important new work.

15 January 1497, royal referee report on Columbus's paper "The Discovery of the Indies, Ophir or Cipangu": *I cannot recommend this paper for publication. First, the discovery was made long before Columbus. Second, he is mistaken in his claim that what he discovered is the Indies. Third, in our time such a discovery is trivial and of little general interest.*

18 June 1500, letter from Columbus to Editor-in-Chief, Royal Society: *Bad, irresponsible referees are the single most hazardous thing to any explorer. They are far more dangerous than hurricanes, which can be handled by a skillful mariner. Who can estimate the psychological damage from a direct hit and humiliation by anonymous arrogance, with little hope for future satisfaction? The most endangered papers are the most innovative. They are the most demanding of the referees' time and efforts, and quite often a referee prefers to suggest, "Reject it" rather than admit, "I didn't have the time to think it over." The first solution to a problem is seldom transparent and easy to understand. To substantiate the rejection, a referee may not care to read even the abstract but goes straight to section 2 to look for minor faults. As a result, some great explorers, who can live without* RRL *credit, have decided never to submit their papers to* RRL. *But what about the famous-to-be?*

Our students rate their professors in anonymous questionnaires. Our authors should rate referees! Every referee report should be accompanied by a questionnaire from the journal. Referees must remain anonymous, but they must not remain unimpeachable. Some should be "fired." The explorers' community must protect itself against these anonymous killers.

30 October 1500, letter from Editor-in-Chief, Royal Society, to Columbus: *Thanks very much for your thoughtful letter of 18 June. I can hardly disagree with your comments; as one who has published maybe 100 papers myself in* Royal Rev., *I guess I have screamed some 25% of the time! And when it comes to* RRL, *I have also joined the coterie of "some great explorers" who refuse to send them papers—even after assuming my present exalted position!*

If you have any substantive ideas about how we might actually restructure our operations—within the constraints of our current budgets and huge submission rate—I would be delighted to know them. Nothing we do is at all perfect or graven in stone. I just want to see that we publish the world's best exploration journals.

15 November 1500, letter from Columbus to Editor-in-Chief, Royal Society: *Thank you very much for your letter of 30 October. I believe the most reluctant referees are established explorers with enormous demands on their time. I also believe that the most productive and creative age is the one at which an explorer is desperately short of money. Hence the recipe: Pay the referees generously, and choose them from the youngest and the most ambitious adventurers. They will learn a lot; they will be responsible (lest they lose the income!); they will be careful, when in doubt, to discuss the paper with and to turn for advice to their senior colleagues, thus involving them in real (their students are there to judge them!) refereeing. This will save for the doing of true science at least 30% of the time that is now wasted in an uphill battle for the survival of one's papers. And since every single paper is worth well over 1000 florins, if referees get 200 florins total (a fortune for young ones), 100 extra florins will still be saved for research! Those 200 florins may be the best investment of all publication-related expenses. They also will benefit those who need them most—on both ends, author and referee. The money must be overhead on all research grants and funding.*

This last letter was never answered or acknowledged. Columbus died, ultimately rejected. America was named after Amerigo Vespucci, not after Columbus. It is amazing that almost five centuries later, in 1936, a certain Albert Einstein was so outraged by the refereeing at *Physical Review* that he stopped publishing his papers there.[1] It is even more amazing that nobody cared, and little changed.

Reference

1. A. Einstein, letter to J. T. Tate, 27 July 1936, in A. Pais, *"Subtle is the Lord . . .": The Science and Life of Albert Einstein*, Oxford U. P., New York (1982).

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11/92

Early Glimmerings of Optical Microcavities

I was surprised, and amused too, to find, on reading the brilliant report by Barbara G. Levi on semiconductor microlasers (September 1992, page 17), no reference to the contribution of the Quantum Optics Laboratory of the University of Rome to the field

continued on page 106

continued from page 15

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.

References

1. F. De Martini, G. R. Jacobovitz, Phys. Rev. Lett. **60**, 1711 (1988).
2. F. De Martini, G. Innocenti, in *Quantum Optics IV*, J. D. Harvey, D. F. Walls, eds., Springer-Verlag, New York (1986), p. 188. F. De Martini, G. Innocenti, P. Mataloni, G. R. Jacobovitz, Phys. Rev. Lett. **59**, 2955 (1987).

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10/92

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.

Reference

1. K. S. Riedel, S. M. Kaye, Nucl. Fusion **30**, 731 (1990).

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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