on fish-habitat relationships with Aquatic Habitat Toronto. *Ecological Solutions and Evidence*, London, v. 3, n. 4, art. e12187, p. 1-10, 2022. Available from: <https://doi.org/10.1002/2688-8319.12187>.

SERROUYA, R.; SEIP, D.R.; HERVIEUX, D.; McLELLAN, B.N.; McNAY, R.S.; STEENWEG, R.; HEARD, D.C.; HEBBLEWHITE, M.; GILLINGHAM, M.; BOUTIN, S. Saving endangered species using adaptive management. *Proceedings of the National Academy of Sciences of the United States of America*, Washington, v. 116, n. 13, p. 6181-6186, 2019. Available from: <https://doi.org/10.1073/pnas.1816923116>.

SHAW, D.M.; ELGER, B.S. The relevance of relevance in research. *Swiss Medical Weekly*, Basel, v. 143, n. 1920, p. w13792 (1-4), 2013. Available from: <https://doi.org/10.4414/smw.2013.13792>.

SUTHERLAND, W.J.; FRECKLETON, R.P.; GODFRAY, H.C.J.; BEISSINGER, S.R.; BENTON, T.; CAMERON, D.D.; CARMEL, Y.; COOMES, D.A.; COULSON, T.; EMMERSON, M.C.; HAILS, R.S.; HAYS, G.C.; HODGSON, D.J.; HUTCHINGS, M.J.; JOHNSON, D.; JONES, J.P.G.; KEELING, M.J.; KOKKO, H.; KUNIN, W.E.; LAMBIN, X.; LEWIS, O.T.; MALHI, Y.; MIESZKOWSKA, N.; MILNER-GULLAND, E.J.; NORRIS, K.; PHILLIMORE, A.B.; PURVES, D.W.; REID, J.M.; REUMAN, D.C.; THOMPSON, K.; TRAVIS, J.M.J.; TURNBULL, L.A.; WARDLE, D.A.; WIEGAND, T. Identification of 100 fundamental ecological questions. *Journal of Ecology*, London, v. 101, n. 1, p. 58-67, 2013. Available from: <https://doi.org/10.1111/1365-2745.12025>.

SWISS FEDERAL GOVERNMENT. *Loi fédérale relative à la recherche sur l'être humain, LRH [Federal Act on Research involving Human Beings]*. Available from: <https://www.fedlex.admin.ch/eli/cc/2013/617/fr>. Accessed on: 8 dec. 2022.

TERWILLIGER, R.F. To what relevance is research relevant? *American Psychologist*, Washington, v. 25, n. 12, p. 1174-1175, 1970. Available from: <https://doi.org/10.1037/h0037907>.

Van GRUNSVEN, R.H.A.; LIEFTING, M. How to maintain ecological relevance in ecology. *Trends in Ecology & Evolution*, London, v. 30, n. 10, p. 563-564, out. 2015. Available from: <https://doi.org/10.1016/j.tree.2015.07.010>.

WADGYMAR, S.M.; LOWRY, D.B.; GOULD, B.A.; BYRON, C.N.; MACTAVISH, R.M.; ANDERSON, J.T. Identifying targets and agents of selection: innovative methods to evaluate the processes that contribute to local adaptation. *Methods in Ecology and Evolution*, London, v. 8, n. 6, p. 738-749, 2017. Available from: <https://doi.org/10.1111/2041-210X.12777>.

WYCKHUYS, K.A.G.; LU, Y.; ZHOU, W.; COCK, M.J.W.; NARANJO, S.E.; FERETI, A.; WILLIAMS, F.E.; FURLONG, M.J. Ecological pest control fortifies agricultural growth in Asia-Pacific economies. *Nature Ecology & Evolution*, London, v. 4, n. 11, p. 1522-1530, 2020. Available from: <https://doi.org/10.1038/s41559-020-01294-y>.