

than the random errors! It depends on the power spectrum of the errors. For a white power spectrum, as for shot noise, the low-pass filtering action of a moving average reduces the noise power in proportion to the bandwidth, and so the root-mean-square noise decreases in proportion to the square root of the bandwidth reduction. Those systematic errors that were referred have their power spectra concentrated near dc and so do not get reduced by low-pass filtering. On the other hand, systematic errors, particularly in the case of quantization noise, can sometimes be concentrated deliberately up near the Nyquist frequency and so become almost completely excluded by low-pass filtering. This opportunity has been known for a long time. The introduction of ordered dither of the signal with respect to quantization levels, whether it be accomplished open-loop or by closed-loop feedback, as with delta-sigma data converters,¹ does the trick.

A rare counterexample to Murphy's law led to my awareness of the possibility. My measurements with a sensitive tiltmeter² looked to be much cleaner than expected. After publication I found that laser intensity ripple coupled with a small imbalance of the three-port homodyne mixer to give the dither by sheer accident. A check of the noise power spectrum showed that the noise was mainly near the Nyquist frequency, so that subsequent filtering removed most of it. The result was that the noise became reduced by much more than the square root of the bandwidth reduction and ended up probably less than a picoradian at a kilohertz bandwidth, close to the shot noise limit.

Simple examples are Wilkinson (single-slope) and successive-approximation analog-digital converters, where the resolving powers increase linearly and exponentially with bandwidth, respectively. More incisive examples are the oversampling converters used in audio compact discs. The physics community could profitably exploit the vastly improved trade-off relationships to reach the very sensitive measurements sought by LIGO, the Laser Interferometer Gravitational Wave Observatory.

References

1. See J. C. Candy, G. C. Temes, *Oversampling Delta-Sigma Data Converters*, IEEE P., New York (1992), for an extensive review.
2. L. N. Mertz, *Rev. Sci. Instrum.* **62**, 1356 (1991).

LAWRENCE N. MERTZ
Palo Alto, California

7/92

The quip by Daniel Kleppner's friend about the seductive perils of statistical analysis brings to mind the cautionary words of Ernest Rutherford: "If an experiment requires statistical analysis to establish a result, then one should do a better experiment."

RICHARD PETRASSO
Massachusetts Institute of Technology
7/92 Cambridge, Massachusetts

Antenna Array Amount Amendment

We very much regret that in our article "The Search for Forming Planetary Systems" (April, page 22), the number of antennas planned for the Berkeley-Illinois-Maryland array at Hat Creek in the California Cascade Mountains was incorrect. The relevant sentence should have stated that within a year BIMA will have nine 6-meter telescopes.

ANNEILA I. SARGENT
California Institute of Technology
Pasadena, California
STEVEN V. W. BECKWITH
Max Planck Institute for Astronomy
5/93 Heidelberg, Germany

DOD Acting Research Director's Past Actions

I appreciate the complimentary write-up by my good friend Irwin Goodwin of my appointment as acting director of research and laboratory management at the Department of Defense (October 1992, page 108). My mother would have loved it. Permit me to make just two corrections. First, I could never have turned out the three Defense Critical Technologies Plans "virtually single-handedly": They were truly a team effort by many dedicated scientists and engineers at DOD, and I was fortunate to have had their support and cooperation. Second, as to my future responsibilities, they are unknown. I shall endeavor to serve in whatever capacity I can be most useful in bringing science and technology to the service of my country.

LEO YOUNG
Department of Defense
10/92 Washington, DC

Must Scientists Help Define a 'Better World'?

In his Opinion column "Physicists in the 'Age of Diminished Expecta-

tions'" (March 1992, page 61), Arthur Kantrowitz demonstrates trust in the progress offered through modern physics and encourages the scientific community to seek ways in which it might "restore our faith in the potential of science-based technology" while helping us resist those who seek a "risk-free," more cautious society.

We need continued technological advances, especially when they promise potential solutions to societal needs, but the seriousness of the problems that technology creates are today of equal concern. Kantrowitz worries about the decline of American productivity and raises the question, "How can physicists help in restoring the hope . . . of Americans that their children would live in a better world?" but he fails to consider what is meant by the idea of a better world, and that there are competing visions of what that world may look like. Technology and the national economy are not the only dimensions in which human progress is properly measured. Yet rather than asking physicists to consider issues of socioethical import, of what true progress for ourselves and our world might be, Kantrowitz demands that physicists do a better PR job within the growing competition "for control of the public perception of scientific findings." Surely the *a priori* question is, What are the reasons for the loss of confidence in science and technology?

Why is it that today more diseases are curable and more lives saved, and yet a steady erosion of trust in MDs continues? Doctors have been trained to be objective technicians without training in compassion and care. Placebo tests demonstrate the place of nurture in effective healing, and enough alienated voices demonstrate the need for a change in medical training, yet our trust in technology to the exclusion of wider human values and needs continues.

Are we to continue, too, with the assumption that everything our technology creates will be for the good? Or, if anything perilous is developed, that the peril will yield to further technological solutions? Surely our hope for a better society needs to be based on a vision of the good rather than on the narrow ideal of technological progress. The idea of an objective and amoral science's developing complex technologies while leaving instrumental decisions in other hands is Orwellian. The genius of technology is that it can be used to create or destroy, and its power is now so great that we cannot but ask ethical questions of its advance. This is not to lay responsibility solely at the scientists'

door but to ask for interactive dialogue on how we want to shape our world and what are the most pressing needs we face as a global society.

Is it possible to articulate a vision of the good so that we may better determine the directions our experimentation takes? For science will continue to follow the powerful visions of the society that it serves. We determine directions all the time, but the vision is usually fixed by the highest bidder, not the greatest good. It is therefore no longer safe for science to be completely unchained from questions of ethics and virtue and for progress to be its only yardstick. For our society is groaning, not under the straitjacket of a medieval religious authority (to which Kantrowitz alludes) but under an increasing burden of technological materialism that we hardly know what to do with, from imagination-dimming Nintendo to the complexity of economic collapse in a post-cold-war world in single-company cities too dependent on manufacturing weapons systems.

I am not arguing for science once again to be the chained servant of wider ideologies, but rather the opposite, for science has become the willing bond servant of politics, economics and nationalism, which do not concern themselves with the universals that should concern scientists and ethicists.

So, what kind of risks should we take? Risks are for gain, but we must differentiate among the kinds of gain. Risks can be for improving the quality of society and for relief of suffering. They can also be for nationalist gain or personal and institutional prestige. Nobel laureate Roald Hoffmann writes: "In this century science and technology have transformed the world. What we have added, mostly for the best of reasons, is in danger of modifying qualitatively the great cycles of the planet."¹ Taking a calculated and brave risk is one thing. Failing to foresee overwhelming danger is quite another. Kantrowitz fails to point out the difference. And it is the difference between hope and despair.

Stanley Hauerwas, an ethicist at Duke University, suggests that "the ethical problem is how to be joined to the Good without illusion . . . for right action and freedom are possible only on the basis of our prior attention to the Good."²

We must ask the question, What would a better world actually look like? and not assume a straightforward trajectory based on what has been done in the past. Our technological legacy is every bit as morally ambiguous as every other field of

human endeavor, including politics, economics and, yes, religion, all of which fail miserably when not imbued with a moral vision of a good society.

It is therefore my hope that the scientific community will take steps to tackle ethical questions in the larger context of shared concerns with others attempting to articulate a better vision for our society and our world. Only then will we be able to meet Kantrowitz's closing challenge: "to shed light on the 'invisible' evils of the late 20th century."

References

1. R. Hoffmann, *Cornell Alumni News*, December 1991, p. 43.
2. S. Hauerwas, *Vision and Virtue*, U. Notre Dame P., Notre Dame, Ind. (1974).

ROGER A. BADHAM
*Cornell United Religious Work
Cornell University
Ithaca, New York*

1/93

KANTROWITZ REPLIES: Roger A. Badham appeals to the scientific community to join with others in tackling ethical questions. He asks that scientists and ethicists "concern themselves with the universals."

Whenever, in responding to this persistent request, science gives any support to the pretension that its achievements are miracles attesting to higher wisdom, then that fraud invalidates the factual output that a rational society requires from science. If in addressing the "universals," we abandon science's essential restriction to falsifiable statements, then our self-policing methodology will be incapacitated and scientists' "vision of the good" will have no special validity.

It was of course the thrust of my Opinion column that science should do more to communicate its knowledge and especially its ignorance to the public when that information is important in the making of public policy. In describing this thrust as a demand for "a better PR job," Badham apparently seeks to belittle the role of science in providing public information essential to democratic control of technology.

Risk management provides a good example. Repeated raising of false fears has increased human suffering (for example, by delaying the progress of medicine and agriculture). Badham's allusion to "failing to foresee overwhelming danger" without providing a single example is an unfalsifiable statement clearly motivated by his "vision of the good." Risk assessment needs all the information science can provide. Making an effort to

protect that information from the confusion created by unfalsifiable statements will improve democratic risk management and reduce human suffering.

ARTHUR KANTROWITZ
*Dartmouth College
Hanover, New Hampshire*

4/92

An Ethics Exam for Physicists Everywhere

I am delighted by the new APS guidelines for professional conduct (see *PHYSICS TODAY*, January 1992, page 62) and I thank the responsible committee very much for the effort!

Questions of ethics are best treated by example, and a public discussion of ethical issues of our profession would surely increase the awareness of the physics community. The following are some issues I would like to see discussed:

▷ The relationship between junior and senior coworkers: For example, when can a senior author's name be on a paper by a junior student or postdoc? When the senior person is supplying the money? The initial working direction? The experimental setup? Or does it have to be more?

▷ What importance should be placed on seniority when it comes to giving grants? If a senior worker has hundreds of papers on his or her resumé, and a junior person ten, when should the junior person get the grant? I have heard that about 80% of the grants in materials research are given to people over 40. Is this the best way to do it?

▷ No matter where he or she is on the author list, the senior person is the one most often quoted as responsible for a paper, presumably because of name recognition. Is this professional?

▷ Can a referee recommend rejecting a paper for a "grayish" reason? Say the author has a theoretical model: Can the referee reject it because the author has not shown that the model agrees with *all* experimental data? Can one reject it because it is not "important" enough, not "novel" enough?

▷ I noticed with delight that the APS guidelines ask for peer reviewers to disclose conflicts of interest, such as being a direct competitor of the person whose work one is reviewing. I bet it is almost universal, however, that one is reviewed by a direct competitor. What should be done in this case?

EUGEN TARNOW
*Los Alamos National Laboratory
Los Alamos, New Mexico*

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