



Promoting the Science of Ecology

How to Write an Influential Review

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Some Sources of Information

NSF publications can be obtained by writing either to the relevant program or to the Forms and Publications Unit, Room 232, National Science Foundation, 1800 G Street N.W., Washington, DC 20550. BSR has the same address; its telephone number is (202)357-7332. The following are especially useful: NSF 86-20. A one-page description of the activities of the Division of Biotic Systems and

Resources and its programs, with relevant telephone numbers.

NSF 87-40. Guide to Programs; Fiscal Year 1988. This is the most useful single source of information on support available throughout the entire Foundation.

NSF 83-57. Grants for Research and Education in Science and Engineering. Contains everything you need to know about preparing a proposal.

HOW TO WRITE AN INFLUENTIAL REVIEW

It is not the critic who counts. The credit belongs to the (one) who is actually in the arena, whose face is marred by dust and sweat and blood; who strives valiantly; who errs, and comes short again and again; who knows the great enthusiasms, the great devotions; who at the best knows in the end the triumph of high achievement, and who at the worst, at least fails while daring greatly.—Theodore Roosevelt

Often we are asked for our anonymous written opinion of a colleague's research proposal or manuscript. Most of us respond whenever we can, because it is one of the most important ways that we contribute to the development of our science. Such peer review is the primary mechanism by which we, the community of scientists, affect the distribution of limited grant funds and journal pages. Our reviews have enormous influence on the direction and rate of progress in our discipline.

It is not much help if we use this influence unwisely, or our review is not taken seriously. How should a review be fashioned so that it is taken at face value and not discounted? What sort of influence should it try to exert?

Surely a review should seek to identify and encourage the most promising and innovative research. Yet it is precisely the newest ideas that are the least tested, the most controversial, and easiest to criticize. In fact, history reveals that new theories often are incomplete and often do contain serious errors. But wouldn't it have been a tragedy if Natural Se-

lection had been rejected because Darwin founded it upon incorrect mechanisms of inheritance?

If we are to advance our science, it is necessary that we take risks and actively encourage the development of new concepts, theories, and methods. If we, as reviewers, are afraid of mistakes, and insist that our peers write airtight proposals, then who will dare to tackle the difficult questions? If we require that a proposal be so well described that we can visualize every aspect of the research, is the work really likely to produce any surprises or major new discoveries? We have to be honest and point out potential problems, but, above all, we must strive to identify and express our enthusiasm for new ideas and innovative approaches.

How do we write a review that will convince panel members and program directors that a proposal, for all its rough edges, is an eminently worthwhile risk, an exciting adventure with a high payoff if it succeeds? The Research Support Liaison Committee in Ecology, Evolution and Systematics took this question to the staff members of NSF's Division of Biotic Systems and Resources. The way we phrased it reflects a nagging perception that we shared with them. With funds scarce relative to need and the average quality of proposals very high, is there a tendency of reviewers, panel members, and program directors to be conservative, avoid risks and look for grounds to criticize a proposal? Is any fault or loose end likely to prevent funding? Must we avoid negative comments in our

reviews if we feel a proposal should be funded?

The answers from BSR scientists were unanimous. Furthermore, the same questions could be addressed to a journal editor, and the answers would be similar. So the Research Support Committee felt they should be shared.

The Correlation Between Detail and Negativity in Reviews

The more negative the tenor of a review, the more detail it has. This is a fact. We know no reason why it has to be so, but it is. Negative reviews are often full of well-reasoned objections. Positive reviews are more often brief statements of approval. They may be full of glowing adjectives, but they rarely contain the details and logical arguments that would give them substance.

Faced with positive reviews lacking substance and with well-documented negative criticisms, panels are often swayed by the negative comments. What else can we expect? If NSF is to discount negative comments about promising but risky and even somewhat flawed proposals, it must have good reasons for doing so.

Accentuate the Positive

We all know what a long, negative review looks like. It is time to produce some long, positive ones. When you encounter a good proposal (or MS.), spend your time on that one. Describe in detail what makes it good. Remember that NSF panels and ad hoc reviewers are drawn from a wide spectrum of our peers. Just because we recognize the merits of a proposal does not mean that other reviewers will appreciate them. State why the problem is important, what contribution the proposed research will make, and why the investigators are well qualified to do the work. Avoid hyperbole and be specific. Point out weaknesses, too, and explain why they have not put you off. Give the program director (or editor) a good excuse to follow your advice.

Many reviewers seem to think it is their primary responsibility to discover and call attention to all the flaws in a proposal or manuscript. Perhaps they have the attitude that it should be deemed worthy until proved otherwise. This may sound fair, but it is, in truth, pernicious. It makes us into petty bookkeep-

ers, subtracting the value of each counterfeit penny without noticing that they are coming from a solid gold box. If we accept that even the best science—and especially science that has not yet been done—must be imperfect, then we should be able to resist the temptation to dwell on the negative aspects. If we adopt the attitude that a proposal does not deserve funding unless the research is daring, novel, or interesting, then we should place more emphasis on the positive aspects of a good proposal and write longer, more constructive positive reviews.

We need to remember that it is much more damaging to our discipline to suppress an important contribution than to fund or publish a questionable piece of research. New ideas and conflicting data cannot have any influence unless they are developed, whereas serious errors will usually be detected and corrected, either by the investigator before publication or by the scientific community soon afterward. This is why it is essential to be broad minded, and to consider the potential importance of a piece of research as well as to search for flaws.

Regulate the Proportion of Positive Comments

Do not undermine what is intended to be a positive review by devoting more attention to criticism than to supporting comments. The overall impression your review makes depends upon the proportion of criticisms that are positive. You cannot counter that fact with an introductory or concluding paragraph. If there are three pages of negatives, and the first sentence says "This is a manifestly important proposal and should be fully funded," what do you think the panelists notice? Their overall negative impression cannot be reversed. Believe it. If you really think the proposal is that good, fill the review with your reasons and mention the negatives briefly.

The score assigned by reviewers to NSF proposals is also very influential. As much as panel members and program officers try to read the reviews carefully, it is a fact that proposals that receive low scores or average less than 2 (very good) have a difficult time. Scores should reflect the priority that we place on supporting a particular program of research, not the number of nits that can be picked from the way the proposal is written.

In writing a review, we would all do well to bear in mind the difficulties we would have in trying to write a proposal to support the most exciting of our own research.

If you feel compelled to note every flaw, send the details directly to the author, and point out in your review that you have done so. The panel will then be even more convinced of your positive opinion because they see you are taking so much time to help. For example, a prescient reviewer of Darwin might have written, "I have my doubts as to the validity of Mr. Darwin's ideas on the process of heredity, and I have written to him in detail about these doubts. But even if I am correct, Mr. Darwin's proposal to visit the Galapagos Islands remains extremely valuable." And just to be sure that you realize Darwin is hardly the only example, consider whether it would have been wise to discourage R. A. Fisher's researches because his models lacked the component of genetic drift. Would you have suppressed Eldredge and Gould's work on punctuated equilibria because they first insisted that punctuation *had* to be connected to speciation? Would you have retired R. H. MacArthur as soon as he made the illogical jump from resource-use overlaps to competitive alphas?

Negative Reviews

Simultaneously, we should learn to write shorter negative reviews. Simply admit that you found nothing particularly exciting or novel. It is very important, however, to write such reviews, thus calling attention to proposals that are solid but unexciting and unlikely to result in significant advances. If other reviewers have expressed similar concerns, the panel will have little difficulty making an evaluation. The panel needs help ensuring that critically short funds are not wasted on pedestrian projects.

The above is not intended to suggest that we should endeavor to be less critical. Serious criticisms and substantial concerns should always be expressed, but this can be done in dispassionate language without indulging in ad hominem assaults.

Critics of art and literature, whose criticisms are often published and who earn their keep from them, feel they must entertain their readers with a rapier wit, caustic comments, piercing put-downs, and acid cuts. Many appear

to have decided that criticism is a written version of prizefighting except that in boxing, low blows are against the rules. Leon Wieseltier calls it "aggression as an intellectual instrument."¹

Unfortunately, all too many negative scientific reviews seem to have been written by put-down artists. This is not only cruel and cowardly (at least the literary critic signs his piece), but it minimizes their influence as well. The editor, panelist, or program director is driven to sympathize with the victim. This may mean that if you and you alone noticed the flaws, but reported them intemperately, your criticism will be ignored. Moreover, such aggressive attacks leave a lasting impression of unprofessionalism on your part.

The Payoff to Our Whole Science

Basic science in America is often under attack as a social luxury, and an expensive one at that. Usually the attacks are oblique. Politicians ask what direct, immediate benefits to expect from our work, or give golden fleece awards to projects whose titles make them easy targets.

Dr. Janet V. Dorigan of the Department of Energy has observed that when scientists are under attack, they circle round, wagon-train style. The physicists aim outward at their opponents. Biologists, on the other hand, aim inward, at each other. Their weapons, of course, are disparaging reviews and negative comments.

The earth pulses with fascinating ecological and evolutionary questions, and threatens with environmental concerns. The questions are as intellectually challenging as those facing any other scientific discipline. The answers are essential to deal with the environmental problems that beset the world. But we cannot convince other scientists (or the public, or government officials) of the importance of our work if we seem to be calling each other incompetent. Why should anyone want to invest in a bunch of incompetents?

In order to convey a more accurate impression of the value of our collective labors, we all need to make a conscious effort to tolerate diverse ideas and unconventional approaches, and to promote independence and originality. Robert Reich has written that "Technological innovation is largely a process of imagining radical alternatives to what is cur-

rently accepted." Thus it can thrive only if dissent is tolerated.² In our reviews, we must encourage that dissent and emphasize the advances it will make possible.

A famous cartoon character (Pogo) once said, "We have met the enemy and he is us." This may accurately characterize many human activities, such as ethnic and religious prejudice, war, and degradation of the environment. But it cannot be permitted to be true of the scientific enterprise, which is in its very essence mutualistic and collaborative. The peer review system has obvious limitations and imperfections, but it is the best means that scientists have devised to evaluate each others' work. It is up to us, the community of ecologists, evolutionists, and systematists, to use the peer review system carefully and wisely. Only then can it serve the goal we all share: the rapid advancement of our discipline.

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¹ *The New Republic*, 16 XII 1987:42. But notice that even professional critics often make fools of themselves with negative reviews: Henderson, Bill (1986, 1987) *Rotten Reviews I & II*. Pushcart Press, New York, New York, USA.

² *The New Republic*, 3 VIII 1987:32.

DIFFERENCES IN THE PROPORTION OF WOMEN TO MEN INVITED TO GIVE SEMINARS: IS THE OLD BOY STILL KICKING?

Recently, attention on these pages and elsewhere (Cole and Zuckerman 1987, Loehle 1987, Sih and Nishikawa 1988) has been directed toward the question of the equality, or lack of it, in publication rates for academic women in comparison to their male colleagues. Sih and Nishikawa (1988) found evidence that suggested that statistically significant differences did not exist between men

and women in current (1986) publication rates within the disciplines of ecology and evolution (henceforth E & E).

The ability that women have to publish papers is both a major component and a reflection of women's success in entering these male-dominated fields in large numbers. A somewhat more subtle commentary on the acceptance and importance of E & E scientists