Editors' Corner-Vol.4

The July-August issue of FERMAT contains a wide variety of contributions that come in many different flavors.

ARTICLES

In the Articles section you will find:

- Design of Large Finite Arrays Using Simulations or Measurements of Small Arrays, by Ahmed A. Kishk
- On the Superposition and Elastic Recoil of Electromagnetic Waves, by Hans G. Schantz
- Optimization Techniques for Electromagnetic Design with Application to Loaded Antenna and Arrays, by Simone Genovesi, Davide Bianchi, Agostino Monorchio, and Raj Mittra
- Volumetric Method of Moments (V-MoM) in Modelling Nanotopologies: A Review, by X. Zheng, V. K. Valev, N. Verellen, V. Volskiy, P. Van Dorpe, G. A. E. Vandenbosch and V. V. Moshchalkov

MULTIMEDIA

The Multimedia Section of this issue features 4 contributions covering a wide spectrum. They are:

- Quality Factor For Antennas (A Tutorial), by Arthur D. Yaghjian, Mats Gustafsson, B. Lars G. Jonsson
- Printed Multi-Band MIMO Antenna Systems: Techniques and Isolation Mechanisms, by Mohammad S. Sharawi
- Weather Radar Dual Polarization Technology And Penn State's Ionosphere Research Laboratory, by Thomas A. Seliga
- Measurement Setups for Millimeter-Wave Antennas at 60/140/270 GHz Bands, by Xianming QING, Zhi Ning CHEN

NEWS & VIEWS

Moving on to the News & Views section, this issue of FERMAT salutes one of the giants ––none other than Roger F. Harrington. Here you will also find:

- How it all began, and a walk-through its history—ElectroScience Laboratory-The Ohio State University, by John L. Volakis
- Women in Engineering: Hélène Roussel 's Autobiography
- Comprehensive Electromagnetic Solutions—FEKO

FORUM

Next, in the Forum section, you will find an informative article on Engineering Experience and Philosophy by Mohsen Kavehrad.

Engineering Experience and Philosophy

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My research interests center on areas of technologies, systems, and network architectures that enable the vision of the information age. I intend to concentrate my research efforts on interdisciplinary topics. An important aspect of my research activities has been technology transfer. I have deliberately sought research topics with an eye towards applications.

The need for new approaches to engineering education is widely understood. Indeed, it is no wonder that engineering attracts a declining share of our brightest young people. Electrical engineering, with a deserved reputation as a difficult, overly mathematical, and often obtuse course of study, is a prime candidate for innovation and reform.

I began my research career at the Fairchild Industries (Now Orbital) in Germantown-Maryland working on NASA spacecraft communications systems and subsystems designs in 1977. Following my work at GTE Laboratories, and the Old Bell Laboratories, when AT&T was a monopoly, I had the opportunity to work with Nobel Prize winners like Dr. Robert W. Wilson at Crawford-Hill Bell Laboratories in New Jersey. Crawford-Hill is a historical site known for the measurement of the Big Bang noise, which had been discovered in theory by a Princeton University professor. I co-authored papers and patents with many of the scientists at these work places.

Over time, I have concentrated my research efforts in industry and academia to take after the Old Bell Laboratories model: “Good engineering research is what makes people change the way they conduct their lives.” Good examples are the Integrated Circuits (ICs), the Internet or Lasers, all introduced in 1970’s.

I witnessed the birth of Internet as a PhD student in the 1970s; my term project in a course on Computer Communications Networks at Brooklyn Polytechnic in New York was the Sabre Network - the very first commercial airline reservation packet-switched computer network using IBM computers. Sabre Network had been launched by American Airlines.

When George Heilmeier was the director of ARPA in the mid-1970s, he had a standard set of questions he expected every proposal for a new research program to answer. These have been called the Heilmeier Catechism. It's a good exercise to answer these questions for an individual research project, too, both for yourself and as a way to convey to others what you hope to accomplish. So here they are:

- *What is the problem, why is it hard?*
- *How is it solved today?*
- *What is the new technical idea; why can we succeed now?*

- *What is the impact if successful?*
- *How will the program be organized?*
- *How will intermediate results be generated?*
- *How will you measure progress?*
- *What will it cost?*

There are other views on how to measure "Good Engineering" research. George Bernard Shaw said: ''*The reasonable person adapts oneself to the world; the unreasonable person persists to adapt the world to oneself. Therefore, all progress depends on the unreasonable.''*

Engineering research is a chaotic, nonlinear, unreasonable process that requires creativity, persistence, and the ability to innovate.

I appreciate the views of Claude E. Shannon, the father of Information Theory field who had some good ideas on how to identify good researchers: *"A very small percentage of the population produces the greatest proportion of the important ideas. This is akin to an idea presented by an English mathematician, Turing, that the human brain is something like a piece of uranium. The human brain, if it is below the critical lap and you shoot one neutron into it, additional more would be produced by impact. It leads to an extremely explosive of the issue; increase the size of the uranium. Turing says this is something like ideas in the human brain. There are some people if you shoot one idea into the brain, you will get a half an idea out. There are other people who are beyond this point at which they produce two ideas for each idea sent in. I think, for example, that anyone will agree that Isaac Newton would be among the exceptional ones. When you think that at the age of 25 he had produced enough science, physics and mathematics to make 10 or 20 men famous - he produced binomial theorem, differential and integral calculus, laws of gravitation, laws of motion, decomposition of white light, and so on. Now, what is it that elevates one up to such a level? What are the basic requirements?*

I think we could mention three things that are fairly necessary for scientific research or for any sort of inventing or mathematics or physics or anything along that line.

1. ***Training and experience**. You don't expect a lawyer, however bright he may be, to give you a new theory of physics or mathematics or engineering.*

2. *Certain amount of **intelligence or talent**. In other words, you have to have an IQ that is fairly high to do good research work. I don't think that there is any good engineer or scientist that can get along on an IQ of 100, which is the average for human beings. In other words, he has to have an IQ higher than that. Intelligence is a matter of heredity.*

3. *This is the one that makes an Einstein or an Isaac Newton. We will call it **motivation**. In other words, you have to have some kind of a drive, some kind of a desire to find out the answer, a desire to find out what makes things tick. If you don't have that, you may have all the training and intelligence in the world, but you don't have questions and you just won't find answers. It is a matter of probably temperament; that is, it is likely a matter of early training, early childhood experiences, whether you will motivate in the direction of scientific research. I think that at a superficial level, it is a blended use of*

*several things. This is not any attempt at a deep analysis at all, but my feeling is that a good scientist has a great deal of what we can call **curiosity**."*

Everyone is born with varying degrees of these necessary characteristics, but I think it is possible to encourage and further develop these characteristics in new researchers by the suggestions made below:

- Don't believe everything you read (critical evaluation).

''If I had thought about it, I wouldn't have done the experiment. The literature was full of examples that said you could not do this.'' Spencer Silver on the work that led to the unique adhesives for 3M ''Post-It'' Notepads.

- Have many fundamental measurement tools (avoid: solution in search of problems).

''If the only tool you have is a hammer, every problem begins to look like a nail.'' – Unknown

- Practice through real research - - - He who performs not practical work nor makes experiments will never attain mastery in the field.

''A lecture is the process of transferring information from the notes of the instructor to the notes of the student without going through the mind of either.'' - Anonymous

- Encourage concentration in the field of research … inspiration requires perspiration.

''Even if you are on the right track, you'll get run over if you just sit there.'' - Will Rogers

- Think and use good engineering judgment: learn how to create good things over a limited time period with a limited source of funding, as the real world out there works this way !

''We haven't much money so we must use our brains.'' Lord Rutherford, Cavendish Laboratory

- Don't rush to judgment – new ideas are very fragile.

''The concept is interesting and well-formed, but in order to earn better than a "C" the idea must be feasible.'' A Yale University management professor in response to Fred Smith's paper proposing reliable overnight delivery service …… Smith went on to found FedEx Corp.