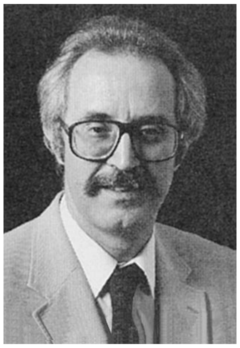
EDITORIAL



Edward R. Dougherty
Editor

A popular topic at conferences is the need for sustained, far-sighted research thrusts. Perhaps it's the prejudice of my age and the (often similar) ages of the colleagues with whom I dine, but there appears to be growing concern over increasing pressure to abandon the kind of laborious, long-term effort required for deep scientific understanding in favor of short-term projects that produce quick results. If I were the only one sensitive to these changes, then I might attribute my disquiet to individual perception. But, to the contrary, concern is raised by researchers from across the globe. Therefore, I will postulate pressure to abandon fundamental research as fact and offer a few comments. I will stay within the confines of imaging and, even more particular, address an area in which I am personally involved. I have no doubt that similar arguments apply to other areas of engineering and science, and to many specialties within each area.

In recent years the number of researchers applying automatic design of imaging algorithms has grown substantially. One need only consider the related areas of computational learning, neural networks, distribution-based statistical optimization, and AI-based heuristics. The ascendancy of automated design techniques reflects the extreme diversity of imaging environments, wide variability in image data, and lack of algorithm robustness resulting from significant nonstationarity even in apparently simple problems. Lack of robustness is critical because it means that algorithms are *ipso facto* application-dependent. Hence, algorithm training must proceed on carefully defined sample and synthetic data. Especially important is the need for relevant image models.

Because we need algorithm learning, we need to understand learning. It is not sufficient to pick learning and image models off the shelf and try them willy-nilly on some haphazardly obtained data. One need only read a book on classical experimental design to recognize the pitfalls of *ad hoc* approaches to statistical estimation and decision. Since the new methods represent more sophisticated methods of estimation and decision, it is reasonable to conclude that their application requires at least as careful modeling and design as classical techniques. It is also reasonable to presume that development of good sampling, modeling, and training techniques will require far more effort than similar developments in the past, and that understanding will only come through small increments sustained by concentrated effort over many years. It is doubtful that much can be accomplished by jumping from one set of problems to another, or by a graduate student who just barely acquires appreciation of a problem and then changes research direction on completion of the PhD. On the contrary, it will take insight gained over many years of concentrated effort to expand our understanding.

It needs to be recognized that image processing is a very hard subject. Mathematically, it sits within multidimensional random processes. This recognition alone indicates how difficult it will be to achieve significant results. Most problems are inherently nonlinear and involve spatial statistics. Even when a problem can be posed in a mathematical framework, such as minimization of an integral, closed-form solution is usually out of the question and iterative numerical techniques can be computationally imposing and lack convergence criteria. Crunching raw image data to form probabilistic estimates is out of the question except for very small windows involving low-bit images.

To progress to the point where we can deal with the kind of problems confronting us in tough areas such as biomedical imaging and robot vision, we need an enormous increase in knowledge concerning training and estimation in the design areas related to imaging algorithms. A significant increase in knowledge can only come with a concomitant increase in research aimed at fundamental understanding. We are not confronted by a conflict between theoretical and applied research, as some would contend. Application cannot proceed without the prerequisite scientific and mathematical tools. The tool kit for image processing is too small. And it can only be enlarged by a concerted and sustained effort.

My conjecture is that there are many areas of technology that suffer from too small a tool kit. It is the nature of technology to move into new areas as older areas become exhausted after having generated new demands. I have no doubt that many of those calling for less fundamental research would have called into question the pragmatism of Kolmogorov's work on optimal linear filters, Dirac's use of generalized functions, Lebesgue's theory of integration, or, more recently, Shannon's information theory, Daubechies' work in wavelets, or Matheron's development of random sets. But where would image processing be without these breakthroughs? I am not claiming that many of us will make equally significant contributions, but I do claim that these major contributions did not take place in a vacuum. They took place in the framework and with the supporting contributions of an active research community dedicated to pushing the frontiers of scientific/technological knowledge.