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# The Loss of Innovation: Peer Review in Multi- and Interdisciplinary Research

by
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***Abstract***: Multi- and interdisciplinary research, processes of synthesizing new questions and paradigms between two or more fields, are particularly sensitive to inept peer reviewing. This is primarily because: a) such science is difficult to evaluate because it is new; b) evaluation of the results is difficult; c) the grant system favors those who write well; and d) scientists and the public believe that funding should depend upon principle of equal access for equal merit.

In reviewing multidisciplinary projects, reviewers must assess each investigator's skill in his/her particular area, as well as determine that the project head has adequate administrative ability. In reviewing interdisciplinary research, each investigator must be evaluated for secondary competence in the other field(s) as well as in his/her primary field. Moreover, reviewers should also possess appropriate secondary competences. Review of interdisciplinary research must, in addition, take into account methodology of the proposal, clarity of ideas expressed, and closeness of the two (or more) fields. Reviewers of multi- and interdisciplinary research play critical roles in the progress of science and must possess well-educated intuition, flexibility, and sensitivity to their simultaneous responsibilities as guardians of competence and innovation.

## REVIEWS OF PEER REVIEW

In 1986, the American Council of Learned Societies reported on scholars' opinions of computers, libraries, publications — and peer review. One message was unsettling: "Three out of four respondents consider the peer review system for journals in their discipline biased, especially in favor of established scholars. Nearly half say reform is needed" (Morton and Price, 1986, p. 1).

Although peer review may work very well in reality, scholars' beliefs to the contrary are important because credibility is central to the system. Scholars' distrust for colleagues' reviews represents a deep and widespread cynicism that peer review is professionally inept, bureaucratically unfair, and technically incompetent. The suspicion that peer review is not what it should be has arisen more than once in scholarly studies and critiques (Lock, 1986), in personal stories (Szent-Gyorgyi, 1971, 1972), in journalistic commentary (King, 1988; McDonald, 1981; Walsh, 1987), and in scientific humor and parody (e.g., Englebretsen, 1983). Recall Sydney Harris' marvelous cartoon showing a mathematician simply crossing out a colleague's work, while the colleague stands aghast, exclaiming, "That's *it*? That's peer review?" (Bishop, 1984, p. 44). Indeed, humorous and sardonic critiques of peer review — many published in *The Journal of Irreproducible Results*, the great journal of scientific parody — outnumber serious studies of various or alleged flaws in the peer review system.

It is unfortunate for the scholar interested in the intellectual and economic structure of science, and particularly unfortunate for policy makers, that research into peer review is "patchy" and "anecdotal" (Lock, 1986, pp. 96, 100). Indeed, Lock's book is one of the few full-scale treatments, and he found himself depending, for example, upon letters written to *Science* (Lock, 1986, pp. 98-99). Yet that fact itself is informative. To criticize peer review except on the strongest scholarly grounds leaves one open to the charge of sour grapes or even of pork-barrel politicking (Rose, 1986), especially since few scholars are privy to the details of grant peer review (but see Porter and Rossini, 1985). Furthermore, as King's (1988) journalistic commentary on the 1988 National Science Foundation report "Proposal Review at NSF: Perceptions of Principal Investigators" amply illustrates, criticisms of peer review can evoke strongly defensive reactions from program directors and grant officers, some of whom flatly deny any basis to the complaints, e.g., of cronyism. After the Morton and Price critique was published, Morton (1986) wrote, if not a retraction, then a serious modification of the conclusions drawn by Morton and Price (1986).

Since, in this paper, my topic concerns only a portion of problems posed by scholarly peer review, differences between formal statements and commentary (e.g., in the NSF report on proposal review) and informal undercurrents of far deeper dislike and distrust seem to represent a serious difficulty. Whom are we to believe? Are there major problems or not? Lock (1986) argues that extensive research is needed before we can fully understand how scholars react to peer review. In some senses, he is correct if, for example, we wish to know precisely how many scholars in what fields and in what institutions feel what. But do we need "extensive research" before concluding that problems *seem* to exist? I do not think so, nor do I feel that "extensive research" is a prerequisite for suggesting ways to improve reviewing.

Moreover, analyses of topics such as "what really happens during grant peer review" can all too easily become politicized, so that gathering objective data and drawing sensible conclusions become nearly impossible. Partisanship can "prove" anything. Instead, let us agree that the folklore of science and scholarship — the jokes and cartoons, for example — should be taken seriously, especially when they reveal attitudes critical of what is officially denied or downplayed. In this comment, I am modifying Dundes and Pagter's (1975, p. xix) definition of the "folk" (as in the word *folk*lore) to apply to scientists and scholars: individuals who are "… bound together by the mutuality of the unhappy experiences in battling 'the system,' whether that system be the machinery of government or the maze where one works." Scientists and scholars, like all other citizens battling "the system," feel that something is wrong, and express their doubts not through official channels or on questionnaires sent to them by the National Science Foundation, but by the cartoons they tape to their office doors. It would be foolish indeed to ignore the message: something certainly *seems* wrong. Accordingly, there is nothing amiss in suggesting how grant reviewing can be improved, particularly interdisciplinary grant reviewing. So, the purpose of this paper is not an attack on grant review policies, nor a defense of how reviewers do their work. Instead, I accept the idea that problems are perceived to exist, and I therefore examine several types of interdisciplinary work in order to identify unique problems each poses to the conscientious reviewer and suggest ways of alleviating them.

**Some Basic Criticisms of Peer Review**. Lock (1986, pp. 97-98) has provided a valuable list of crucial, and highly emotional, themes that arise when scholars express doubts about peer review. Each points us to areas that need careful thought, particularly when one is reviewing multi- and interdisciplinary work. The list is as follows.

1. Creative science is difficult to evaluate because it is new.
2. The correct evaluation of data as they are produced is very difficult.
3. The system favors those who can write well.
4. New ideas cannot be protected from reviewers. In contrast to ordinary journal publication, an idea in a grant application can easily be "borrowed" without acknowledgement or citation.
5. The only way to write a research grant application is to describe experiments that have already been done.
6. It is difficult to appeal against a negative decision.

Of these themes, the first three — novelty, evaluation, and clarity — seem central. Each evokes powerful emotions, for each centers on a principle dear to our American hearts: that an idea will receive a fair and open hearing, on its own merits, aside from "old-boy" bias and patronage, pork-barreling, and deliberate dismissal of innovation as threatening to the intellectual status quo. Coupled with one's investment in one's own ideas, this ideal makes it easy to see why scientists and scholars are so concerned about the fairness of grant review processes.

But more than personal feelings are involved when, in particular, the reviewing of *grant* applications is criticized. From grants come the financing that scientific and scholarly progress requires: denial of funding chokes off research before it begins. An editorial rejection of an already complete manuscript may be remedied by rewriting or by further research, but rejection of a grant proposal may prevent possibly innovative and potentially significant work from ever being started. Whereas the editor's rejection might be paraphrased as meaning *this isn't good enough for publication yet*, the rejection of a grant application seems to say *this work isn't even worth doing in the first place*. To be sure, some rejections of grant proposals may occur because the proposal does not match the granting agency's mission, but when a proposal is rejected for scientific or scholarly reasons, the idea itself is being rejected, rather than its possibly inept, but remediable, execution. Thus, the rejection of a grant application represents a far deeper criticism of the value of an idea than does the rejection of a manuscript.

Furthermore, there exists a crucial difference between the rejection of a grant proposal and the "rejection" of work already completed. Presumably, a rejected manuscript can ultimately find a home in the literature after revision, reworking, rewriting, and resubmission. Then, once published, it becomes subject to the broad judgement of the scientific and scholarly community as a whole. The results might not be replicable; the reasoning may be subtly faulty; the work may be trivial. Or perhaps not: a published work is, in theory, retrievable by people to whom its ideas and data, even if faulty, incomplete, and non-replicable, may serve as springboards for improved ideas and less faulty data and reasoning. But when a grant application is rejected, the work will in all likelihood never see print because it may never be undertaken. In this situation, the scholarly and scientific judgments that are made by readers of a published paper will not be made: it is as if a few individuals, lucky enough or political enough to become reviewers for the large granting agencies, have substituted their own, possibly narrow and parochial, viewpoints for the consensus of the scientific and scholarly community in general. Clearly, grant reviewing creates ethical and scientific responsibilities far transcending the responsibilities involved in reviewing a completed manuscript for possible publication in a given journal.

These concerns achieve even sharper significance when decisions about grant proposals are made at times of limited funding for all scientific and scholarly work. Then fears of pork-barreling and favoritism become even more acute. In a delightful parable about the social dangers — and advantages! — of recombinant DNA research, Charles Sheffield comments about such things:

> ... For four years Oscar and I had submitted proposals to the National Science Foundation and the National Institutes of Health, and seen our requests refused outright or squeezed down to a hardly-useful pittance. Small private universities, with tiny Biology Departments and no Nobel Laureates, were not the places that the ball of government funding came to rest. Last year we had gone through the usual ritual, with the usual pessimism, only to find that somewhere, far upstream in the government funding process, a mighty dam had broken. Our research was on replacement processes in the replication of DNA, a long way from the RNA retrovirus that causes AIDS. But our principal keywords, Blood and Phage and Transcription, had somehow hurled our proposal into the *thalweg* of AIDS mainstream research. Suddenly we had a million dollar grant, fancy new hardware, and enough soft money for a dozen graduate students.

(Sheffield, 1989, p. 127: quoted by permission of publisher and author. Two typographical errors in the printed version have been changed at the author's request to reflect the original manuscript version. *Thalweg*, according to the Oxford English Dictionary, refers to the line in the bottom of a valley where the slopes of the two sides meet to form a natural watercourse. Mr. Sheffield informs me by letter (2/20/90) that "…today's main use of the word among hydrologists is to refer to the central, deepest and fastest-running channel of a river." Ah, the joys of interdisciplinary correspondence!)

Although the account is fictionalized, many scholars and scientists will instantly recognize Sheffield's dual pillorying of the grant review process: competently designed work is not funded, and irrelevant work is funded through incompetent bureaucratic blunders that throw money at what seem to be fashionable areas of research. It makes no difference that officers of NIH or NSF might deny the possibility of such an error: what counts is the belief that grant reviewing is somehow intrinsically unfair and incompetent, rather than operating under the stated ideal of equal access to funding for equally meritorious ideas.

If we agree that the importance of peer review of grant proposals lies, in part, in its control over access to essential funds, and that it putatively and ideally operates under an ideology of equal access for equal merit, we can see that criticisms of how grants are reviewed are not just minor quibbles, sour-grapes resentment, or politically-motivated attacks on Big Spending Government Agencies. Instead, these criticisms represent the sense — often expressed only in informal ways, through jokes and fictionalized accounts — that something is seriously awry in how funding is allocated by those who control such matters. It is that sense — or belief, if you will — that makes criticisms of peer review so important: what is at stake is not merely dollars, but trust itself. *That* is what makes peer review so crucial.

**The Need for Safeguards**. The clash between these Utopian and democratic ideals of science and the *Realpolitik* of grantsmanship and academic competition reaches a pinnacle when proposed or completed research involves several disciplines at once. Then, Lock's three dilemmas — the protection of novelty, care in evaluation, and clarity in presentation — come to the center in ways that differ from their ways of affecting unidisciplinary work. Multi- and interdisciplinary research in particular need unusual forms of meticulous peer review.

Generally speaking, we may assume that scientific progress comes from two sources. One is detailed work within a field that fills in the cracks of an existing and respected paradigm. This is Kuhn's (1970) classic view of progress in the mature sciences. Although Kuhn also argued that such sciences go through occasional periods of crisis, when poorness of fit between new and anomalous data and the existing paradigm can no longer be ignored, nonetheless, even after a paradigm shift, this form of science depends on gathering data that fit directly into the paradigm, whether it be new or old. Physics after Einstein and after the quantum mechanics revolution remained physics, even if Newtonian formulations were now seen only as approximations.

However, another form of progress also exists. It comes about when two or more hitherto isolated fields reveal commonalities or convergences which themselves become the object of study and, ultimately, provide new paradigms. Examples, discussed further below, include biochemistry, physical chemistry, and social psychology. Casually, we often speak of "cross-fertilization" when two previously distinct disciplines meet and produce new fields of research.

Obviously, inept peer reviewing — which can be defined to include reviewing which rejects novelty when it could be valuable, which evaluates data carelessly and thoughtlessly, or which denies funding merely because the reviewer won't take the time to understand what has been written — is bad enough for the first kind of progress. But, with respect to the second kind, inept reviewing can alter the intellectual structure and future of science itself.

Since that is a strong conclusion, I need to make a distinction. We might readily believe that a particular piece of proposed or completed research could suffer from accidental vagaries of incompetent reviewing, and therefore never see the light of day. But it is harder to see that the march of science *itself* 'could be so vulnerable to a few occurrences of incompetent reviewing. We tend to believe that scientific progress is a collective, not an individual, process. If one worker in the scientific vineyards fails to harvest a certain result, then we believe that another will — and the march of science goes on unimpeded. That march, we feel, is relatively immune to small perturbations represented by incompetent reviewing: sooner or later, we say, the promise offered by allying two fields will show itself, even if occasional individual proposals or papers are lost to unfair reviewing. But such a view underestimates the exquisite sensitivity of interdisciplinary work to small but negative perturbations — such as the rejection of an individual grant or paper — particularly at the onset of such work.

This idea itself has an interdisciplinary origin in what today is called chaos theory (Dewdeney, 1987a; Fisher, 1985; Stewart, 1987). In it, certain physical systems — often complex and given to turbulence, like water flowing in a channel (Fisher, 1984) or the orbits of a planet's satellites (Edelson, 1986) — will follow one path as long as causal parameters stay *precisely* at certain values. However, small fluctuations in these parameters can cause large and abrupt shifts that lead the trajectory in directions that are unpredictable in theory and practice (Dewdeney, 1987b; la Brecque, 1987). In particular, the growth of aggregates of solid material occurs when free-moving molecules attach to already aggregated molecules, to produce meandering fractal shapes whose overall position and orientation depend *crucially* on the position and orientation of the molecules that initiate the process (la Brecque, 1986/1987).

It seems like a useful metaphor for at least some aspects of human history: "for want of a nail, a kingdom was lost," as folklore has it. For example, I speculate that given a proper metric, the growth of the scientific literature will probably follow *exactly* the mathematics of the fractal growth of aggregates just mentioned. However, be that as it may; chaos theory teaches us in general that small perturbations in a dynamically growing, interactive system can have immense effects on the system's entire subsequent history. Although the idea cannot be considered a complete model of the history and growth of science, it nonetheless promises some insights into a troublesome area. In particular, it focuses our attention on the process of creative scientific innovation: what happens when a genuinely new idea is first formulated but has not yet been tested? Since — for whatever reasons — such originality is not spread about in equal proportions among scientists, it seems quite reasonable to propose, chaos theory aside, that the events surrounding innovation can contribute a great deal to ultimate scientific progress.

Thus, I suggest that the danger to science is not either in funding or publishing poor work, but in rejecting important innovations. Although no science can tolerate a preponderance of poor work, all fields can tolerate *some* work which no one can replicate or which proves so idiosyncratic that it cannot be generalized at all. (Courtesy, or perhaps the libel laws, prohibit me from listing examples, but I have encountered such things in my own scientific career.) The machinery of science is fairly tolerant of such grit, and such work dies a natural death. Instead, the problem is losing the great innovations before they can be recognized. Then, to use the chaos theory metaphor, the future path of scientific work grows in a very different direction, as the metaphoric molecules of science — individual scientific papers and research projects — attach to other molecules and meander off with a different orientation. It is, of course, the nature of such loss that no one can tell what it *might* have been like otherwise. The *what if* theme is a staple of science fiction, not of science itself.

Inept peer reviewing is of the greatest danger in precisely this context: a grant rejected means that no one except a few reviewers will ever see the proposal. And if the work is so expensive that it cannot be completed without funding, then no one will ever see the results. The loss may not be great, perhaps, if it merely intended to fill in a few cracks in a well-developed field of science. But what if the work could have been the nucleus for an entirely new direction of growth?

In this situation, a few reviewers have acted as if they represented all of science, and as if their individual judgments spoke for the collective decision of scientists generally. Yet, if the decision of the few was wrong, then no one can later undo the damage if it turns out that the rejected proposal would have represented a major step forward. Thus, peer reviewing can sometimes have immense effects precisely because it controls the twin filters of research, grant approval and publication.

Obviously, the exact degree of risk produced by such failures cannot be assessed. No one can sit down with all the proposals rejected by NSF or NIH and ferret out those which would have been revolutionary. And the reason is the emergent quality of interdisciplinary work: the importance of a piece of research to an allied field can only be assessed retrospectively, after the unexpected finding has been published, slowly recognized as more general than first thought, and placed at the center of the theories and paradigms of a new, interdisciplinary field.

Thus, the story is told of Galileo that because he lacked accurate timepieces, he established the law of falling bodies — equal distances fallen in equal time, regardless of the weight of the object — by using musical tempi to determine how much time elapsed between the object's release and its fall to earth. Drake (1975, p. 100) quotes Galileo as having written that he measured time "… by singing a song while a ball was rolling down a plane, and it proved quite exact" (Galileo was himself an accomplished performer on the lute). Today, however, someone proposing *An Investigation of the Temporal Properties of Solid-State Catalysis* by using a Moog synthesizer as a clock would be laughed out of science (or laughed at as a satirist). We laugh because we understand that the parallel is deliberately anachronistic. Yet we may well ask if equally peculiar-sounding proposals, using equally disparate modern techniques, have been rejected despite the possibility that they might have been very valuable. Perhaps they have. Perhaps, in their understandable desire to prevent crackpots from taking over science, reviewers too quickly reject valid interdisciplinary work.

So, the question becomes whether or not it is possible to guard against the worst cases of lost research, especially interdisciplinary work. To consider the shape of these safeguards, I feel that we should first differentiate between two kinds of research.

## Inter- and Multidisciplinary Research

Although these terms can be used in a variety of ways (Chubin, Rossini, Porter, and Connolly, 1986), it will be useful here to speak of *interdisciplinary* work as occurring when experts from two or more "allied" or potentially "allied" fields work together on a project where they share with each other the insights and theories of their own fields, and to speak of *multidisciplinary* work as occurring when experts collaborate in an overall project where each has a specified or delimited role that does not require sharing of insights and theories. Since these are abstract definitions, some examples will clarify them and the problems of peer review associated with each. (Many more examples and discussions are given by Epton, Payne, and Pearson, 1983.)

## Reviewing Multidisciplinary Proposals

As just defined, multidisciplinary work often entails a kind of modular or through-put effort that involves a number of disciplinary specialties, spliced together so that each contributes its own, modular, part to a larger research whole.

Industrial, commercial, and medical research is often multidisciplinary in this sense. So, typically, are many modern engineering projects. As an example, consider the following project. Though its details apply to the pharmaceutical industry, it would also apply, other things being equal, to other multidisciplinary projects.

Thus, a certain plant is reported to contain a pharmacologically active substance, and "United Pharmacology, Inc." ("UPharm") decides to develop it commercially. Since multidisciplinary work often has this sort of concrete goal or end-product — an object, a patent, a commercial process, or the like — UPharm can appoint a manager or managerial team to oversee progress through the entire research project, which consists of a series of complex, but modular, events.

First, sources for the active ingredient are identified. This job combines botanical knowledge with experience in agriculture and possibly in the laws regulating importation of plants and plant products. Then comes the problem of isolating, identifying, and then synthesizing the substance, a task involving organic, analytical, and synthetic chemistry. Assays for the substance are developed.

When adequate test quantities of the substance are obtained, the project shifts to the biological researchers, who determine efficacy using *in vitro* and *in vivo* model systems. Considerable skill is needed to make such tests even partially predictive of efficacy in human disease. Presuming that the substance seems effective, it is given next (or in parallel) to the toxicologists for assessment of acute and chronic toxicity, carcinogenicity, and perhaps teratogenicity. The skills required here involve not only technical knowledge, but also knowledge of a large set of federal regulations that concern toxicity and related matters.

Further tests are next made using animal model systems, with the goal of producing prima facie evidence of potential efficacy and non-toxicity for a New Drug Application (NDA). Assuming apparent efficacy, non-toxicity, and acceptability of the NDA, clinical trials are arranged, typically requiring supervision by medical personnel. Experimental design questions are extremely important at this step, requiring statistical consultants.

Now the project moves to pilot- and large-scale production, involving experts in formulation and manufacture. Decisions are made about dosage forms, shelf-life is tested, and, if all has gone well, marketing people now enter the system, to plan advertising and marketing strategies.

Although more details can be added, and although they may vary if the product is paint rather than a pharmaceutical, the flow-through pattern remains the same. In many ways, this kind of research resembles an assembly line: at each point, people with very different specialties ("disciplines") will enter, make a contribution, and then leave, to work on the next panacea (or paint). Hence my term "modular" for this kind of research: the stream of work can be visualized as a set of plug-in units performing different functions in the whole.

It is not necessary that the process occur sequentially in time for it to be multidisciplinary in this modular sense. If the project goals can be achieved without necessary success in another area — a criterion not met in the case of UPharm's project, since no one would waste money on later steps if, say, the product was proven carcinogenic at an early state — then the different experts can work simultaneously and very possibly independently to achieve the subgoals of their assigned portion of the overall project. Stoddard (1982) gives an example involving the multidisciplinary study of a geographic region of the United States (and recounts difficulties with obtaining grant approval); again the fundamental characteristic is that experts in one field can work without necessarily sharing "common frameworks and conceptual tools" (Stoddard, 1982, p. 210).

In both sorts of multidisciplinary work, cohesiveness of the whole is maintained from the top down, from higher management down to individuals in laboratories or television studios. Moreover, the flow of work is from discipline to discipline in a pattern that is dictated by expediency and effectiveness, according to the pre-designed goals of the project.

However, an even more significant aspect of such research concerns the flow of information in the system — the question of who knows what and when. At each step and within each discipline, information about success or failure is channeled upward toward those in charge, rather than laterally. In this way, the manager knows where the project is at each stage. It follows that a crucial characteristic of through-put, modular research is that in principle a person early in the chain need never know what is happening at a later stage. Likewise, in simultaneous modular research, individuals in one portion of the effort do not need to know, in principle, what has or has not been done or found by others who, on paper at least, are working on the "same" project. In such systems, information and results are concentrated at the top, and flow upward and downward rather than laterally.

Of course, investigators working on different portions of the project might communicate with each other, either formally or informally. However, lateral communication is a matter of policy — e.g., UPharm's scientists may share information across the process, whereas individuals working on a high security Armed Forces defense project probably would not — and it remains true that in *principle* a multidisciplinary project can be performed by units separated geographically or by institutional requirements and regulations.

The upward flow of information at each step now creates the possibility for "review" of the entire project. The central managers can evaluate the success of each step again in a modular fashion. Moreover, and very importantly, experts at the different stages can publish their results independently of each other, so that the process retains its modular quality even within the broader framework of external scientific peer review. In specific, at each step the work is judged according to criteria *already existing within each modular discipline*. Thus the organic chemists must meet the criteria of organic chemistry, and the toxicologists must meet the standards of toxicology.

Furthermore, the modular quality of multidisciplinary work implies that failures at any one step are the responsibility of the experts involved in that step, rather than representing a failure of the throughput system itself. Thus, when the animal model experts report lack of efficacy, they — and no one else — must justify that failure by referring to appropriate criteria in their specialty. A failure here cannot be blamed on "misunderstanding" what the organic chemists did; instead, accountability is also modular, throughout the entire chain.

So, more vividly, multidisciplinary work is like a chain of Pop-It beads. Each person looks at, and works within, his or her own Pop-It bead, and must take responsibility for the failure of that, and only that, Pop-It bead. Centralized management alone need concern itself with the overall pattern of success or failure.

**Reviewing Multidisciplinary Proposals**. This model of multidisciplinary research provides one way to conceptualize peer review of a grant application that involves people of more than one specialty. If the proposal tacitly or explicitly represents this form of modular research, then review need focus on only two complementary aspects of the proposal.

The first concerns the credentials and capabilities of each individual named on the proposal: each person must meet all the criteria usual for his or her specialty. No single individual need meet more than one set of disciplinary standards, since the through-put nature of the system means that individual competence is sufficient. Thus, reviewing a modular multidisciplinary proposal is equivalent to performing a series of mini-reviews of each stage and of each individual's skills: can the personnel at each stage be assumed competent, and their proposed methods and approaches be considered state of the art? Contrariwise, a single weak link in such a proposal can also easily be identified, and suggestions for correction be forwarded to the proposal writers, in the form of advice recommending a stronger individual or a better protocol for a certain, specified step in the whole chain.

Yet, second, the possibility exists that even if all the individual links in the system are each highly competent, their overall fit and coordination may leave something to be desired. Multidisciplinary work of this kind requires a high level of competence among the central managers. Thus, a reviewer might erroneously assume that because the Principal Investigator is competent at his or her own specialty, he or she is also competent at coordinating the work of different specialties. However, expertise in one area does not assure expertise in coordinating the through-put process. There is no guarantee that subject matter expert #1 will know enough about areas 2 and 3 to ensure that expert #2 tells expert #3 what #3 needs to know. In order to make up for that lack of knowledge, the Principal Investigator may simply ask expert #2 to tell #3 everything, thereby drowning expert #3 in information both needed and not. Accordingly, successful through-put for modular multidisciplinary research has an absolute requirement that transcends subject matter competence of the individual investigators: the need to coordinate and regulate the flow of information between stages and, if necessary, to redirect the efforts of a given module towards the overall goals of the project.

In brief, then, review of multidisciplinary or modular proposals requires a series of mini-reviews of the competence of each proposed stage, plus asking if the proposal contains ways to ensure the needed coordination of information flow between modules. Often, in commercial settings, the task of coordination is performed by individuals who are not experts in any of the subject areas but who are, instead, purely administrators. It is beyond my scope to ask when or if such a system is ideal; presumably, it depends on the skills and experience of the administrator, something to be decided on a case-by-case basis. However, regardless of the technical and discipline-related expertise of the central managers of modular research, the need for administration remains. Hence, the need for asking if a proposal will be administered properly remains an important part of the review process.

Modular research is not necessarily restricted to commercial endeavors. Any goal-oriented project that calls for several disciplines can be modular. For example, a team containing field biologists, geologists, archaeologists, and environmental scientists might collaborate, in modular fashion, on a project involving the environmental impact of development in a certain region. Even if the "goal" is only a report to a federal agency, here again individuals in each specialty could work solely within their own area of expertise to contribute separate sections to the final report. Overall success then depends on centralized administrators for coordination of the whole project.

**Synopsis and Suggestions about Multidisciplinary Research.** These illustrations permit us to characterize modular multidisciplinary work as involving the coordination of individual research efforts, each done according to criteria for excellence within each area, but which must also be competently articulated across the modules. Administration may be by subject matter experts serving as managers or by professional managerial experts, but, either way, the system requires that information be transferred from the modules to the managers and back. Results obtained within each component can, in principle, be published (and understood) without necessarily citing other results from different modules of the same overall project. Reviewing such projects, either in proposal stages or after completion, requires a series of mini-reviews plus assessment of overall articulation among the modules. Finally, several reviewers may be needed for making such mini-reviews, so that subject matter experts review appropriate subject matter areas.

Crucial questions concerning module articulation include asking whether the through-put proposal contains evidence that the needed coordination will be achieved and how information will be transferred from one module to the next. It seems reasonable to ask that the proposal writers address specifically the competence of proposed coordination personnel, be they the Principal Investigator or specially chosen managers. It cannot be assumed that subject matter competence alone predicts or ensures the ability to coordinate a project among several potentially disparate disciplines.

## Interdisciplinary Research

This characterization of multidisciplinary research allows us to define *inter*disciplinary work in a somewhat different manner. If the crucial feature of multidisciplinary work is coordinating the research modules, then the crucial feature of interdisciplinary research is achieving genuine theoretical and intellectual integration across what otherwise would remain independent fields of research. Interdisciplinary work therefore leads to some very different problems of coordination and proposal evaluation.

The otherwise vague notion of “genuine theoretical and intellectual integration” across fields implies that subject matter experts must become competent in each other's fields to a far greater degree than in modular research. Obviously, such a condition is necessary if we are to speak of *genuine* integration, but that condition is not sufficient. Accordingly, in the following discussion I shall consider first what it means to say that one becomes “competent” in another field, and then take up two further lines of questioning: how “close” two fields must be to assure a reasonable likelihood of competence, and how integration can be produced and how it can be detected by a proposal reviewer.

**Secondary Competence.** From the onset, we must avoid the parochiality of saying that “competence” means having an advanced degree in the second field. To be sure, formal education can produce competence, but it is not the only route. Self-disciplined self-education will also work. Intensive reading in the classic and modern work of the second field is minimally needed, as are — ideally — discussions with colleagues in the new field. Competence also involves knowing the basic paradigms and questions of the second field, and how they have shifted with time. Likewise, continued modesty and a willingness to admit ignorance are essential.

These may be called the “internal” criteria of secondary competence, for they depend on not fooling oneself that — say — reading a *Scientific American* article is the same as achieving a genuine education in the second field. Thus, achieving secondary competence ultimately involves genuine interest and genuine knowledge, and, in that, is not different from competence of any sort.

Yet, idealized descriptions of such internal criteria are no help when one is asked to evaluate someone else’s knowledge, for example in a grant proposal. Instead, we need external criteria for assessing secondary competence.

For specificity, assume that a biologist with interests in anthropology prepares a grant proposal for botanical research in a certain region, and includes a proposal to work with an anthropologist to gather native names for plants and to compare native ideas of plant classification with Western taxonomies. Such a project might be difficult if it were done in modular fashion, because here success means more than simply compiling two lists, one of the Western names for plants and another of native names. Instead, the biological and anthropological researchers must work together, so that when one identifies a plant by its Western name, the other can ask native informants about its native name and alleged properties. The two disciplines — taxonomical biology and anthropological fieldwork — have been brought into close and necessarily intimate connection.

How can a reviewer assess the needed secondary competences of these two investigators? The biologist should know some principles of anthropology, and the anthropologist of biology. How can this mutual knowledge be assessed?

It is easiest to say what assessment is *not*. The reviewer may readily be tempted to assume appropriate levels of secondary competence merely if the proposal seems methodologically sound, that is, if the taxonomic and ethnographic research protocols are each individually sound. However, the reviewer's assumption amounts to transforming this interdisciplinary proposal into a modular proposal — an easy way out, to be sure, but one that misses something crucial. We can assume that alone the biologist is competent (which can be judged by prior research and education) and that alone the anthropologist is competent (which can be judged in like manner). But solo competence does not necessarily produce the interactive integration needed for success. In this project, the two researchers must work together, to develop questions and procedures about problems that they may never have encountered before. For example, the botanist identifies as two species a plant for which the natives have only one name. Given that each researcher is competent with respect to the issues involved — that there really are two species by Western botanical standards and just one native name by Western ethnographic standards — the real question is what causes the *difference* between Western and native taxonomy? It is not sufficient merely to report that the difference exists; the true *interdisciplinary* question is why it exists. Elucidating that problem now will require that the biologist have sufficient ethnographic field skills to speak intelligently to native informants, and that the anthropologist have sufficient botanical skills to understand the biologist's conclusions. Accordingly, the focus shifts from a modular question (what names are given to these plants by Westerners and by the natives?) to a genuinely integrative one (why are there differences between the principles of Western and native taxonomy?). And my point is that the likelihood of the second question being answered depends on how competent the two specialists are in each other's fields.

For purposes of proposal assessment, the competency question is partly answered by the work cited by the investigators. There is a considerable literature on so-called "native taxonomies," and, in fact, anthropologists would call this proposal "ethnobotanical." So, no matter how skilled the biologist is according to internal standards of prior reading and so on, the proposal must meet external criteria, by including references to the ethnobotanical literature.

However, with this, we reach the central problem in reviewing interdisciplinary research. The reviewer *also* must know that a field called "ethnobotany" exists. In brief, reviewing interdisciplinary research proposals requires that the reviewers themselves possess sufficient secondary competence to determine if the needed literature has been cited. Otherwise, it is the blind leading the blind.

**Inappropriate Modular Review**. The familiarity of modular research may lead reviewers of interdisciplinary research to make precisely the mistake illustrated above. The reviewer encounters a proposal involving more than one discipline and presumes that its validity can be measured as a kind of sum of validities of its separate subject disciplines. The reviewer in essence says that the biologist seems competent, and then a second reviewer agrees that the anthropologist seems competent. But validity, as the preceding example indicates, is not the simple sum of competences within modules, for now we are dealing with interactions between two disciplines. The results that arise are more complex than a simple summation would imply.

Yet one can easily imagine an anti- or non-interdisciplinary scholar or reviewer arguing that these natives were simply mistaken and were backward in their ignorance of Western scientific botany. Although that ethnocentric viewpoint seems to have a sort of face plausibility, it misses a crucial component of the overall phenomenon under study: the question is not the purely botanical *What plants exist in a certain locale?* but the interdisciplinary question *How do Western and local native taxonomies differ and why?*

To answer the first question, an investigator must be competent in modern botany and taxonomy, qualifications that can be assessed by a reviewer in a modular and unidisciplinary fashion. Answering the second requires something in addition: the ability to understand that the difference between Western and native taxonomies is *itself* a research problem, and one that transcends the borders of either botany or ethnography. The second question is integrative, and although nothing forces scholars to raise such questions, once asked they cannot be answered within the limits of one and only one science or discipline.

I suspect that it is this confusion between modular and integrative research that makes review of interdisciplinary research so difficult. The reviewer can no longer rely merely on his or her own knowledge of (say) scientific botany, but must also be able to recognize when and where an integrative question is being asked. Accordingly, interdisciplinary work requires that proposal writers and reviewers each have sufficient secondary competence to recognize when the appropriate interdisciplinary questions are being asked. However, since disciplinary tunnel-vision does exist, some additional burdens are created for both the proposal writer and the reviewer. First, proposal writers need to display bibliographic evidence of their knowledge of how the two (or more) fields fuse and integrate and to describe how the fields interact. Second, the reviewers must possess sufficient knowledge of the two fields to be able to evaluate the proposal writers' knowledge of that fusion. It is explicitly and centrally a time for knowing more than the paradigms and theories of one's own field.

**Excessive Review Demands**. But, with that, we encounter another problem. When confronted by a proposal or manuscript involving disciplinary interactions (e.g., the ethnobotanical proposal above), a reviewer can be tempted to over-utilize his or her own competence, and thereby demand a higher level of competence than is actually needed for the project's success. Thus, the reviewer reads the ethnobotanical proposal, spots the omission of much work, and — applying the criteria from his or her own field — concludes that the author(s) are simply ignoramuses.

Here, I suggest that the ideas of primary and secondary competences come to our assistance. The question is not whether the biologist is completely competent in the secondary field, but whether or not he or she knows *enough* to carry the project to successful completion. To be sure, an academically trained anthropologist may know a great deal about ethnobotany, including its history, its applications in many different cultures, and its relevance to broader theoretical issues in anthropology. However, assessing a proposal is not the same as evaluating a graduate student's qualifying examinations. Instead, the question the reviewer must ask is whether the writer's secondary competence is adequate for the purposes of the grant, *given* that he or she will be collaborating with an anthropologist.

Since it is not my purpose here to criticize published interdisciplinary work, I'll not make a list of cases in which someone's secondary competence seemed inadequate for genuine interdisciplinary integration. Instead, consider my own earlier claim that one of the ideas for this paper came from chaos theory. I have only a layperson's knowledge of chaos theory and can use it only as a source for an intriguing metaphor. A hypothetical reviewer who was assessing my knowledge for the purposes of grant evaluation could quite correctly infer that I lack the expertise to collaborate with a mathematician and an information scientist to determine if, say, the scientific literature grows according to the rules of fractal mathematics. On one hand, that lack does not prohibit me from using what knowledge I have as *metaphor*, and a literary critic — not a mathematician! — might find the metaphor intriguing for how it juxtaposes two disparate processes. On the other hand, the technical reviewer of that grant application could properly consider my knowledge simply inadequate. For the technically trained person, chaos theory — and, by extension, *any* theory in the social or natural sciences — is not a collage of metaphor and misunderstanding: it is a complex body of knowledge with its own internal logic and rigor.

Whereas the example might seem overly personal, it nonetheless illustrates a point. Understanding that is adequate for deciphering technical writing in a field that is not one's own is *not* the same as the understanding which comprises genuine secondary competence. The latter need not equal the level of the life-long expert's knowledge, but it must visibly be sufficiently great for the reviewer to decide that it forms the basis for potentially valid technical communication between experts.

In essence, we have here a criterion of information flow and translatability. However, unlike the modular case, in which information flow is up and down, in the interdisciplinary case information flow must be lateral, directly from one expert to another. Accordingly, the question the reviewer must ask is whether the two experts have sufficient knowledge of each other's fields for that information flow to constitute *communication*. Do the researchers speak each other's languages well enough to understand when they have encountered a new — and genuinely interdisciplinary — question? The answer, in part, demands again that the reviewer him- or herself must be able to recognize what constitutes real communication between two fields, and to distinguish it from mere cross-citation and metaphor-mongering done by a skilled grantsman to create the appearance, but not substance, of interdisciplinarity. (A drastic and expensive solution is a site visit, where grant program officers ask directly about communication.)

Yet, there is a danger on the other side as well. A reviewer can be too generous, becoming simultaneously both unrigorous and condescending. This reviewer notes the omission of crucial work (at least it is crucial in the reviewer's own field) and then says, "Well, what do you expect from biologists? *They* don't know anything." Next, the reviewer says, "Oh, well, let it slide — that's not the major thrust of the proposal anyway," and perhaps recommends that that portion of the proposal either be dropped or expanded by including someone with the "proper" subject matter expertise.

The frequency of such laxity — or generosity, depending on one's viewpoint — is probably nearly impossible to ascertain. Few conscientious reviewers would readily admit to laxity, though some might admit to generosity. Furthermore, it is unclear how long such a reviewer will remain on a granting agency's list of trustworthy referees. This whole matter deserves more research, as does the possibility of "narrowly trained 'expert evaluators'" being annoyed by the inclusion in a grant proposal of extraneous material — i.e., material not in their field of expertise (Stoddard, p. 213). It assuredly seems that the problem is finding a balance between too much laxity or generosity, and too little, and I suggest that the notion of secondary competence may help achieve the needed balance.

So, here, we encounter a practical difficulty. In theory, it is sensible to talk about achieving a balance, but how can we be reasonably sure in practice that we are neither too harsh not too generous with an interdisciplinary proposal? One answer — which I have already rejected in this context — is that interdisciplinary work should be treated like multidisciplinary work, in which each subject area is the perhaps jealously guarded preserve of subject matter experts alone. This seems no answer since it denies the existence of between-field interactions that should be evaluated by reviewers who possess the relevant secondary competence. Perhaps the best solution is the least rigorous, at least quantitatively: it concerns the *ideas* presented by the proposal writers. Given that the reviewer is an expert in area A, with secondary competence in area B, are the proposed ideas interesting, challenging, or potentially valuable for extending the existing boundaries of fields A and B? Of course, all proposal writers claim the virtues of interest and innovation for their ideas, but, despite such rhetoric, an important core remains.

That core concerns specificity and clarity of the proposal itself, and corresponds to the sixth issue raised by Lock (pp. 97-98). However, I am not referring now to the methodological aspects of the proposal, but to its ideas. Are they presented in vague and glittery generalities (e.g., "The proposed research addresses central concerns at the interface of areas A and B") or are the ideas specific? The ideal proposal should, I suppose, whet the appetite of the reviewer: "Hmmm — I never thought of that connection before. I want to know more." The key word, then, is *immediacy* of writing: if the proposal writer can gain the reviewer's interested attention, then it stands to reason that the project will also be interesting when completed.

It may sound odd to link writing style (e.g., immediacy) to the intellectual content of an interdisciplinary proposal, particularly since Lock (pp. 97-98) suggests that scholars *object* to having writing included as a criterion for grant approval. Yet, lack of clarity will sabotage an interdisciplinary proposal, simply because most readers will not know — nor care about — the jargon of the secondary field. For example, if I write that the "hypotheses of Fuldenbeiner and Balakireff will be tested in an ethnographic setting," I have communicated nothing — and I have certainly not made it clear to the reader why these people's ideas are relevant to anything at all. Such sentences may contain the necessary technical content, but they are more appropriate to a specialist journal than to a proposal. They do not make the reader aware of the rich potential of linking two hitherto unlinked fields.

I am not, of course, suggesting that proposal writing become an exercise in popularization. Instead, the implication is that after knowing the literature, the next crucial need in interdisciplinary proposal writing is the ability to explain to non-experts what is being proposed and why. And the reason is simple: despite a reviewer's possible secondary competence, he or she is closer to the layperson than to a lifelong expert. Hence, this requirement for writing a proposal means thinking to the heart of the matter and then explaining it carefully and clearly to someone who is not trained in both areas.

From this comes a criterion for assessing interdisciplinary proposals. If the proposal does *not* contain clear and specific explanations of how and why the different fields link up, one can legitimately wonder if the writers themselves understand what they are proposing. Thus, implicit in the need for clear writing is a suggestion for how to review such proposals: clear writing and explicit thinking probably bode well for the project — and conversely.

**Is It Sufficient to Assess Proposed Methodology?** Any reviewer of scientific proposals must evaluate how likely it is that the proposed or completed methods will reach the work's stated objectives. But, when one deals with interdisciplinary work, there are complexities not found in unidisciplinary reviewing. Within a discipline, it is legitimate and necessary for the reader to ask if standard, proven methods are proposed or, if they are not, to ask precisely how the modifications or innovations might reach the desired goals. Here, the reviewer can draw on his or her own within-discipline research experience to assess the likelihood of methodological success. If the reviewer believes that one should employ traditional — i.e., well-tested and well-proven — methods, the proposal might well be downgraded if it does not use them. However, if the methods proposed have been tailored for an *inter*disciplinary project, it may be less appropriate for a reviewer to dwell on the absence of standard methods or the uncertainties surrounding the proposed alternatives, than to consider instead how and why methods suited for research *within* a field might be counterproductive or marginally useful when used *between* fields.

Accordingly, a reviewer must expand his or her methodological vision when confronted by an interdisciplinary proposal. Interdisciplinary work entails borrowing ideas and theories, and likewise involves borrowing and modifying methods. Two requirements thus seem to appear, one for the proposal writer, and one for the reviewer.

The proposal writer should explain clearly and explicitly why the methods have been chosen and why they have been modified from traditional or standard within-discipline techniques. Preliminary data are most useful for such a purpose, but so would be an analysis of what the modified methods are supposed to do. Yet, if the proposal writer is too terse in explaining methodological rationales, it remains the reviewer's responsibility to fill in the missing details.

An example arises when natural scientists become interested, as some do, in human social behavior. When we cross from one domain of science to another, we expect to find different sorts of theories being used to explain different sorts of subject matters — we do not expect to understand molecules and people in the same terms. It may be harder to see that subtly different methods are also needed.

Thus, there are subtle differences between how natural and social scientists understand the concept of "experimentation." For the social and psychological scientist, experiments with human beings are necessarily surrounded by ethical concerns, akin to those arising in certain types of biomedical research. Yet, beyond ethical problems, other differences also exist. A central problem is that human behavior is multivariate, and often over-determined. It may simply be impossible to isolate a *single* major cause for a behavior pattern, and, in consequence, an experiment designed to elicit single-cause explanations for human behavior is probably methodologically and theoretically unsound. However, the art of the experimentalist in the natural sciences is to achieve precisely the goal of showing how a single variable, or several variables taken one at a time, control or determine the system's behavior. A social scientist might automatically select methods that display multi-variate effects, while the natural scientist might want methods that show how one thing works at a time. "Why are you measuring so much?" asks the natural scientist, and the social scientist replies "Why are you measuring so little?"

Clearly, it is no easy task for a reviewer to assess how and why methods might change when a project becomes interdisciplinary, but the effort must be made. Again, it seems next to impossible to assess how frequently methodological tunnel vision — which amounts to misunderstanding the characteristics of the second field — has sabotaged the review of interdisciplinary proposals. However, in the hands of someone devoted to methodological purity above all, it may be a serious danger.

How can the danger be lessened? The concept of secondary competence requires that the reviewer become knowledgeable about the methodological problems of the second field, and be sensitive to how those problems can interact with problems arising in the first field. A second generality concerns multi- vs. univariate techniques, just mentioned. But there is a third and subtler issue: if the proposal is genuinely innovative, no one may be able to tell if the methods proposed will work in the *real* world (as opposed to the rhetorical world of grantsmanship). It does not matter, in this regard, whether granting agencies like or dislike open-ended or potentially uncertain research. Even the most firmly worded and seemingly guaranteed methods may come to grief. And matters can be made worse if the reviewer insists that a proposal should use the "time-honored" methods of only *one* field.

In specific, consider the need for control groups in social scientific research. For the natural scientist, a control group may be nothing more complex than a set of people who "did not receive" the experimental treatment, whatever it may be. If so, then — put colloquially — any batch of people might do as a control. For example, in a biomedical paper in my files, the question was whether or not women seeking transsexual surgery ("sex change" surgery) differed hormonally from women not seeking surgery. A control group had to be selected, and, in this paper, women medical students were chosen. It is a doubtful choice indeed: the stresses of medical school *themselves* might produce hormonal changes. The published paper is thus flawed in a major way. However, the purely biomedical researcher might not think of the social milieu of the "control group" women as potentially affecting them physiologically.

Yet, even if the proposal writer does not think through the interdisciplinary correlates of the work — as these researchers apparently did not when they planned their procedures — the proposal reviewer must do so. In general, for all biomedical work — which probably represents a fair proportion of all grant-funded research — the question of selecting a control group, seemingly a trivial issue, raises *major* questions of an interdisciplinary nature.

The example also widens our focus, for it shows that work which *seems* unidisciplinary — here, a "simple" biomedical question of human endocrinology — may in fact have deep rooted connections to domains studied by people in other fields. These connections lurk not in the results, nor in the rhetoric of the proposal's introduction, but in the innocent-looking descriptions of the Methods section. The conclusion, which I believe is extremely important, is that whereas methodology assessment is not alone sufficient for evaluating a proposal, it presents the strongest need for interdisciplinarity — and for the reviewer to possess genuine secondary competence. A sociologically trained reviewer could have helped that proposal on transsexual women immensely, as unconventional, untraditional — and even radical — as the idea might sound.

**Face Validity of Interdisciplinary Research**. There exists another external criterion for assessing interdisciplinary proposals. It can be called "face validity," and refers to the prima facie believability of the proposal. Does it make sense to adjoin the two fields in the proposed manner, or does the linkage seem artificial (and perhaps merely thrown together as an exercise in grantsmanship)? This question leads to the notion of the "closeness" of fields.

The importance of the idea of "closeness" lies in the presumption — sometimes a reasonable one, I suggest — that the success of an interdisciplinary project will be greater the "closer" the two fields are to each other. To see how this idea leads to review criteria, I will expand somewhat on what it means to say that two fields are "close" to each other.

The first way of defining closeness is bibliographic ("bibliometric") and depends on assessing how frequently workers in the two fields cite each other's work. Abstractly, imagine fields A and B, between which there is as yet no perceived link. If we examine the journals and papers in field A, we shall find few, if any, citations of work in field B, and vice versa. There is simply no reason for workers in these fields to cite each other. However, imagine next that a few researchers see hitherto unknown connections. Their first publications will obviously cite papers in both fields. And, if the connections prove valuable, as time passes, more and more authors will cite from fields A and B, and workers originally in one field will begin to cite from the other. This pattern of change can be detected by quantitative analyses of bibliographic entries (this topic has a large literature: Garfield, 1972; Garfield, 1983; Porter and Chubin, 1985).

It is then clear that, at first, fields A and B were far apart, but came closer and closer as connections between them emerged. Indeed, what has emerged is nothing less than an interdisciplinary area of research. Thus, analyses of co-citation patterns in bibliographies can reveal the existence of an incipient or developing interdiscipline.

An example of this process, illuminating because it illustrates how seemingly unlikely such combinations of fields can be, concerns classical Mendelian genetics and mathematical information theory ("Shannon-Weaver" information). Prior to approximately 1954, few geneticists bothered to read or cite work done in cybernetics or in mathematical information theory, and if we had tabulated cross-references from genetics journals to information theory research, we would have found few cross-citations. On the face of it, there were simply no obvious connections between the biological science of inheritance and a mathematical theory then applied primarily to telephone switchboards and noise on communication channels. However, with the discovery of the structure of DNA in 1953, such a connection became possible. But it was not made by geneticists so much as by physicists, who were familiar with the mathematical notions of information and could apply them readily and directly to explain the function of nucleotide sequences in DNA (e.g., the astronomer George Gamow; see Olby, 1974, especially Chapter 15). Almost immediately in genetics there sprung up the now fundamental ideas of "genetic information" and the "encoding" of protein structure in DNA. Yet, in 1950, who would have thought that a theory developed for dealing with messages transmitted on telephone lines could have anything to do with the most intimate aspects of cell structure and function?

We therefore seem to have in the history of citation patterns a way of detecting an emerging interdiscipline and a way of measuring how "close" two fields are or have become. Yet, since such assessments are *post hoc*, they do not help the reviewer recognize a brand-new field: co- and cross-citation research can identify fields once formed, and can track their growth, but what happens prior to their formation, say, in 1952 in the history of genetics?

With this question, we encounter the idea of face validity in its full power. An individual reviewer, looking at a brand-new and *perhaps* genuinely innovative integration of fields, can rely, it seems, only on his or her intuition that the fusion will be fruitful. In fact, it seems that the only real test of fruitfulness is to try it — which entails, at a practical level, approving essentially all interdisciplinary proposals. Yet, that idea falls afoul of our sense that certain fields have nothing to do with each other in *nature*: that, for example, as far as we know now, it is senseless to talk of the psychology of electrons. But I will now suggest that some criteria do exist for assessing the face validity of a proposed interdisciplinary fusion of fields.

**The Circle of Disciplines.** There are many ways to organize and arrange the intellectual history of humankind. However, for our purposes, one way of arranging the fields of scholarship provides some hints about face validity of interdisciplinary work. This arrangement utilizes existing overlaps to build a larger model of how currently existing fields interact.

Let me begin with mathematics, surely the queen of the sciences. If we ask what kinds of problems professional mathematicians are drawn to, we quickly encounter physics. It is simply impossible to envision modern physics without its mathematical formulations and derivations. And, indeed, an "interdiscipline" called *mathematical physics* exists and is of high lineage. So, if we draw a small circle to denote mathematics — a Venn diagram containing all mathematical work — that circle will surely overlap strongly with the circle we would draw for physics. Yet, next, we recognize that physics and chemistry have strong affinities for each other (in "physical chemistry" and, of course, thermodynamics). So the circles of chemistry and physics overlap in real and important ways. But we also recognize that in this century biology has drawn heavily upon chemistry (as "biochemistry").

From these well-known examples, a principle seems to emerge. The structure of science as an intellectual endeavor grows by expansion of fields until their boundaries touch and "cross-fertilize." This admittedly biological metaphor suggests that interdisciplinary work is at the heart of scientific progress, because, as soon as the new interdiscipline is born, researchers begin to expand its domains. The result becomes, finally, an approximation to the seamless web of knowledge to which the efforts of science aspire.

Moreover, if I may be permitted to continue the metaphor, the Venn circles of the natural sciences soon grow small pseudopods at their edges. A breakthrough has occurred which generates its own literature and, in turn, further breakthroughs follow. Of course, some of these pseudopodial Venn circles expand in other directions, particularly towards the practical and applied sciences, such as engineering. But, applied science aside, these intellectual pseudopods sometimes encounter similar pseudopods emerging from other fields and coalesce with them. Then we have an interdiscipline born.

Before drawing some conclusions about peer review in these interdisciplines, let me complete the circle. In recent years, biology and psychology have come closer together, in fields such as behavioral biology and psychobiology. Yet, from psychology we can move towards the social and behavioral sciences per se, such as social psychology, sociology, anthropology, and economics. And, in turn, they are intimately related to history and political science. (In each case, there exist named interdisciplines between these fields.)

But psychology and anthropology also lead towards linguistics (as, increasingly do certain areas of biology). From linguistics we can move towards the humanities and philosophy (through the "philosophy of langauge," e.g., Searle, 1969). From philosophy to logic, and from logic to mathematics. The Venn circles of knowledge all ultimately overlap and interact, though sometimes only indirectly.

And interactions occur not just between neighboring circles. I have already noted the interrelations of genetics and information theory, but similarly there is the emerging field of biohistory (e.g., Karlen, 1984). And then there are geography, engineering geology, archeology, architecture (naval and otherwise), materials science ... the list becomes encyclopedic as more and more relationships are found and developed. Accordingly, co-citation, already mentioned as one way of detecting new fields and interdisciplines, is not the only way for seeing when and where scholars cross the Venn circle of traditional disciplines. Other ways include the appearance of new journals (often with interdisciplinary names), national and international conference titles, and — in academia — the presence on doctoral committees of faculty in "related" fields. For example, in electronic music, MIT Press publishes *Computer Music Journal*, which points not merely to the existence of an interdiscipline, but also to the growth of standards of publication and accomplishment in a new field. In its pages appeared Kemal Ebcioglu's (1988) report of an artificial intelligence system that harmonized Bach-like four-part chorales when given a theme. Assuredly, that is a triumph of adjoining computer science with the humanities, even if Ebcioglu modestly says only that "… its competence approaches that of a talented student of music who has studied the Bach chorales" (page 49). It is a fitting metaphor: those of us who seek in interdisciplinary areas for new ideas, as well as for standards by which to judge them, wish also to become talented students.

**Interdisciplinary Mediating Fields**. What does this admittedly simplified Venn diagram model tell us about reviewing interdisciplinary proposals? First, we have learned something of what interdisciplinary work is not. Scholars and scientists do not sit down and choose fields to put together, as if they were saying "Hmmm. The intersection of philosophical logic and geophysics looks interesting. Let's do some." Instead, the growth of different fields towards each other is far less arbitrary, and represents two organically connected processes.

One is growth of already existing tendrils of interest and discovery that the two fields have sent towards each other. When they listen to each other, scholars sometimes find common realms of interest, and collaborative, integrative work may begin. Although later such efforts may falter, when they start they represent no mere exercise in juxtaposition but a serious belief that interdisciplinary cooperation may be fruitful in theory and in new findings. Virtually by definition such beginnings involve only a few scholars, and also virtually by definition their opinions deserve respectful audience even if one is critical. We cannot ignore it when a scholar from field A says "Topic X is difficult, and it appears that insights from field B may be helpful."

This form of intellectual development is personal in the sense that the insights and hunches of two individuals are involved. However, there is a second, less personal process that also occurs when two fields are seen as mutually illuminating. This process represents connections that exist in nature itself. For example, it turns out empirically that in our universe biological processes depend on chemical reactions (and they on physical events), leading ultimately to the development of fields like biochemistry and biophysics. In another universe, perhaps biological processes depend on mystical vital forces, but that universe seems to exist only in literary fantasy and science fiction. If scientists and engineers lived and worked either in Piers Anthony's fantasy world of Xanth, or in L. Frank Baum's Land of Oz, perhaps then interdisciplinary fields between linguistics and materials science might exist, as a fantastic interface of spells spoken by the magician on one hand and concrete objects like aluminum pipe on the other, but until such time and place a scholarly and practical field called "aluminum linguistics" has no basis in the physical laws of our own (real) universe.

Although philosophically a complex topic, the "reality" of connections between aluminum metallurgy (say) and chemistry forms an underlying substrate upon which interdisciplinary fields can develop when scholars begin to collaborate. Thus, these scholars rely on more than mere mutual and personal interest or respect when their work begins and grows; their work also depends on the existence in *nature* of discoverable connections between the fields, and not on the purely personal creation of a fantastic universe in which bauxite melts and extrudes pipe when the proper magical words are spoken. Electrical energy pumped into a bauxite/cryolite mix yields aluminum, and that *fact* makes possible the development of profound and extensive connections between energy production as a part of physics and aluminum utilization as a part of engineering.

These considerations suggest that between two "major" fields (e.g., biology and chemistry) there exist well developed interdisciplinary fields which, in practice, serve to transmit theories and findings about nature from one field to the other. Thus, a cellular biologist learns of the importance of chemistry not primarily by taking organic chemistry courses, but from studying what can be called the "mediating" field of biochemistry.

However, mediating fields exist not merely because they are information conduits. They represent the aggregate sense and experience of workers in the parent fields that real connections do in fact exist between fields that now must be seen as "isolated" only in the historical sense that no one recognized their connections before now. These mediating fields serve to bridge over gaps left by preceding intellectual history, and thus come to approximate better and better the ideal of the seamless web of knowledge. In this view, it is merely an historical accident that something called *bio*-chemistry exists, for the name reflects only the historically prior condition of our knowledge, in which biology was one thing, and chemistry another. (And the study of such matters is *itself* an interdiscipline, as the history of science.)

However, prior conditions change, and even academic departments slowly alter their intellectual isolationism as scholars cross boundaries and establish the validity of viewpoints taken from hitherto "different" fields. Yet, personal interest in crossing boundaries does not suffice for establishing an interdiscipline; solid data, representing the real world, are also needed. It must be shown that biological processes depend on chemicals, and that enzymes are not magical entities, but are protein molecules such as chemists might study.

Of course, the example of biochemistry points to an existing interdiscipline, inviting one to ask about new interdisciplines. They do not have the patina of age and data to make them so respectable, but one can still draw a conclusion about their potential validity. When hitherto unrecognized real or natural connections exist between fields, then new interdisciplinary collaboration will bear fruit, at least if done competently. Since, at the start, we do not know with certainty if such connections exist, scholars and scientists must do the work and gather the data. From this, an initial criterion for assessing the potential fruitfulness of an interdisciplinary proposal can be suggested that does not depend on assessing the personal rapport between two scholars. The question is simply whether it makes scientific sense to juxtapose the fields, or if the proposed juxtaposition is merely an idiosyncratic invention of fertile minds, such as aluminum linguistics.

This criterion is perhaps a bit conservative, although it is practical. If interdisciplinary progress fills the gaps between fields, then the face validity of the proposal increases the closer the two fields are in the Venn sense outlined above. The reason is simply that two already close fields are more likely to share *real* connections than are fields that seem remote by the Venn standard.

However, this criterion must be used with extreme caution. For example, geology and psychology seem to have little to do with each other, and we do not speak of mineral psychopathology, at least outside of Piers Anthony's world of Xanth. Yet, if we consider people living in a certain geological area, their perceptions and beliefs about their environment certainly pertain to the field of psychology — and, in this case, a seemingly implausible juxtaposition makes sense because one can point to the field of *geography* as mediating between the seemingly disparate fields of geology and psychology. Thus, people's perceptions of their environment — of its geology, terrain, soil, climate, water, vegetation — exist within social and cultural systems and structures. Accordingly, we see geography as the systematic study of the relationships between the physical environment and human activity, and its interests include examining how people react to their physical environment, modify it, and employ it for various social and economic purposes. Thus, what began as seemingly disparate fields — geology and psychology — reveal themselves as intimately connected, once the mediating field — geography — is recognized as such.

Accordingly, the Venn model by itself is incomplete, for it locates the sciences and humanities in ways that suggest that sometimes they are diametrically different — and can offer nothing to each other. Yet, as the example of geography illustrates, the impression is false. Even so, I suspect that many scholars — reviewers of grant proposals among them — accept as intuitively obvious and intuitively true that fields on opposite sides of the Venn model "have nothing to do with each other," and that a proposal juxtaposing them is *therefore* silly nonsense. It is probable that the Venn model better represents the organization of academic departments in the university than the real nature of connections between real phenomena. If so, then a scholar who rejects a proposal juxtaposing psychology and geology (say) as silly is simultaneously asserting that we in *our* department have nothing to do with them in *theirs*. No doubt it is an accurate reflection of how some universities operate, but it is questionable indeed if nature works the same way.

**Some Practicalities.** A more general principle seems to follow from this discussion of mediating fields. Most interdisciplinary proposals probably belong to two (or more) fields between which mediating fields have already begun to develop. And, if so, then the reviewer's problem is much reduced. No longer need he or she depend on intuition or hunch to assess the face validity of the proposal. Stoddard (1982) in fact suggests that the best way to get a large, multidisciplinary proposal funded is to begin with a module reflecting a known *inter*discipline. If the reviewer is well enough educated, he or she should be able to place most interdisciplinary proposals into a map of knowledge and recognize what, if any, already existing interdisciplines pertain to the proposal.

In brief, then, the well-educated reviewer should be able to discern quickly that a proposal about psychology and geology is intimately connected to geography, or that a proposal about information science and nucleotide structure is now part and parcel of biochemical genetics. Yet, from a practical viewpoint, no individual reviewer will have sufficient secondary competence to review all secondary areas touching his or her primary field of expertise. A biological reviewer comfortable with social scientific applications of biology may have to defer, when it comes to a bioengineering proposal, to a biologist whose secondary expertise is medical instrumentation. Though the principle is obvious when the two fields are close (either in a citation sense or in the sense of my illustrative Venn circles), it may be harder to put into practice when the fields are not close. Then, I suggest, it may be the responsibility of the program officer of the granting agency to make a major effort to find reviewers with appropriate secondary competences, or, failing that, to alert reviewers to take special care in their reviewing. Such practical difficulties aside, however, the conclusion may be put into an aphorism: interdisciplinary proposals require interdisciplinary review, by reviewers whose primary and secondary competences match those of the proposal writers.

Yet, as I mentioned before, these suggestions are fairly conservative. By matching interdisciplinary proposal writers to interdisciplinary reviewers, there will be a tendency to utilize individuals as reviewers who are working in already existing (or at least identifiable) areas of disciplinary integration. Since such areas presumably grow between “close” fields, the suggestions above amount to saying that interdisciplinary work will be approved by reviewers when its interdisciplinary reach, so to speak, is not very great. Again we encounter the intuitive notion that fields located across from each other in the circle of disciplines described above — say, between psychology and astronomy — will not easily be integrated outside of science fiction. And the implication is that even a truly interdisciplinary scholar asked to review a proposal may find it farfetched indeed to see linkages between telephone networks and genes, at least before the linkages are demonstrated theoretically and empirically.

So, the face validity of interdisciplinary research falls into a continuum — fields close together yield higher face validity when liked by a proposal writer than those far apart. Furthermore, in that continuum, the difficulty of reviewing changes. For close fields, where face validity is high, it is easier for a reviewer to assess an interdisciplinary proposal, and probably competent reviewers are easier to find. But as the fields diverge in a citation or a Venn sense, face validity drops and competent reviewers are harder to come by. One can speculate that not many reviewers, even those sensitive to interdisciplinary research issues, will treat "farfetched" proposals favorably. So, the conservative strategy for the reviewer or granting agency officer is to rely on the "closeness" criterion for assessing face validity of an interdisciplinary proposal. However, conservatism can reject quite valid proposals — for example, proposing an artificial intelligence system for harmonizing four-part Bach chorales — merely because they seem outré or silly. Caution and conservatism must be balanced by recognizing that sometimes deep connections between hitherto unconnected fields can be revealed by a well-designed and well-executed interdisciplinary piece of work.

## Some Concluding Thoughts on the Ideal and Non-ideal Reviewer

Without extensive empirical investigations on how interdisciplinary work is assessed, it is probably premature to propose hard and fast criteria for selecting reviewers. It is easy to say the reviewer should tolerate novelty; it is harder to imagine giving psychological tests to reviewers to measure their acceptance of novelty. Even so, some general principles emerge that concern a reviewer's competences and abilities to transcend his or her own disciplinary boundaries. The willingness to transcend is, perhaps, the primary criterion for selecting reviewers from the program officer's viewpoint. In fact, it may be proper to suggest that reviewers of interdisciplinary work should be more interdisciplinary than the authors are. If one chooses individuals intolerant of scientific novelty, the result is foregone: rejection of the proposal no matter what its real merits. For a program officer to adopt such a stance seems to me to violate the principle of funding science and scholarship in the first place.

Of great importance is the reviewer's ability to determine if genuinely interdisciplinary questions are being asked. Fundamentally, the question always is *Will this piece of work contribute to the integration of different disciplines?* Such integration is both theoretical and methodological, and its detection and evaluation depends on sensitivity to differences in theory and method between the two fields. Assuredly, the ideal reviewer — and those who strive for honesty and fairness if they cannot be perfect! — must be at least as sensitive to the proposal writers' concerns (such as those outlined by Lock), as they would be if the proposal were their own and in the hands of possibly unfriendly folk.

Among other burdens placed on a reviewer of an interdisciplinary proposal is that he or she must have primary and secondary areas of competence, be able to assess the degree of knowledge displayed (and needed) by the proposal writers, be neither too stringent nor too lenient, and be able to decipher jargon and dense technical prose from several fields. Likewise, the ideal reviewer must avoid the sort of feeble methodological criticism that can range from "I don't know this technique, so it can't be any good" to "What do you expect from biologists, anyway?" Program officers should recognize that methodological tunnel vision exists, and should warn reviewers against hasty assessments of untraditional methods.

Then, the ideal reviewer must also know how different fields of scholarship fit together, and be able to determine from that knowledge, if the proposal raises genuinely important integrative questions. So the reviewer should know the literatures and have an intelligent understanding of where they are going.

It is not clear how many reviewers meet such Utopian standards. However, it is also not clear that all reviewers must be universal polymaths. If, as already suggested, multidisciplinary research requires careful administration and information channeling, perhaps genuinely innovative interdisciplinary research requires a more laissez-faire ambience, harder to assess and even harder to create, in which one scholar can wander over to another and say "You know, I was thinking about what you said yesterday —"

So, at least of equal value to the interdisciplinary reviewer is a genuine interest in making connections between fields and furthering the prospects of others who also want to make them. *Intelligence* perhaps best describes this quality; that, and *curiosity.* Certainly, lead-footed adherence to the rules of one discipline can reduce scholarship to a barracks drill by the numbers. Ultimately, then, perhaps the most valuable trait a skilled and trained interdisciplinary reviewer can have is intuition — the intuition that leads such a person to say that *this* particular fusion of hitherto unlinked fields is interesting and is worth funding. Now, intuition alone is valueless: it becomes crucial only when tempered by knowledge and sensitivity to the often unpredictable course of science. Aids such as citation analyses and various maps of science are certainly helpful, yet mechanical application of such analyses and maps can be too conservative. In fact, it is with proposals that *seem* unlikely that the greatest care is needed. Some may be utterly unlikely, such as studying the psychology of electrons. Others, however, may prove profound. The least a reviewer of interdisciplinary research can do is recognize the great responsibility entailed in deciding which proposals shall and shall not see the light of day in actual research and potential discovery.

Throughout, my premise has been that interdisciplinary research often generates powerful new insights into nature and humankind. Whereas Kuhn (1970) is no doubt correct that much scientific and scholarly progress is made by completing a paradigm — that is, by research within a discipline — yet it still seems true that interdisciplinary cross-fertilization has had immense effects on how we perceive nature and the place of humanity in it.

Furthermore, it is my strong opinion that such research needs to be protected as well as evaluated critically. Methodological tunnel vision, lack of necessary secondary competences, unwillingness to take the time to *think* about an interdisciplinary proposal may all be historically and sociologically understandable, and even be the inevitable outcome of dividing up the otherwise seamless web of knowledge into what are conveniently called "disciplines." Yet, there are times in the history of scholarship when initially tentative explorations of a new field have led scholars to suggest profoundly new insights and applications of knowledge. No one, perhaps, can measure how frequently such efforts are successful, but they need to be made and, in a world of expensive science, supported. After all, until some astronomers and physicists read genetics, no one would have thought that information theory had anything to do with genes. It is an excellent example, I believe, of how surprising — and ultimately of how immensely important — a small interdisciplinary beginning can be.

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has published several science fiction stories. After a period of work and publication in animal behavior, he turned to anthropology and the study of human sexuality and courtship, which he has been studying for over a decade, and about which he has written a scholarly book and a number of papers. He has been Associate Editor and Book Review Editor of *The Journal of Sex Research*, the United States' oldest professional sexological journal, and has just finished co-editing a dictionary of sexual terms. Among other projects, he is currently working on a book of electronic music, a long-standing interest. With a background like that, what else can one be except an independent scholar?

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