EDITORIAL

More Interesting Experiments, Please!

Reviewers' comments and criticisms about a paper often mask a central issue: How interesting is it? Authors often presume that their results are inherently interesting and speak for themselves. This may be so, if the results are robust and novel, and their implications are obvious and important. Much of the time, however, we need to be told why the work is interesting, or at least why the work was done. Saying "This is the first demonstration of . . ." won't cut it if the reader's response is "So what?".

It's hard to say what's "inherently interesting." A good paper about penguins may tell "more about penguins than I care to know." That's why we sort ourselves into disciplines and areas of interest. Still, reviewers rarely tell authors their paper is "boring" or even "not interesting." Sometimes they call it "interesting" just to be polite or noncommittal. (Mark Rollag has wisely advised me, "Never say 'cockamamie.' Always say 'interesting.'") They usually just ask for additional experiments—"Tell me more"—to make the paper better. I try to attend to whether it's "Tell me more because I'm interested" or "Perhaps if you tell me more I'll become interested"; "Tell me more to prove your point" or "Tell me more so it'll have a point."

What reviewers want to see to make a paper more interesting is customarily expressed in terms of "generally accepted" criteria. These always sound reasonable but, like personality, a paper can be described in positive or negative terms, viz, "fishing expedition" (bad), "hypothesis testing" (good), "empirical" or "phenomenology" (bad), "relevant to the real world" (good), "effects and implications restricted to 'unnatural laboratory conditions'" (bad). Differences between disciplines are apparent in the comments that different reviewers make when they assess a piece of work. Which stock phrases reviewers use is generally a function of where on the reductionist scale they do their thinking and their work. The problem is that one investigator's mechanism is another's phenomenology. One of the nice things about working in an interdisciplinary field like ours is that you get to be criticized from all sides. You may well be asked, "But what is the mechanism?" by one reviewer and "But what is the functional consequence?" by another. In choosing a disciplinary level or deciding what to work on, there is often some trade-off between mechanistic "clarity" and "functional relevance." Your choices of disciplinary level and problems to work on may come down to deciding which level of ambiguity you're comfortable with.

These days, rhetorical emphasis on mechanism and hypothesis testing is much in vogue (though I don't remember the Human Genome Project being scorned as "mere phenomenology.") I would argue, however, that a truly new phenomenon (radioactivity, for example, or the generation of electricity by nerves and muscles) has a better chance of being a lasting contribution than does a new mechanism. Mechanisms are ephemeral; they tend to transmute into markers for phenomena or into the next phenomenon to investigate.

Editors want to publish papers that interest their readers. They must rely, for the most part, on community standards as reflected in the judgments of reviewers. Reviewers should be the people most interested, as well as most knowledgeable, in your area. They may be the only ones who ever actually read your paper. If they're not interested, the rest of the journal's readers won't be. I do think it's important to leave some room for idiosyncrasy and eccentricity, and I believe that elegant work has its own value. Up to a point: "Doing something well is no excuse for doing something that shouldn't have been done at all." Certainly, I've often found myself absorbed by work I had no initial interest in, but I'm not sure whether the hook was in the work alone or in the presentation as well. Maybe I've heard or read work that I should have admired that left me cold. You can present good results in a bad paper. The work is key, but do try to make the paper interesting as well. Fascinating though they may be, it's also best not to assume that anyone actually reads, let alone remembers, your papers—or your editorials.

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Editor

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