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.

DAVID CISSELL
ONR Engineering Support Office
Pasadena, California
DOUG SCOGGINS
ONR Engineering Support Office
Richardson, Texas
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.

LOUIS J. LANZEROTTI
AT&T Bell Laboratories
Murray Hill, New Jersey
JEFFREY D. ROSENDHAL
NASA Headquarters
Washington, DC
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).

ROBERT WEINSTOCK
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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.

VICTOR R. BLOK
University of Maryland, Baltimore County
Catonsville, Maryland
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

true, and to the editor on whether or not it should be published.

GEORGE D. CODY
Princeton, New Jersey

3/89

Corrections

October, page 17—Because the size of a high-energy electron storage ring must scale approximately as the square of the beam energy, a TeV e^+e^- ring would have to have a circumference exceeding 2000 km—not 130 km, as stated.

October, page 36—The colloquium at which Irving Langmuir presented his lecture on "pathological science" was held in 1953 at the General Electric Research Laboratory in Schenectady, New York—not at the Knolls Atomic Power Laboratory. ■

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I certify that the statements made by me above are correct and complete.

Arthur T. Bent, Treasurer