

Editorial fairness: speed, accuracy and awareness benefit authors

James F.A. Traniello¹ and Theo C.M. Bakker²

¹ Department of Biology, Boston University, Boston, MA 02215, USA

² Institut für Evolutionsbiologie und Ökologie, Universität Bonn, An der Immenburg 1, 53121 Bonn, Germany

Following the scholarship-monitoring metaphor of Hojat *et al.* [1], Arnqvist [2] presents journal editors as ‘gatekeepers of science’ and critically evaluates criteria for manuscript rejection without peer review, which he claims unfairly emphasizes novelty in the content of submitted manuscripts. As Joint Chief Editors of *Behavioral Ecology and Sociobiology*, we respectfully disagree with Arnqvist’s characterization. Although we fully appreciate the overarching need to ensure transparency, accuracy, and fairness in the editorial process, we wonder if he has unnecessarily painted editors in language suggesting their ‘gatekeeping’ hinges on subjective assessments ‘destructive to the scientific endeavor’.

Arnqvist lists two criteria for manuscript evaluation by editors: journal appropriateness and novelty. The former is indeed among our most important standards. However, we feel that novelty is not suitably defined, and without at least a provisional definition it is difficult to understand the relationship between using novelty as a metric of quality and engaging in editorial actions that ‘promote poor scientific practice’. We operationally define novelty to accentuate substantial conceptual advancement, independent of model-system taxon, through the generation and testing of hypotheses concerning major theories, creating results of interest and importance to a broad readership. We contend that editors and editorial boards have the perspective, insight, and prudence to judge whether a contribution is ‘substantial’ with respect to these criteria.

Manuscript rejections based only on editorial review, in our experience, are most often made because an author simply chose the wrong journal for submission, independent of whether the results are novel. For example, we often receive manuscripts that confirm the results of published original analyses, now presented with a different species. The content of these manuscripts is certainly novel in that wholly equivalent published studies are lacking, but the results largely support what has already been demonstrated or are taxon specific and better suited for a specialty journal. Other submissions contain only descriptive findings that may be interesting but are too narrow in focus. This is not to depreciate the importance of natural-history studies or descriptive research, but this

type of work rarely constitutes subject matter best published in our journal. We feel confident we can recognize exceptions. If there is a question of significance, we consult experts on our editorial board and request an opinion in a non-prejudicial manner. Decisions to reject a manuscript without review are not made arbitrarily, carelessly, for expedience, to lower the reviewing burden of editorial board members, or to elevate journal status, but to maintain standards for publication quality that satisfy the interests of a broad readership and facilitate advancement in our field.

Efficient manuscript processing ranks high in journal management. A second important goal of rejection without review is therefore to return a manuscript as quickly as possible to the authors, because speedy turnover allows submission to an alternative journal with minimal delay. A rapid editorial decision is a real benefit in having one’s work ultimately appear in print or online in a timely fashion, because weeks or months can be absorbed by peer review that may very likely result in rejection. Such unfortunate delays should and can be avoided.

Given that significantly more manuscripts are received than can be accepted, editorial decisions must be selective, typically due to resource limitations, although online journals may be more accommodating. However, we apply the same evaluation criteria independent of the number of manuscripts we receive, providing that effective editorial review is not compromised. If a large number of excellent papers were recommended for acceptance, we would petition the publisher for additional print pages. Our primary editorial role is to publish manuscripts that represent significant advances of general interest in the journal’s disciplines. A manuscript may present novel results and be interesting and scientifically sound, but nevertheless lack breadth of appeal and significance. A decision to reject a paper without further evaluation is made because we assume that an associate editor and selected referees – whose proven expertise and insight we value and rely on – are unlikely to render a favorable outcome. The system may not be infallible, but it is certainly not rigged.

Some prestigious open-access journals follow an editorial policy counter to what Arnqvist suggests in his critique. *PLoS One*, for example, will ‘rigorously peer-review submissions and publish all papers that are judged to be technically sound. Judgments about the importance of any particular paper are then made after publication by the readership’ (<http://www.plosone.org/static/information>). *SpringerPlus* has a similar policy (<http://www.springerplus.com/about>). These journals offer authors a clear alternative to traditional editorial review.

Corresponding authors: Traniello, J.F.A. (jft@bu.edu).

0169-5347/\$ – see front matter

© 2013 Elsevier Ltd. All rights reserved. <http://dx.doi.org/10.1016/j.tree.2013.10.011>



We were pleasantly amused by the title of Arnqvist's article, which caused one of us (J.F.A.T.) to wax nostalgic about graduate school at Harvard in the late 1970s and recall a Yiddishism applicable here to our discussion of editorial evaluation. At that time, announcements for seminars in Richard Lewontin's laboratory were printed with a graphic of an obviously stressed and profusely sweating caricature of a scientist, who looked like he was stepping out of the pages of a Robert Crumb comic, carrying a briefcase on which 'Schmarvard' was boldly written. It poked good fun at laboring to present one's work in what was cartooned as a hypercritical and anxiety-provoking environment. We hope authors will not be unduly stressed in identifying the right journal to present their research, but will nevertheless be diligent in reviewing journal

standards and contents to determine appropriateness for submission. Authors should not feel discouraged about submitting their work to a highly ranked primary journal, but should rather acknowledge their responsibility to be mindful of the manuscript content required for different venues in making such decisions. If in doubt, email enables rapid communication with editors to offer an opinion. And authors should of course be aware they have options in these transitional times of science publishing.

References

- Hojat, M. *et al.* (2003) Impartial judgment by the "gatekeepers" of science: fallibility and accountability in the peer review process. *Adv. Health Sci. Educ.* 8, 75–96
- Arnqvist, G. (2013) Editorial rejects? Novelty, schnovelty! *Trends Ecol. Evol.* 28, 448–449

Ten years lost at sea: response to Manel and Holderegger

Francine Kershaw¹ and Howard C. Rosenbaum^{1,2,3}

¹ Department of Ecology, Evolution, and Environmental Biology, Columbia University, 1200 Amsterdam Avenue, New York, NY 10027, USA

² Ocean Giants Program, Wildlife Conservation Society, 2300 Southern Boulevard, Bronx, NY 10460, USA

³ Sackler Institute for Comparative Genomics, American Museum of Natural History, 79th Street and Central Park West, New York, NY 10024, USA

Manel and Holderegger [1] (hereafter MH) present a review of the methodological and conceptual advances that have occurred in the field of landscape genetics during the 10 years since the publication of Manel *et al.*'s [2] seminal review. MH proceed to discuss the future of the discipline, focusing on potential applications in terms of adaptation to global change and the conservation of natural resources. MH dedicate a section of their review to the progress made in the subfield of 'seascape genetics' [3]. However, MH refer solely to an approach in which genetic data can be combined with biophysical oceanographic models to investigate environmental influences on larval dispersal. The larval dispersal study by Selkoe *et al.* [4] is presented as a representative example of the research being conducted to understand the influence of ocean currents on patterns of gene flow. Although we agree that important progress has been made in the study of environmental influences on the genetic patterns of the larval stages of marine species (e.g., [5–7]), we wish to note that significant advances in understanding the seascape genetics of highly migratory species, such as cetaceans, were entirely omitted from the review [8–12].

MH also recommend that the field of seascape genetics would benefit from larger-scale sampling to capture wide-

ranging patterns of connectivity and from also being more aware of progress being made in terrestrial landscape genetics, because the methodological and statistical concepts are broadly the same [1]. In elucidating how oceanographic features have influenced the genetic structure of cetacean species and their populations, the studies we wish to highlight have been conducted at broad spatial scales, and have used and adapted many of the techniques employed by terrestrial landscape geneticists; thus, researchers working in the field of seascape genetics are already addressing the issues considered pertinent by MH.

Prior to the birth of 'landscape genetics', Fullard *et al.* [8] presented correlations for the long-finned pilot whale (*Globicephala melas*), which suggested population isolation occurs between areas of the ocean that differ in sea surface temperature. Building on this concept, Mendez *et al.* [9] found significant correlations between genetic discontinuities of franciscana dolphin (*Pontoporia blainvillei*), off the coast of Argentina, and more complex oceanographic climatologies; Mendez *et al.* [9] described this isolating mechanism as one of 'isolation by environmental distance (IBED)'. A comparative study on the humpback dolphin (*Sousa* spp.) in the Western Indian Ocean proved that the principles of IBED could be generalized to other cetacean species and geographic areas [10]. Seascape genetic studies have also been undertaken to investigate the environmental factors shaping the distribution of intraspecific genetic diversity. In an impressive global study, Amaral *et al.* [11] demonstrate how marine productivity and sea surface temperature are correlated with the genetic structure of the

Corresponding author: Kershaw, F. (fk2107@columbia.edu).

0169-5347/\$ – see front matter

© 2013 Elsevier Ltd. All rights reserved. <http://dx.doi.org/10.1016/j.tree.2013.12.006>

