

# Coping with the editorial process: considerations for early-career biologists

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## Abstract

*Coping with the editorial process: considerations for early-career biologists.*— In this essay I describe aspects of the interactions between authors, reviewers and editors with the goal of helping early-career biologists navigate the publication process. Multiple authors and editors have commented on the current difficulties of obtaining quality referees for manuscript reviews, and as a consequence, the frequencies of rejections based on uninformed or erroneous reviews, may be increasing. I suggest a variety of strategies for dealing with: 1) manuscript rejections by editors without review, 2) editors who report but do not comment on reviewer comments, 3) reviews that appear to be uninformed or idiosyncratic, and 4) comments suggesting stylistic revisions rather than substantive ones. The key to any successful strategy for dealing with editors and referees involves ensuring the interaction remains civil and retains a high level of objectivity regarding criticism. In addition, the specific strategies that an author uses to respond to stylistic and substantive editorial comments will depend on their experience and perhaps, reputation in the field. The techniques suggested herein should serve to stimulate discussion of some problems in our field and also increase the probability of acceptance of a worthy manuscript submitted for publication.

Key words: Scientific publication, Publication process, Reviewing, Editorial, Editors, Manuscript rejection, Manuscript revision

## Resumen

*Lidiar con el proceso editorial: consideraciones para biólogos en la fase inicial de su carrera.*— En el presente ensayo se describen algunos aspectos de la interacción entre los autores, los revisores y los editores, con el objetivo de ayudar a los biólogos en la fase inicial de su carrera a navegar por el proceso de publicación. Múltiples autores y editores han señalado las dificultades actuales para encontrar revisores de calidad que revisen los manuscritos y, como consecuencia, es posible que esté aumentando la frecuencia de rechazos debido a revisiones sin conocimiento de causa y erróneas. Se sugieren varias estrategias para lidiar con: 1) el rechazo de manuscritos por los editores sin una revisión; 2) los editores que informan de los comentarios de los revisores, pero no formulan observaciones al respecto; 3) las revisiones que parecen infundadas o idiosincrásicas; y 4) los comentarios que sugieren cambios de estilo en lugar de cambios sustantivos. La clave de toda buena estrategia para tratar con los editores y los revisores consiste en asegurarse de que la interacción sea cordial y mantenga un alto grado de objetividad respecto de las críticas. Además, las estrategias concretas que un autor utiliza para responder a los comentarios estilísticos y sustantivos de los editores dependerán de su experiencia y, tal vez, de su reputación en el sector. Las técnicas que aquí se sugieren deberían servir para estimular el debate sobre algunos problemas de nuestro sector y aumentar la probabilidad de que se acepte un buen manuscrito sometido para su publicación.

Palabras clave: Publicación científica, Proceso de publicación, Revisión, Edición, Editores, Rechazo de manuscritos, Revisión de manuscritos

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## Introduction

All research scientists undergo the familiar, and occasionally tedious, experience of addressing reviewers' and editors' comments prior to acceptance of a manuscript for publication. Despite the great importance of interactions among authors, reviewers and editors (I will use the terms referee and reviewer interchangeably), there are few journal articles that describe strategies for addressing substantive and stylistic comments arising during the reviewing process (but see DeVries et al., 2009; Grod et al., 2010; Schramm & Miranda, 2012; Jennings et al., 2012; Fox et al., 2016), although some information also may be found in the blogosphere (see posts by Heard, 2016a; Duffy, 2015; and Fox, 2015), as well as the books on scientific publication by Heard (2016b) and Benson & Silver (2013). A strategic approach to addressing editorial comments may be particularly important in cases where reviewers or editors have made errors or appear to have a lack of expertise or interest in the manuscript. In this paper I describe strategies for dealing with these situations constructively, based on my own publication experience (author or coauthor on 120+ papers appearing in 35+ journals, editor or editorial board member for five scientific journals). Although my unit of study is small and unreplicated ( $N = 1$ ), I propose strategies with the particular hope that they will be of use to researchers in the early stages of their careers. Not surprisingly, my approach likely is individualistic, however, it has been quite productive for me and my students. In addition, for two reasons I have included several examples where my responses are harsher and less appropriate tactically, than those I would recommend for early-career scientists. First, these examples demonstrate that in some cases different tactics may be successful for early and late-career scientists, and second, because story-telling and personal insights are important and uncommon modes of communicating scientific information (Gallagher & Maher, 2004; Grobstein, 2005). If I had shaped this essay to portray dealing with problematic aspects of the editorial process as an emotion-free, robotic process, it would be deceitful and an insult to scientific authors, especially students. Consequently, in keeping with the efficacy of 'story' as a mode of communication, I have described the techniques that I used to deal successfully with the editorial process, warts and all. Perhaps, this is yet one more case of a late-career person telling an early-career person 'do as I say, not as I do', but it also acknowledges the humanity of the scientific endeavor and makes the reading more interesting.

Undoubtedly, the most important point that an author needs to recognize while engaged in the editorial process, is that editors and referees are overworked and underpaid (if paid at all), and should be treated with respect and courtesy. These individuals provide altruistic services for improvement of our field, at personal cost to their own research programs and advancement. Nonetheless, being an editor is not all self-sacrifice; it also is a well-recognized and important career achievement that increases recognition

and aids in promotion. Being an editor accrues the additional benefit of being on the decision-making side of the publication process, which provides excellent training for helping pass your own work through the publication ringer. Similarly, serving on a grant review panel also serves as great training for getting your own proposals funded.

Despite the rewards of reviewing manuscripts (as with reviewer and referee, I will use paper and manuscript interchangeably), it has become sufficiently difficult for editors to find qualified and thorough reviewers in ecology, evolution, and conservation science that a 'reviewer crises' has been declared (Hochberg et al., 2009; McPeck et al., 2009; Grossman, 2014). This crisis has precipitated several effects that negatively impact the reviewing process. For example, it is difficult to find sufficient reviewers that are experts in every subfield within our discipline; the result is that a behaviorist might end up reviewing population dynamics papers and a habitat specialist reviewing dietary papers. Although this may improve the paper by requesting revisions that will increase readability for a more general audience; there may be negative consequences as well. For example, the reviewers may not be familiar with specialized analytical approaches used in the subfield, or with a specific literature. Unfortunately, this may result in rejection of a paper because of reviewer ignorance rather than quality deficits. The likelihood of such errors increases when speedy reviews are requested by journals in an effort to minimize the time between submission and publication; and speedy editorial decisions have become an advertising point for many journals. If a fast review is requested, say within three weeks, the unfamiliar reviewer may not have time to gain sufficient background to provide an informed review given their everyday professional responsibilities. In addition, mismatches between reviewers and manuscripts are likely to become more frequent as knowledge and specialization expand exponentially in our field.

The paucity of reviewers also means that both referees and editors of general journals in broad fields commonly must evaluate papers outside of their own areas of research, which may lead to an increase in errors and misunderstandings. In this situation, editors may serve more as conduits that summarize and perhaps overemphasize the reviewers' assertions, rather than function as evaluators of reviewer's remarks. Certainly, it is now common to receive editorial decision letters that contain little or no guidance regarding which reviewer comments must be addressed and which are matters of opinion. Even in the past, some editors failed to provide this information, which is unfortunate, because a lack of guidance during the revision process may leave an author stranded in a sea of misunderstandings or errors. If my own personal experience is an indicator, it is likely that the reviewer crises has led to a decrease in the quality of reviews, with a concomitant increase in idiosyncratic comments representing reviewer's personal preferences rather than the consensus of professional opinion. I view the lack of 'comment filtering' by editors as a major problem in the current

reviewing system and have addressed it elsewhere (Grossman, 2014); nonetheless, in this essay I will propose strategies for dealing with a lack of guidance from editors, as well as a variety of other issues that commonly arise in the publication process.

### Situations and strategies

As with any strategy discussed in this essay, courtesy and objectivity are the keys to positive outcomes, as is an appropriate response strategy. Although the ideal situation is for everyone to be treated exactly the same, human nature being what it is, it is likely that a fresh PhD submitting their first paper will be more successful with a circumspect strategy than a past-President of the Ecological Society of America (I discuss some of these differences later in this paper). It also is not surprising that researchers with extensive and strong publication records likely possess greater credibility 'when dealing with matters of professional opinion' than less experienced researchers, because they have a proven history of publication and research experience.

The first evaluation that a scientist's manuscript encounters is when the editor decides to either send the paper through the review process or reject it without sending it through the journal's review process. If the decision is the latter, my experience indicates there is little that can be done strategically, although if the rejection seems excessively arbitrary, it may be worthwhile to enquire politely regarding its grounds. Rejection without review is particularly common in the most prestigious journals such as the *American Naturalist*, *Ecological Society of American Journals*, *British Ecological Society Journals* and *Society for the Study of Evolution Journal*, all of whom have very high submission rates. But even prominent journals focusing on specific taxonomic groups (e.g., mammalogy, ichthyology, etc.) now employ this approach; concomitant with the increased number of researchers in the scientific community and its consequent, the increased number of manuscripts submitted annually. Clearly, for some journals pre-review rejections are necessary to maintain a reasonable work-flow, and may even be helpful from the standpoint that the author does not have to wait months to receive a decision. Personally, I try to submit papers to journals that have published conceptually similar works, but only if my manuscript has unique aspects not addressed by the previous publication(s). This increases the probability that a paper will be sent out for review, but also may serve as the basis for an inquiry if your paper is rejected out of hand. Nonetheless, be prepared to hear 'we've already published sufficient papers on this topic'.

I have rarely been successful in getting a manuscript reconsidered when it was initially rejected without review by an editor. The one success involved a case in a well-known monographs journal where a page limit had recently been enacted but this information was not in the Notice to Authors for the journal. When I enquired as to the rejection the editor admitted that I was over the page limit and informed me that he

would consider a resubmission pared to meet the new page limitations. So I reduced the manuscript and it was published by the journal.

My first negative experience with reviewers' comments involved one of my dissertation papers on a species of fish, a goby. Members of the Gobiidae typically are abundant, functionally important, components of estuarine fish assemblages worldwide. The manuscript examined the feeding ecology of this estuarine species over slightly more than a year including gut fullness, diet, prey diversity, diel feeding periodicity and size-linked changes in diet. The paper was accepted pending revision by a well-known marine ecology journal and I revised accordingly. There was no guidance from the editor regarding the reviewers' comments; consequently, I made most of the requested revisions and offered detailed explanations for why I had not complied with the few remaining substantive recommendations. Rather than make the decision, the editor sent the paper back to the referees, one of whom was satisfied by my revision and the other was not. That referee insisted that a section of the manuscript on prey size selection in large and small fish (data and discussion) be removed because its contribution to the paper was insubstantial. I then called the editor who said I had to make every change requested by the referee or my paper would be rejected. Being a new PhD looking for a job, I complied, although my lack of experience likely had little impact on the editor's insistence on revision. My paper was published without the prey size data, but given that life had just handed me a bowl of lemons, I decided to make lemonade. I took the excised data and wrote a new paper reframing the research question in terms of foraging theory and the ontogenetic niche. This manuscript subsequently was published in a highly respected ecological journal and according to Google Scholar, has been cited 100 times. Conversely, the marine ecology paper has been cited 56 times; draw your own conclusions from these data. These examples illustrate two key principles of scientific publication. First is the necessity, if the peer-review process is to function in an objective and efficient manner: that editors act as independent judges who assess the validity of a reviewers comments and communicate those assessments to authors rather than just forward them along without evaluation (Grossman, 2014). Second, if you and your experienced colleagues are convinced that a manuscript is important and publishable, as I was with the prey-size data, do not give up after a rejection (or editorial exorcism) or two. In particular, try and decide whether the question and data can be reframed in a manner that may increase its relevance and broaden its scientific appeal before resubmitting.

### A taxonomy of editor's and reviewer's comments

We all deal with editors and reviewers' comments and it is best to remain open-minded and focus on the fact that most reviewers want to improve a manuscript rather than prevent its publication. I have

written previously about the starting point one should use when reviewing a paper (Grossman, 2014); i.e., it is publishable until it accrues a sufficient number of problems that it becomes unpublishable (and see the post by Hendry, 2014), or at least view the manuscript neutrally. Nonetheless, the instructions to reviewers of many journals seem to stress journal characteristics that tip the balance towards rejection such as: necessarily high rejection rates to maintain journal quality and reputation, page limitations, high submissions, and helping raise the journals Impact Factor. I am not arguing that these are unimportant aspects of journal management, but I suspect they result in the rejection of some valuable manuscripts, albeit ones that might require nontrivial revision

### **Taxon one – the editor's decision**

Given that your paper has been sent out for review, you will receive a set of reviews along with a 'decision letter' from the editor. For most journals this is a form letter with blank spaces for insertion of the actual decision, which may be somewhat opaque, especially for highly desirable journals. In the golden days of my youth, the categories typically were fairly straightforward: 1) accept, 2) accept with revision (sometimes this was split into the categories of minor, moderate and major), and 3) reject. Two of the journals that I do editorial work for have a similar, and fairly straightforward rating system with: Accept, Minor Revision, Major Revision, Reject & Resubmit, and Reject. However, a number of journals have more confusing systems. My first inkling that things had changed occurred when a paper I sent to a major ecological/evolution journal was returned with mixed reviews and a decision that the manuscript was not publishable in its current form. I read this as a rejection, but given that I knew the editor I gave him a call. He then informed me that if I thought I could address the criticisms I should revise the paper and send it back. The paper was subsequently accepted without further review. So the lessons here are that anything that is not a complete reject has the possibility of eventual acceptance and when in doubt, politely contact the editor who handled the manuscript. Nonetheless, terms such as 'revision requested', 'contingent accept' and 'reject with invitation to resubmit' may be confusing and vary from journal to journal. My advice on uncertainty in the decision is similar to the previous comment on rejection; any verdict that does not render the paper acceptable without a qualifier should be interpreted that the possibility of rejection still exists.

### **Taxon two – constructive reviewer comments**

Let us assume that you have received a verdict that your paper requires revision before it can be accepted, and let us also assume that you have received little or no guidance from the editor on which comments must be addressed and which are optional. There really is no reason to extensively discuss constructive comments you agree with, other than to merely write

something like 'good idea... changes made'. Nonetheless, it always smooths the road to let the editor know that you have considered all comments carefully, by providing positive feedback on reviews and comments. If a referee has made a cogent comment that improves the manuscript, let the editor and the reviewer know by commenting positively in your response. In addition, many mainstream journals copy the referees with the authors' responses and final verdict, and it is a more positive experience when reviewers know their comments were taken seriously, especially if the review took substantial effort to complete.

### **Taxon three – your writing is not clear**

A referee has every right to object to the prose in a manuscript if it is unclear. Nonetheless, they also must fulfill their responsibilities by detailing exactly what was confusing in the prose and more importantly, by providing a short and succinct example of 'how clarity can be improved'. I am not suggesting that the reviewer rewrite entire sections of a manuscript, but a simple comment that 'lines 156–159 are unclear' may yield few insights for an author for whom the passage remains clear. Even proficient writers receive comments regarding a lack of clarity or misunderstood prose, and it is not infrequent that they occur in reviews that appear to be hurriedly completed. This may suggest that the reviewers did not review the manuscript carefully, but also may indicate that a reader who skims the paper also will not grasp the gist of what is written (i.e., it may truly be a place for clarification, although I am not suggesting that we write for readers who skim rather than read carefully). Obviously, comprehension of the written word represents an interaction between the writer and the reader and failures may occur on both ends. However, a substantial number of referees seem to believe that the failure always is on the part of the author, which is unfortunate.

One way to identify problems in your writing prior to submission is to have it reviewed by colleagues, which, early in my career, was a common practice. Unfortunately the greatly increased workloads of most researchers today, have rendered this practice less common, but being a presubmission reviewer is beneficial role for graduate students and post-docs. But what is an author to do when the reviewer has failed to identify the specific items or sections that they perceived as unclear? When I find myself in this situation, I examine the writing in the manuscript thoroughly and clarify everything that might be perceived by another reader as ambiguous or vague. Sometimes, having worked on a piece of research for years, things seem obvious to us when in fact they are not obvious to a new reader. After revising the manuscript, and without making any comments about clarity, I ask my lab members to review the paper. As an early-career scientist, you may ask experienced colleagues, senior graduate students, or committee members to review a manuscript. I then respond to the editor that the text has been reexamined and XX changes made to improve clarity.

#### **Taxon four – I am right, the referee is wrong!**

This is the type of situation where you are tempted to respond with a snide comment, but here also politeness pays off. It also is where you should do as I say and not as I do. In responding to an erroneous comment, it always is wise to take the burden on your own shoulders by beginning your response with something like 'I am confused by the Referee's comment that we used an inappropriate reference...', or 'Perhaps my writing was unclear...' and then go on to make your point. I do not recommend that early-career scientists let their frustrations show regarding errors in reviews, but I suppose that is a personal decision. In general, I do not think that this is good practice, unless you are willing to have a paper sent back again for review and revision. Remember, your goal is to get your paper accepted and not to engage in verbal or written fencing.

Consequently, here is the comment and my rebuttal although I have corrected the reviewer's grammar to a point where it is now comprehensible. Referee's comment: References need some additions and especially lack references to Jonsson & Jonsson Ecol of ... brown trout – springer 2011, but there are a few other novel references missing. On line 34 Quinn ref is fine – but Jonsson would be more correct – Tom's (Quinn) trout data are for steelhead alone, and my response 'Despite the fact that the referee is wrong regarding the Quinn reference, which contains information on multiple trout species including brown trout, we have added the Jonsson reference.' (the subject of the manuscript was brown trout). The difference of opinion on whether or not a reference should be included or excluded is a minor matter and it almost always is good strategy to comply. Complying with relatively insignificant editorial matter demonstrates to the editor that you are: 1) not defensive about your manuscript, 2) take the review process seriously, and 3) are not going to make a fuss over trivial issues. Whether or not you should point out that the referee is wrong, as I did, is a matter of strategy, which may differ for early and late-career researchers. If the referee clearly is hostile and had made a number of erroneous comments, then it may be good strategy to point out their multiple errors, but only in a polite and professional manner. Nonetheless it generally is wise to begin such a response with something conciliatory such as 'Perhaps I have misunderstood comment X but it appears that...' Conversely, if the reviewer has been objective and provided a thorough and constructive review: nothing is to be gained by emphasizing a minor error.

Here is another example from a recent paper, and because I had waded through multiple pages of erroneous comments, and am old and cranky at times, my patience with this hostile reviewer (and the editor whom I suspect had not even read the review) had evaporated completely. To use a scientific phrase, I was totally pissed and as one reviewer of the current paper remarked 'Is this an example of what not to do?' So, no I would not recommend that an early-career scientist use this tone, but sometimes written venting

may have mental health benefits (Smyth, 1998) as long as you are willing to accept the consequences (potential rejection).

Referee's comment: 'The authors refer to long-term studies, which is not the case [sic]: although the total time is 20 years, each station is individually followed during 11 and 13 years.', and my admittedly irritated response: 'But the claim that this study is not 'long-term' is ridiculous when it encompasses 20 years of samples which represents 8–10 times the mean generation time of this species. Long-term must be judged against the mean generation time of the species not by any absolute length of time or some personal opinion. In addition, we have published papers in journal A using 'long-term' that had half the data but encompassed 4–6X the mean generation time...'. You should never descend into emotional argumentation when challenging a referee's erroneous assertion: keep the response factual, unemotional, and professional. Dealing with errors in a review also is a case where professional experience likely plays a role in the response strategy. There is no doubt that a researcher with years of experience and an extensive and high-quality publication record, likely can be more aggressive than a graduate student or new faculty member.

#### **Taxon five – you should have conducted analysis X**

Probably the most common problematic referee comment is that a given analysis is inappropriate for the question being addressed and instead you should have used analysis X. Anyone who has taken multiple statistics or data analysis classes knows that for most scientific questions there typically are multiple valid approaches for statistical or data analyses. And that does not begin to address the fundamental philosophical and practical discussions in our field, over the utility and necessity of information-theoretic (AIC, BIC) versus frequentist statistics, although the former approach enables you to conduct multiple-hypothesis testing in a more rigorous and inferentially-valid manner (Burnham & Anderson, 2002; Grossman et al., 2006). I suspect that disagreements over analytical approaches will become more common given the plethora of statistical programs, especially open source programs such as R. Remember, every reviewer will have their own favorite analytical approach but that does not make it right. The first step in responding to such comments, is to take a long hard look at the comments to assess whether the referee has indeed, made a valid suggestion regarding improving the analysis. Obviously there is a tendency in creative work to insist we are right which may blind us to constructive criticism. However, if your analysis was correct, the appropriate response is to factually document the validity of your analysis by citing references and examples where the technique was used in a similar or identical analysis, especially in the same journal. Two examples follow below.

### Example one

Referee's comment: 'how do you introduce non–delayed density dependence in the per capita growth rate model. As such, it appears that you write:  $N(t+1)/N(t) = f(N(t))$  as  $\text{density}(t) = N(t)/\text{wet.area}(t)$  and this statistical model is surprising as you have  $N(t)$  on both sides of the equation.' My reply: This is an equation that has been used in a number of previous studies (Grossman et al., 2006, 2010, 2012, and references therein) including several published previously in journal A. We suspect the referee's confusion comes from not having read these papers which were cited in the Methods for tests of density–dependence. Nonetheless we have clarified this issue by adding the following text. 'Because density at time  $t$  appears in both sides of the regression equation for per capita rate of increase ( $y$ ) vs simple density–dependence ( $X$ ), a finding of significance in such a model only is suggestive of a density–dependent effect (Grossman et al., 2006, 2010, 2012). However, findings of density–dependence in growth data represent strong evidence for density–dependence, especially when combined with similar results from per capita rate of increase data (Grossman et al., 2006, 2010, 2012)...' Your argument also might be strengthened by citing papers other than your own.

### Example two

Here is an example involving a claim that an analysis was not included in the manuscript, when in fact it was. In addition, the potential analysis itself was misunderstood by the reviewer. Referee's comment: to what degree are these autocorrelated – mean ann flow —max ann flow max ann weekly flow— I am asking whether some of the explanatory variables are correlated – eventually you should consider whether the final model then could be simplified by emitting some variables?: 'The same argument of course applies to temp' and my response: For some reason the referee didn't seem to realize that we used PCA to identify correlations among flow (and temperature) variables so that they could be used in analyses as independent variables (i.e., the score of each year on both components one and two). Same comment for temperature data. In addition, with AIC analysis there is no 'final model' there are a series of competing models that are evaluated for their relative explanatory power. There are ample citations in the ms. to support the analysis, and the same techniques have been used in three previous papers published in journal A (Grossman et al., 2010, 2012, 2016). Lines 212–216 (unchanged from the original ms.) state this quite clearly 'In brief, we constructed a set of biologically realistic linear regression models that included one or more of the following predictor variables: 1) population density, 2) age 1 density, 3) adult density, and 4) annual values on principle component one for both temperature and flow data (Grossman et al., 2012). We also constructed a global model including all variables for each analysis'.

These responses illustrate several strategic points when dealing with comments regarding missing or inappropriate analyses. First, quote the original manuscript (with line numbers) if it demonstrates the referee's comment was inaccurate. Second, if the analysis has been acceptable to the journal (or other equivalent journals), in recent articles, then it should be acceptable now given that it has been vetted previously by multiple, and hopefully, independent referees. This point may affect where you submit articles; strategically, it is better to submit a paper to a journal where your analyses will be familiar, rather than to one where they will be considered non-standard. When the referee is wrong, be polite and meticulous in how you demonstrate that errors have been made. Use quotations and do not paraphrase, make your case strong and unambiguous, but be polite and professional.

### **Taxon six – the rigid referee**

There is not much you can do to deal with this type of referee and I include this example only so that readers will know that it does occur and perhaps is getting more common as reviewers try to clear their desks quickly. In addition, my previous discussion of the referee and editor who insisted that I make every change suggested in the editorial process (goby dietary paper example) is a good example of the 'rigid referee' taxon, nonetheless, here is another real world example. Reviewers comment: 'The manuscript has failed to comply with the manuscript submission guidelines to such an extent that it is my opinion that it should be rejected and resubmitted once extensive style and formatting revisions have been made.', and my reply: 'It would have been nice to know what these major format failures were and certainly would have saved myself and the referee time if they had just reviewed the content of the ms. which clearly was unaffected by formatting. We did forget to put in line numbering but that would hardly seem to warrant rejection. We have made a few minor formatting revisions to the ms.' Fortunately the editor was proactive and had already sent the paper out for an additional review. Consequently, the decision on the manuscript was based on two thorough and one incomplete review. If the editor has not been proactive and rejects a paper based on minor errors in manuscript formatting then it likely is time to find another journal whose policies are less rigid. Getting to know editors and journal policies may be a way of preventing such a problem and many colleagues are willing to share their experiences regarding editors and journals.

### **Miscellanea**

One thing to remember regarding interactions with editors is that they do have to power to make or break your paper. When I first became a sectional editor for a journal, back in the last century, I went to one of my mentors who had extensive editorial

experience and asked him for advice. He gave me many valuable tips, but one thing he also mentioned was that some editors had a 'kill list' a list populated with reviewers that unfailingly rejected papers regardless of their quality. Having been an editor or editorial board member for five different international journals myself, I have to admit that I occasionally encountered reviewers that displayed such behavior (they were off my list after two reviews and their reviews were not considered in decisions). Some editors deemed a kill list necessary, because they had repeatedly dealt with authors who refused to revise their manuscripts despite reasonable referees' comments and basically kept resubmitting the original manuscript (another behavior I have observed). The kill list was a time-efficient way of dealing with these authors; however by being open to constructive criticism and addressing reviewer's comments in a non-defensive manner you can ensure that your papers are sent to thorough and constructive referees. Editors may or may not have such lists, or use different euphemisms, but their potential existence is one more reason to stay on an editor's good side.

Scientific research is a consuming yet joyous task filled with discovery. Nonetheless, science cannot progress unless research results are communicated to other researchers, practitioners, and the general public. Publication in peer-reviewed scientific journals is the most common medium of communication among colleagues and practitioners, although the peer-review process is not error free. I have described a variety of potentially successful response strategies for referees' comments and editor's decisions via discussions of my own publication history. By necessity these suggestions are personal and perhaps individualistic, given they are based on the one senior researcher. However, the apparent lack of published information on the intricacies of the publication process in ecology and evolution substantially warrant discussion of these issues, especially for early-career researchers. Describing editorial issues in science via story and experience render the process more humane and hopefully elicit the recognition that all scientists deal with common issues.

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