theory is not well represented in our top 20 institutions, as is documented in the 1987 Armstrong report to the National Academy of Sciences on the state of theoretical atomic, molecular and optical sciences in the United States. And we are few in number: The Armstrong report cites 1 theorist to every 6 experimentalists in atomic, molecular and optical physics, compared with a ratio of 1:2 in all fields of physics. This will, of course, change. A new position approved at Harvard and the Smithsonian Center for Astrophysics is a beginning. But a more detailed follow-up to the 1988 COP survey would also help.

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2/89

Can One Rely on the PI for Quality Control?

In the May 1988 issue (page 78) Louis J. Lanzerotti and Jeffrey D. Rosendhal wrote about issues facing the US space research program. They suggested that significant cost savings would result from reducing quality requirements and delegating responsibility to the highly motivated principal investigators.

As part of university contract administration functions, the Office of Naval Research maintains Engineering Support Offices. These have provided quality assurance monitoring and assistance on many delegated NASA instrument contracts. We support cost saving efforts, but have a different view of the quality and reliability situation.

There is no question that the principal investigator will be highly motivated to have a productive flight. A great body of knowledge on what is needed to produce a quality product has been developed over many years by universities, industry, the Department of Defense, NASA and others. We have found, however, that PIs not previously involved in space research efforts are not familiar with the detailed methods of quality assurance for flight hardware and software. The processes of this discipline were certainly not taught in our physics classes!

The quality requirements imposed by NASA and actually met by most universities are simply not extensive, especially when compared with those imposed on DOD contracts with industry. We find also that the amount spent on quality is not directly related to the quality of the produced instrument. What seems to be most important is the emphasis and support for a

quality instrument exhibited by the PI, the program manager and the university itself.

There are other ways to lower costs than simply reducing the quality requirements on a given contract. One good example is provided by New Mexico State University's Physical Sciences Laboratory, which maintains a small quality assurance office to give assistance to PIs. This approach provides experienced quality support while reducing the amount each PI has to spend. At many universities PIs are pretty much on their own in determining how to produce a quality instrument and meet contract requirements.

We very much want to see the US space research effort succeed, and we believe that it will. We would, however, discourage the argument that space science should be conducted with reduced quality requirements. The recent double Soviet Mars probe failures demonstrate again some of the problems that can occur when quality and reliability are not properly considered.

The views presented here are solely our own and do not reflect any official Navy position.

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5/89

LANZEROTTI AND ROSENDHAL REPLY: David Cissell and Doug Scoggins appear to have misinterpreted the intent of our remarks concerning quality control of spaceflight instrumentation for research purposes. There is a vast difference between the quality of the final product and the generation of extensive and expensive paperwork and documentation to certify that quality. Having the principal responsibility for quality reside with the PI would not exclude alternative ways for achieving quality, such as the use of the cited center at New Mexico State University. We also stated clearly that a possible decrease in reliability requirements "for some types of science missions" might be economical in the long run, but that such a decision would have to be evaluated in individual cases in terms of cost tradeoffs for servicing or replacement. We want to emphasize that the issue we were addressing was not of needlessly or mindlessly compromising quality, but of achieving the right balance between quality control and expense at a time when space research costs seem to be escalating dramatically.

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6/89

Darwin–Fowler: The Natural Selection?

The presentation of the Gibbs-method derivation of the Fermi–Dirac distribution offered in each of the references given by Charles Kittel in his March letter (page 154) is no less muddled in mystery than what one finds in the usual textbook uncovery of that distribution. While I agree with Kittel that the Darwin–Fowler approach employs, in the hands of Charles Galton Darwin and Ralph Howard Fowler, an "incredibly complex apparatus," I'm convinced that theirs is by far the conceptually simplest formulation of the statistical mechanics of systems of weakly interacting particles.

Because of the value of this conceptual simplicity, PHYSICS TODAY readers should be informed that there exists a rigorous and mathematically elementary, although somewhat intricate, execution of the Darwin–Fowler approach that is accessible to any serious beginning graduate student in physics.[1] It uses methods of undergraduate multivariate real-variable calculus that successfully bypass the formidable saddle-point method applied by Darwin and Fowler to multiple Cauchy integrals over several complex variables. I shall be glad to mail to anyone who requests it a copy of the paper that expounds this elementary execution. (Reprint supplies were twice exhausted in response to 201 requests by mail in the late 1960s and early 1970s.)

In 1965 or '66 Rudolf Peierls told me that the reason Darwin and Fowler produced their work was their inability to understand Gibbs—precisely my problem!

Reference

1. R. Weinstock, Am. J. Phys. **35**, 710 (1967).

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3/89

A Russian Remedy for Overpublication

We cannot cure the disease of the overabundance of published information so long as "we write not just to inform but to survive," as Laurence Passell wrote in the November 1988 issue (page 149). However, there is a symptomatic medicine, one that has already been tested for many years in the USSR. It is the practice of "shelved" (*deponirovannih*) publications.

The idea is simple: The reviewers of a respectable journal suggest that a paper is important, but not important enough to be published in full. So the journal in a special section publishes an extended summary containing enough information that the reader can understand the methods and results without reading the complete text. This summary, in contracted form, also appears in abstracts journals. The complete text is "shelved" in a kind of database, from which it may be retrieved by interested researchers.

The number of retrievers is a measure of the significance of the paper and the correctness or error of the reviewers' judgment. This number, together with the number of citations by other researchers, is probably a better indicator of the quality of an author's work than is the total number of publications.

You could receive the number and probably the names of previous retrievers automatically, together with the paper or separately. (In Russia one goes to a special library for this purpose.)

We could certainly use modern CD-ROM-based databases to store the information on a host computer, and telecommunications technology to retrieve papers while sitting at our home computers.

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12/88

Refereeing Rhyme and Unreason

A recent conversation with a friend on the difficulties that a paper (not one of ours) was having in getting accepted by *Physical Review Letters* led to the spontaneous creation of the following poem:

There once was a fine referee
Who reviewed each paper with glee.
What's new is not true!
What's true is not new!
Unless it's been published by me.

The speed with which the poem was written suggests that it may not be new. I defer to the readership of PHYSICS TODAY on whether or not it is