

On Peer Review Systems

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To have a genuine peer group review as mythically practiced in science and scientific applications, at least two qualities must be met: (1) the reviewer must be competent (knowledgeable and up-to-date) in the field being reviewed; and (2) the reviewer must be genuinely objective with good heart.

Surely there are some men who are competent, objective and willing to praise — even when evidence points away from their own pet interests and experiences. And — while some who are competent and open-minded will be found on peer review boards — more than likely greater than 20% will consist of suppressive personalities, whose primary motivation will be to decrease stature of others' brilliant creations derived through honest sweat and some inspiration.

Let's look, for a moment, at the nature of traditional peer review boards, as reported by Gilbert Ling, Ph.D. (http://WWW.gilbertling.org/lp11.htm#sci_gro)

The Peer Review system

(by Gilbert Ling, Ph.D.)

Seeking advice from experts before making an important decision is common sense. Indeed it was said that when Christopher Columbus conceived of the idea of reaching China and Japan by sailing west, he submitted a proposal to Queen Isabella of Spain. She sought advice from a panel of three experienced navigators, who unanimously recommended rejection. Later she suddenly changed her mind—apparently prompted by the information provided by her keeper of privy purse that it would cost no more than giving a big party to finance Columbus's trip (see D.J.Boorstein, *The Discoverers*, Random House, p. 228). The New World was discovered in consequence.

Large scale peer review as the basis of government funding of scientific research took place after the Second World War. At the time, a common belief was that science made progress by a smooth incremental or cumulative process. Influential people like George Sarton (*The History of Science and the New Humanism*, Cambridge University Press, 1937), James B. Conant, and Lord Ernest Rutherford all believed and promoted such a view. That being the case, it seemed reasonable to expect that the advice from a panel of experts or peers is the most reliable way of correctly determining the merit of a piece of proposed research.

This once popular view of cumulative growth of science was later shown to be only partially correct and largely mistaken. Major upheavals or revolutions often took place inbetween periods of incremental and cumulative developments. Unfortunately, by the time the mistake was broadly recognized and a correct view adopted through the writings of Herbert Butterfield (*The Origin of Modern Science 1300-1800*, G. Bell and Sons, London, 1949), Rupert Hall (*The Scientific Revolution 1500-1800*, Longmans, Green and Co., London, 1954) and Thomas Kuhn (*The Structure of Scientific Revolution*, Univ. Chicago Press, Chicago, 1962), the peer review system was an established tradition in US government institutions and elsewhere. Worse, in more than one confused mind, peer review has replaced objective evidence as proof of truth.

The peer review systems adopted for government-sponsored research funding can be roughly sorted out into two types. One type was first adopted by the Office of Naval Research (ONR) and the National Science Foundation (NSF); the other type was that introduced by the National Institutes of Health (NIH).

1. The ONR-NSF system

This system places greater responsibility on the individual *program managers*. A researcher seeking support writes a proposal describing what he/she plans to do and submits it along with the endorsement from the Institution where he/she is employed. The specific program manager covering that field reads the proposal and sends it out to different experts in the field and seeks their opinions. Based on the opinions gathered, the program manager makes the final decision to fund the proposal or to reject it. The good side of this system is that it has flexibility. When the program manager is dedicated and has the needed knowledge, courage and integrity, the

system can work well.

The down side of the ONR-NSF system is that if the program manager does not have enough knowledge and self confidence, and/or cares little about the true objective of scientific research, he/she can do a great deal of damage to Science.

Thus in the words of Representative John B. Conlan from Arizona: “ (the system can turn into) an ‘Old Boy’s System’ where program managers rely on trusted friends in the academic community to review their proposals. These friends recommend their friends as reviewers....It is an incestuous buddy system that frequently stifles new ideas and scientific breakthroughs, while carving up the multimillion dollar Federal research and education pie in a monopoly game of grantsmanship...” (from Testimonial on July 22, 1975, as a member of the Subcommittee on Science, Research and Technology of the Committee on Science and Technology, U.S. House of Representatives, 94th Congress, First Session. Quoted from *National Science Foundation Peer Review, Special Oversight Hearings*, Publication No. 32, p. 5; U.S. Government Printing Office, Washington D.C.).

[The author of this article, Perry A. Chapdelaine, Sr., obtained nearly a half-million dollar grant in the 1970’s from the National Science Foundation (NSF) through the above described “good-old-boy” system. I first identified an individual at Stanford University who had obtained more NSF grants than any other, and who was already working in my field of interest. I visited that scientist, and convinced him of my computer background and desire. This, in turn, triggered off the “good-old-boy” network at NSF, and the grant was mine, tied in with on-going Stanford University research, within 90 days, surely more to support their “buddy” under the guise of my name than because of genuine interest in my proposal. The proposal to develop computer assisted instruction, written in great haste to meet NSF deadlines, was, in my opinion, unadulterated crap, consisting of several pages of my plans, and tons of “boiler plate,” furnished by the Stanford University scientist. It was sufficiently obtuse so that potentially enquiring Congressman would most likely not understand or criticize it. Although I was full-time assistant professor at two universities, one all White, the other virtually all Black, within two years, as senior project scientist at the all Black school, I recommended the grant’s cessation based on unwillingness of the university -- a “developing” institution -- to objectively implement the grant’s goals. “Developing” is an academic or grant-giver’s buzz-word usually understood to mean “under-par.”]

2.The NIH system.

(National Institute of Health) NIH is a much larger conglomerate of semi-independent Institutions (e.g., National Cancer Institute, National Heart Institute). Each has its own research laboratories and researchers, constituting what is called “intramural activities”. It is the “extramural activity” that oversees funding of research conducted by researchers outside NIH, often in the Universities. This funding is handled by a single NIH institution called the Division of Research Grants. Although there is a second level of peer review by a national advisory council made up by scientists and laymen, the major decision on the funding of research proposal is made by scientists sitting on the Initial Review Groups (IRG) also called Study Sessions. As of 1993, there were a total of 84 study sessions belonging to 16 NIH Institutes.

Research proposals, written by research grant applicants, are distributed by the Division of Research Grants to one of the Study Sessions for review. Of those proposals recommended for approval, each of the Study Section members gives (anonymously) a priority score ranging from 100 (highest) to 500 (lowest) and the average determines funding or non-funding. Sometimes a difference of a few points determines “life” or “death” of the proposal. Even though each Study Section member (which usually number between 10 and 20 but may be more) gives a numerical score, only two of the members read any one proposal. For this reason, there is no chance for a proposal to be funded, unless those two readers absolutely wish the proposal to be funded; they are the proposals’ only advocates. None of the other members really have the detailed knowledge of the proposal to challenge the opinions of the readers, not to mention that they must all cooperate with one another so that proposals read by other members of the Study Section can be accepted or rejected as recommended by their readers.

The power wielded by the two “readers” can be seen from an NIH-distributed document giving advice to grant applicants: “the author of a project proposal must learn all he can about those who will read his proposal and keep these readers in mind constantly as he writes.”Since single copies of this advice could be obtained from the Division of Research Grants, NIH (see linked page lp11a), it represents what is considered acceptable and perhaps even desirable by the NIH authority.

[Linked page lp11a says: An NIH Pamphlet: The National Institute of Health presented at a Special Session of the Federation of American Societies of Experimental Biology in April 1972 an article entitled: “The Project-grant Application of the National Institute of Health” which was later printed in Volume 32, pp. 1541-1542, 1973 issue of the *Federation Proceedings*. The author of the article was Dr. George N. Eaves, Executive Secretary of the Molecular Biology Study Section of the Division of Research Grant of NIH. To emphasize that what was presented in this article is not just the private opinion of Dr. Eaves but the official opinion of NIH, I mention that at the end of the printed article, there is an announcement to the effect that single copies of the article may be obtained from the Information Office, Division of Grants, National Institute of Health, Westwood Building, (Room 433), Bethesda, Maryland 20014. The following is the beginning of the part of the second paragraph in the first column of p. 1542:

“An investigator who is well informed about the peer review process has an advantage. Indeed as Peter Woodford has emphasized in his manual on the teachings of scientific writing, (1) the author of a project proposal must learn all he can about those who will read his proposal and keep those readers constantly in mind as he writes. . . .”]

There are two ways of looking at this advice. For the great majority of grant applicants working in an area of research where the foundation concepts have been firmly established and acceptable to all, this advice tells you about a system with which one might find fault but perhaps also live.

It is an altogether different story when the applicant works in an area of science where the foundation is far from being unequivocally established, as it is in the case of cell physiology. It is in this situation that the absolute, unchallengeable power given to the “readers” of the Study Sections, with their long four-year tenure, their high visibility, their *privileged tradition of recommending their own successors*, as well as *their own needs for the same NIH money for their own research now being disputed*, that the working of the system takes on a significance of truly alarming nature. It is here that the NIH peer review could fall into the same pitfall of generating an “incestuous buddy system” that “stifles new ideas and scientific breakthroughs” as Representative John B. Conlan had described in regard to the NSF system.

End of Gilbert Ling, Ph.D. article

I’ve long felt honored to be published in alternative medical journals either under the name of Perry A. Chapdelaine, Sr., or my pen-name of Anthony di Fabio. My background is certainly not medical, but rather as a former mathematician, systems developer, college professor, and Senior Project Scientist for the aforementioned NSF grant — although I started academic life as a desire to be a research chemist.

I’ve learned a great deal from the writings of others, and haven’t been a bit concerned about the fact that some of it is crap — indeed, some of mine has been crap.

Ninety Percent of Everything

The deceased science fiction writer and former friend, Theodore Sturgeon, was world-wide famous for both his stories and his quotation, that “Ninety percent of everything is crap!”

“Everything” -- he asserted -- 90% of created music, stories, plays, patents, movies . . . are crap!

Ninety percent of political policy, is crap, as is 90% of the laws our benevolent law-givers provide us!

This means that 90% of research in any given field is crap!

One only need review a given field, say, that of the history of discovery of microorganisms, and find numerous false trails, mis-identifications, and mis-conceptions, to realize how true most likely is Sturgeon’s saying. Indeed, one only need read a handful of scientific studies, peer-reviewed or otherwise -- especially in the medical field -- to conclude that a high proportionality of reported research is crap!

One would expect, therefore, that 90% of articles, whether “peer” reviewed or not, will be crap!

It is that other phosphorescent and creative 10% that brings us back again and again to publications such as the *MISHA* newsletter, and especially to grant-givers’ crap-differentiation machinery, the “peer” review.

How to Differentiate

The problem -- as editor or grant-giver -- is to differentiate between the phosphorescence and the crap, but now with a “peer review” process the editor/grant-giver has delegated part of his/her decision-making authority to a good-old-boy buddy system.

It's their right as editor or purse-strings-controller, of course, because they own or control the magazine or the funds, and work hard, along with others to administer it fairly. But I question that any traditional or even modified peer review system will bring about goals of producing inciteful, new articles or discoveries, except in rare almost accidental instances.

One monthly alternative medicine magazine received by us (The Arthritis Trust of America) has excellent double-blind studies which, taken singly or altogether, signify absolutely nothing!

Perhaps I'm pre-opinionated, or overly definitive, but "alternative medicine" to me has meant the searching out of causation, and the manipulating and controlling of the variables that lead to health. Whereas, these published multitude of well-done double-blind studies in this particular monthly magazine -- based on acceptance by "peers" in a traditional "peer-reviewed" setting (just like traditional medical journals) -- have virtually nothing to do with controlling causations, and everything to do with simplistically comparing one herbal remedy against another, or like levels of scientific abstraction, just as one non-steroidal anti-inflammatory drug is so often compared against another, thus resulting in no gain, so far as health is concerned.

This is not to imply that herbal medicine is anathema, but rather that the easy route has again been taken to examine symptoms and "alleviations" rather than determination of controlling causes.

My Test of A Particular "Peer" Review System

To solve the problem of screening out some of the 90% crap, one magazine, T.L., decided to require several "peer" letters from others' before publication was accepted. Apparently the writer could choose their own "peers."

I decided to "play-it-straight" as though I were young, naive and innocent, and really believed in this newly touted "peer" review system; that it was designed to find a qualified person to review our book report on Dr. Gilbert Ling's revolutionary work on cell physiology, and to get the "peer" reviewers' endorsements. What follows is a brief history of my search for an appropriate reviewer and a favorable review. I've removed correspondents' names, but otherwise what follows is an accurate report of my seeking a "peer-reviewed" consensus to satisfy this particular alternative medical publication.

Gilbert Ling's Scientific Lifetime Work

Sky David of Santa Fe, New Mexico, who formerly taught molecular biology at Harvard University, introduced me to a book by Gilbert Ling, Ph.D., titled *A Revolution in the Physiology of the Living Cell*. (Ling's latest expanded publication, is a beautifully complete and further extended book titled *Life at the Cell and Below Cell Level*, ISBN: 0-9707322-0-1)

This book, I felt, was an extremely important contribution to medical science, and should be known by a T.L.s readers. It should not have to be said — but I'll say it anyway — the knowledge of the true functioning of the living cell is basic to every health research project and treatment regimen known to mankind. There can be no more important basic medical/scientific knowledge than knowing that the Na/K pump hypothesis is faulty, and that Dr. Ling, through a lifetime of peer-reviewed reports based on laboratory experimentation, has demonstrated that this hypothesis is unsound, and that another hypothesis perhaps better predicts.

Indeed, Ling's Association-Induction hypothesis has already led to the invention and development of MRI (magnetic resonance imaging), and, through Sky David, may lead to a non-invasive Adenosine tri-phosphate (ATP) inductor that in vivo increases ATP, permitting the body cells to better function, and to restore health. ATP is a nucleotide compound occurring in all cells, fundamental to the energy cycle of each cell.

Besides authoring three technical books, publishing more than 200 traditionally peer-reviewed articles, and being manager, editor and publisher (along with Margaret Ochsenfield) of *Physiological Chemistry Physics and Medical NMR* (formerly *Physiological Chemistry and Physics*), others of great scientific integrity have had the following to say about Ling and his research reports:

"Your book (*A Physical Theory of the Living State*, Blaisdell, New York) strikes me as being one of the most important and advanced contributions to the understanding of the structure of living systems which I have seen during the last 10 or 20 years. . . ." [Prof. Lancelot Law Whyte, Cambridge University, Cambridge, England; Stanford University, California, U.S., 1963; author of *The Unitary Principle in Physics and Biology*, London, Cresset; New York, Holt, 1949.]

"Dr. Gilbert N. Ling played a central role in the development of the Ling-Gerard microelectrode. The microelectrode has subsequently proven to be one of the most important device[s] applied to the study of cellular physiology.." [NIH Summary Statement 1 R0 11 HL 39249-01, April 30, 1987].

“Dr. Gilbert Ling is a prominent cell physiologist and scientific investigator whose reputation is based on his imagination, skill as an experimenter, and prodigious scientific output...” [NIH Summary Statement 2 R01 GM11422-21, Feb/Mar, 1986]

“I am really impressed by your beautiful work...”[Prof. U. Kaatze, Drittes Physikalisches Institut der Universitaet Goettingen, Goettingen, F.R. Germany. Feb/March 1986].

“tour de force and really magnificent . . . my congratulations on a masterpiece. . . .” [Prof. Douglas Kell, The University College of Wales, Aberystwyth, United Kingdom].

“Your papers are the source of my inspiration. This year I’ve acquired the reputation of a ‘Linguist’” [Prof. Vladimir V. Matveev, Laboratory of Molecular Biophysics, Institute of Marine Biology, Academy of Sciences, U.S.S.R., Valdivostok, U.S.S.R.]

“You are if I may say so, one of the great original thinkers of today” [Prof. Harold Tillman, Department of Physiology, University of Surrey, Guildford Surrey, England].

“I am inspired by your sense of history and the thought that those who make new history are usually those who know history.” [Prof. Sidney W. Fox, Institute of Molecular and Cellular Evolution, University of Miami, Coral Gables, FL].

Applying The Newly Designed “Peer” Review Procedure

As for many years I’d worked in various applied statistical applications, and am quite familiar with mathematical statistics, I’ve long admired Dr. A.G.’s analyses of double-blind studies. His analyses are published in T.L. I think A.G. does a superb job, is a keen thinker, and provides an admirable service. He can spot a clinker in the experimental design or data, or in its conclusions, faster than most folks can react to 2-quinolinemethanethiol (skunk spray)

As T.L.’s guru of the scientific method A.G., surely, would be able to understand and comment favorably on my book review of *A Revolution in the Physiology of the Living Cell* by Gilbert Ling, Ph.D.

First, though, I had Dr. Gilbert Ling review my manuscript for accuracy, and permission, which he did most graciously.

Here’s A.G.’s response:

A.G.’s “Peer” Review

“As a reader and reviewer, I would say that the possibility of a new explanation for cellular function is intriguing. However, I thought the review was way too long and also too repetitive. I think you can state the hypothesis in one paragraph. I also do not think it appropriate in a review such as this to get into the usual paranoia about how screwed up orthodox scientists are, particularly since the book is about a *hypothesis*. What I would have considered much more interesting and important is some examples of the scientific data showing why the old theory is untenable, as well as some data supporting the new theory. Your statement that such evidence exists is not, unfortunately, convincing, particularly (as you acknowledge) the opinion comes from a layman.”

Acknowledging A.G.’s comments in a genuine concern for proper communication, I revised my book review, but could not comply with his desire that it simultaneously be shortened and also simultaneously provide scientific data demonstrating support of the Ling hypothesis. After all, Ling’s lifetime work of more than forty years was summarized in a book that I had just summarized further into a rather brief statement saying, in effect, “Lo! Ling has proved this.”

A.G.’s major point, well-taken, led me to include a very tight condensation of each and every scientific study that supported Ling’s Association-Induction hypothesis, and which clearly — taken together with a lifetime of laboratory experimentation — demolishes the traditional Na/K Pump hypothesis. This, necessarily, lengthened the book report, not shortened it!

I forwarded to Dr. A.G. the new report, and some of Dr. Ling’s original, published, peer-reviewed (traditional) scientific papers.

My follow-up letter to A.G. some weeks later did not elicit any further response.

D.L.’s “Peer” Review

Having received some good advice, but no blanket approval from A.G., I scratched my head to conceive of someone scientifically respected who would understand the extremely important medical implications of Ling’s work.

Dr. D.L. had just written an excellent book and he was editor of a want-to-be prestigious alternative medical journal.

With great respect, I tendered my A.G.-revised article to D.L. Here's his answer:

"It is an extremely erudite article in itself and is, I feel, way beyond my basic science knowledge. If Ling is right and membrane channels are a myth, then it is indeed a revolution. I do not have the expertise to follow your pre'cis of the book. In my view, it requires someone with the basic knowledge that is conversant with the science.

"I am aware that it is 'only' a book report, but it is so detailed that it has become a miniature exposure of the book itself. I am unable to judge, from your remarks, whether the book represents 'good' science or not. The membrane channels have been around for a long time and to trash them is a huge jump."

Wayne Martin, B.S. "Peer" Review

Well, fair enough!

According to A.G. the review must be shortened, but more illustrative of the scientific work itself; with D.L. the review has become a miniature exposure of the book itself, but beyond reach of the reviewer's expertise.

So who's next?

Well, Wayne Martin, B.S. wanted to be a doctor but during the great American depression years the best he could do was become a metallurgical chemist, and a very good one. He not only developed the chrome for car bumpers, but also the aluminum-magnesium alloys that helped to build our war-time and civilian airplanes and is still used in automobile engines. Over the years I've admired Mr. Martin's ability to dig into scientific/medical studies and come up with a new view, often a new synthesis. He is a frequent contributor to alternative medical publications.

Here's Mr. Martin's comments:

"I think that A.G. and I may have the same objection. I have taken the time to read this twice and it leaves me cold. It poses no problem of health and suggests no answer to a problem.

"I had to force myself to read it and to stay with it until I was through with it. . . . What you sent me was not junk. It is the very best of writing. It is just that it fails to attract my interest."

Peer "Peer" Review -- Gerald H. Pollack, Ph.D.

Again I say, fair enough!

The review, although an exposure of the book itself, contains too much detail that should be abbreviated while presenting more actual scientific data, and is also boring, leading to no new solutions to health problems. (Of course, development of the MRI, and possible *in vivo* non-invasive increase in cellular ATP are new solutions, but then, in fairness to all "peers" neither of these was mentioned in my book review.)

So far my test has batted 100%, which is to say that no one that I sincerely respect is willing to stick out their neck, and approve my book review!

I could get a dozen or so closer friends to act as "peers," but decided against it.

"So," I said to myself, "Why not try someone who does not know me or my educational background, who is genuinely a peer, that is, who is knowledgeable in the biological cellular field, and who also understands the huge medical implications of Ling's Association-Induction Hypothesis?"

I've never met Dr. Pollack, nor spoken with him, nor have I shared any other correspondence with him, except to request of him by letter a reading of my Ling book report, and to subsequently provide me with his opinion addressed to the proposed magazine of publication interest.

Gerald H. Pollack, Ph.D. is Professor of Bioengineering at University of Washington, Seattle, WA — certainly a "peer" of the first water. Here is what he had to say about my book review "Correcting an Inaccurate Paradigm on Cellular Functions":

"I am writing to endorse the review of Perry Chapdelaine. I have read through his review, and am familiar with the work of Gilbert Ling, whose book was reviewed.

"Ling's view is clearly in the minority. Most workers in the field ignore it, and some younger researchers are unaware of its existence. On the basis of "popularity" Ling's work would not be worth a second glance.

"However, if Ling is correct, all we know of the biology of the cell is wrong, and after having read and considered his work extensively I have come to the view that he *is* correct, essentially. I am not alone. If you go back to the old literature you find that the central concepts of Ling have been embraced by notables such as

Albert Szent-Gyorgyi, a Nobel Laureate and one of the leading scientific figures of this century. To this day, one finds that of those who have taken the time [to] read and understand Ling's work, most think he is correct.

"I might also mention that on a regular basis I take the trouble to expose my (rather bright) students to the "alternative" ideas expressed in Ling's books. I feel it is part of their education. Of those students who have taken the trouble to read through, not one doubts the reasonableness — or in most cases correctness — of Ling's ideas.

"So, in sum, Chapdelaine has come upon a seminal piece of work from a scientist who will ultimately be recognized as one of the pioneers of modern biology and medicine. Chapdelaine's review covers the territory well and should definitely be published."

The Amateur's Contribution

Virtually all giant leaps in thought have been produced by the amateur: The amateur is too little respected. Jesus wasn't a priest; Pasteur wasn't a doctor; Freud wasn't an M.D. or a psychologist. Until Galileo there was no astronomy, hence no peer astronomical review.

Not all amateurs are geniuses, but any genius is just an amateur seen from a point 20 years or more later in time.

The basic problem in peer evaluation seems to be the underlying assumption that all that is known and reasonable in a given field of thought will be known by those making the review. If it is too far beyond "what-is-already-known," then the submitter must be only an "amateur," or worse, a nut.

Yet scientific method demands that proof be submitted that can be subjected to open, honest, objective scrutiny amendable to verification or denial.

If one rules out the "amateur" at the very first, then how can one uncover that which truthfully shakes out old paradigms and unworkable hypotheses?

Gilbert Ling, Ph.D. — a long respected peer-reviewed scientist — is not exactly an amateur, unless his work is viewed from the perspective that no one else in the world has plowed his new furrow — that cells simply cannot and do not operate by means of little gates, called "pumps," and that, instead, they communicate primarily in other ways.

Einstein was only a patent examiner! This amateur shook the foundations of physics when most authorities had concluded that most all physics was known.

Gilbert Ling is only a research biologist — but his theories have already led to development of the MRI, and possibly a viable ATP inductor!

I ask you now, in all fairness: Does any "peer" review system do more than help to screen out some of the 90% crap?

Doesn't it also operate to screen out that brilliant, creative explosion, those alternatives so necessary to bring about a healthy revolution in health care?

Does it not also screen out the very product for which the peer-review system was designed?

Incidentally, to the credit of the publication where a slightly different version of this article was sent to its editor as a personal letter, not to be published, both of Gilbert Ling's book reviews were subsequently published despite submission of the above reviews, both positive and negative.

These reviews were "Correcting an Inaccurate Paradigm on Cellular Functions" regarding *A Revolution in the Physiology of the Living Cell*, (Krieger Publishing Company, Malabar, Florida, USA; ISBN 0-89464-398-3; 378 pages, hardcover, 1992, \$64.50) and *Life at the Cell and Below-Cell Level: The Hidden History of a Fundamental Revolution in Biology*, (Pacific Press, NY, PO Box 1453-C, Melville, NY 11747; 373 hp paperback, \$19.95 ISBN: 0-9707322-0-1) both submitted by Perry A. Chapdelaine, Sr., M.A. and Perry A. Chapdelaine, Jr., M.D., M.PH.