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of quantum electrodynamic confinement in microlaser physics. In fact Gloria R. Jacobovitz and I published the first proposal of the optical microlaser, the related quantum theory and its relevant properties in 1988, together with a report on the very first experimental realization of the device, in a paper with the title "Anomalous Spontaneous–Stimulated-Decay Phase Transition and Zero-Threshold Laser Action in a Microscopic Cavity."[1] That work followed two earlier papers reporting the first QED confinement effect on spontaneous emission at an optical wavelength λ, in a planar Fabry–Perot cavity of size $\lambda/2$ confined by semiconductor multilayered mirrors.[2] Interestingly enough, among the today widely advertised "photon bandgap" structures, only the Fabry–Perot geometry and the recent ones reported by PHYSICS TODAY (the droplet and the microdisk of Samuel McCall and Richart Slusher) have so far provided laser action.

From a structural viewpoint, the difference between the modern semiconductor Fabry–Perot microlaser and the one we reported in 1988 consists essentially of the replacement of the original molecular medium by an active quantum well. Apart from such technological considerations, it is certain that the nontrivial and highly unexpected properties of the vacuum-confined microlaser, whatever its structure and shape, have their origin in the introduction of new, fundamental quantum theoretical conceptions within the framework of laser physics and of statistical mechanics. The relevance of the reduction of the dimensionality of the statistical mode reservoir down to a single mode, caused by a reduction of the cavity size, within the quantum dynamics of any physical system undergoing a phase transition appears not to have been adequately considered in the past; certainly this concept was new in laser physics when we introduced it[1] in 1988. In that context this physical effect leads precisely to the striking "thresholdless," high-gain behavior of the microlaser, which we also demonstrated experimentally in reference 1, and which is now correctly emphasized by the PHYSICS TODAY report.

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Physicists' Statistical Biases Evaluated

In his Reference Frame column in the July 1992 issue (page 9), Daniel Kleppner encourages physicists to be skeptical about statistical analysis. Clearly, physicists should be skeptical about all scientific investigation—not just statistical but also numerical, asymptotic, phenomenological and *physical*. Statistics, like any other analysis method, can be misused. However, when used effectively, statistics can and has significantly enhanced experimentalists' ability to resolve the signals from the noise and to estimate the size of the uncertainty as well.

All too often, opportunities are lost because experimentalists are unaware of the appropriate statistical methods. Unfortunately, Kleppner's essay discourages physicists from learning and using more sophisticated analysis methods. When physicists are better educated in statistics, they will be able to evaluate the merits of a particular data analysis rather than relying on blanket skepticism.

Kleppner's essay contains several technical misnomers. First, he considers an experiment where the empirical fit residual squared error is Δ. Kleppner assumes that Δ is less than the *a priori* estimate of the experimental error based on known error sources (which I denote by σ^2). Kleppner then asserts that the actual experimental uncertainty is Δ and not Δ/N, where N is the number of points. ("Uncertainty" refers to the expected squared error in the inferred parameter.) However, a more reasonable analysis of the uncertainty divides the residual fit error into a random part and a bias part due to systematic error. We can estimate the bias squared as the difference between the experimental residual variance and the variance due to known sources of random error: $(\text{bias})^2 \sim \Delta - \sigma^2$. Having N observations decreases the variance to σ^2/N while not altering the bias. Thus the total uncertainty satisfies "uncertainty"$\leqslant \Delta - \sigma^2 + \sigma^2/N$. Alternatively, the bias may be zero, and the actual variance may be larger as a result of unknown sources of random error. Thus we have the lower bound: $\Delta/N \leqslant$ "uncertainty."

By examining the distribution of residual fit errors, it is often possible to clarify the extent to which bias errors contribute to the residual error. More sophisticated versions of this analysis of variance have been used to predict the uncertainty associated with extrapolating experimental performance to the next generation of fusion devices.[1]

A common oversight occurs in Kleppner's story of the illusionary peak in the data set of his youth. If the resonance frequency is unknown and if many different frequencies are examined, then the probability of finding a large peak due to statistical noise is much higher. Let p be the probability that an experimental measurement exceeds a certain threshold due to random noise. The probability that at least one of K independent measurements exceeds the threshold is $1-(1-p)^K$. Thus for large K, the probability of detecting a false peak using the single test statistic is quite high. I conjecture that Kleppner may have used the statistical uncertainty for a single known resonance frequency when in reality the frequency was unknown.

I mention these examples only to show that even an illustrious physicist such as Kleppner could benefit from more statistical training. The typical training of physicists is almost devoid of statistical analysis. As a result, experimentalists often miss details that could have been seen with more sophisticated statistics. Additional time and money are expended to buy resolution that would be unnecessary if better statistical methods were used.

I believe that the APS as a society needs to recognize that poor statistical training is one of our greatest weaknesses. I hope that in the near future the APS can encourage interdisciplinary efforts to advance the level of signal processing and statistics in physics. To this end, I would like to hear from other interested physicists who specialize in advanced statistics and signal processing.

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The cautionary admonitions in Daniel Kleppner's "Fretting about Statistics" may be too discouraging and warrant redress. Sometimes the systematic errors go away even faster 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 tiltmeter[2] 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 tradeoff relationships to reach the very sensitive measurements sought by LIGO, the Laser Interferometer Gravitational Wave Observatory.

References

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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."

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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.

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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.

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Must Scientists Help Define a 'Better World'?

In his Opinion column "Physicists in the 'Age of Diminished Expectations' " (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'