Acceptance speech - Okawa foundation price
By Olivier Faugeras

Good evening ladies and gentlemen, members of the Okawa Foundation, the board of directors, the selection committee and Okawa award recipients. I am deeply grateful to Okawa Foundation for bestowing on me this great honor.

While the prize is given to an individual, I realize that so much of the work goes to my students, postdoctoral researchers and colleagues, Inria, the French National Institute in Computer Science and Applied Mathematics, the European institutions and those that have given me support over the years.

I am honored to share the Okawa prize this year with Prof. Katsushi Ikeuchi, an internationally recognized scholar, teacher, and a friend, who has contributed much to Computer Vision, Vision-based Robotics, and the application of these techniques to the preservation of cultural heritages.

I spent two thirds of my career working in Image Processing first, Computer Vision and Vision-based robotics then. I am reaching the end of the last third dedicating my time to a better understanding of the basic laws that govern the functioning of the human brain.

I have always been fascinated by images and how the human visual system can detect the relevant bits from the huge amount of information they contain, and make sense of it. Perhaps also because they are the stuff dreams are built of, but this is another story.

European education differs in many ways from US education (I do not know enough about Asian education to risk a comparison), and within Europe, the French system surely puts a heavy stress on formal mathematics. The US education system is more pragmatic and puts more emphasis on Engineering approaches. I am a child of both systems, having studied mathematics in France and Computer Science and Electrical Engineering in the US. As a consequence I have always struggled in my work to extract the basic mathematical structures that were present behind the problems I was studying. But I have also dedicated a lot of efforts to the implementation of these structures in software and sometimes hardware.

Let me try to answer the question of what is computer vision? One possible definition is that it is the set of techniques that allow computers to automatically find descriptions of the world from images taken by cameras or other sensors. These descriptions should match those that would be provided by human observers and contain the 3D structure of the scene, the relative motions of the objects moving about in the environment as well as their shapes, etc…. The goal here is to help the humans to perform visually guided tasks or even to replace them with visually guided robots. Great progress has been made in these directions, I have contributed to some of it and so has Prof. Ikeuchi. But how has this progress been achieved?

Certainly for a great part by inventing clever algorithms and putting them into efficient code running on very fast hardware. But what did we learn from this? Do we have, as of today, a theory of computer vision that works as well as Newton's theory of gravitation, Quantum mechanics of the theory of relativity? The answer is clearly no: partial theories have been developed, such as geometric computer vision, or variational computer vision, which I know well and will illustrate in my lecture at the University of Tokyo tomorrow.

I have learnt a great deal not only by developing the mathematical details of these partial theories but also by implementing them on computers and applying them to real practical problems. In doing so I have been able to observe where and when they failed and this has considerably helped me to produce even better theories. But can we afford to multiply the experiments in order to falsify a theory? The answer is again clearly no, this will take for ever!

This brings me to another point, which is dear to my heart. A theory may be wrong for at least two reasons. The first reason is its internal structure: it may be mathematically wrong. The second reason is its explanatory power that may be limited. We should first make sure that the theory is mathematically correct before we even test it: this has not always been the case in computer vision and I could cite many examples of mathematically wrong theories that have been used in computer vision. I am pleading here for the need for proofs of mathematical correctness for any theory in computer vision. These proofs are still too scarce to my taste.

Going back to this idea of a grand theory of computer vision that I was, like many others, trying to develop, I got sidetracked by another idea which in fact was at the very origin of image processing and computer vision, namely the idea, pushed forward mostly by David Marr in the 70s, that vision is a process that is conceptually independent of the physical support on which it operates, be it hardware as in computers or wetware as in living beings. As I was growing more and more frustrated by my attempts to develop the Grand theory of computer vision with an approach that was purely mathematically and algorithmically driven, I began to wonder if by taking a closer look at how vision worked in living beings I might not stumble on some new concepts that I had otherwise missed.

This is how I became interested in the years 2000 in computational neuroscience, i.e. in developing theories that can account for the emergence of high-level cognitive functions from the simultaneous activity of billions of neurons. Each of these neurons is a very complex machinery that some people have compared to a super microcomputer even though they have very little in common: a neuron is a chemical as well as an electrical engine and it makes heavy use of genetic engineering. Moreover, unlike computers, neurons are probabilistic engines: ion channels are submitted and very sensitive to thermal noise, action potentials, the stuff neuronal communications are made of, may not propagate beyond the synapse where they are supposed to signify an important event, etc…. I found it extremely surprising that such a noisy elementary building block, when connected to millions of other such blocks, had the ability to seamlessly process gigantic amounts of data!

There is a striking similarity between this situation and the ideas of statistical mechanics, invented by the great Austrian Physicist Ludwig Boltzman: It explains and predicts how the properties of atoms (such as mass, charge, and structure) determine the physical properties of matter (such as viscosity, thermal conductivity, and diffusion). My current endeavor is to develop what may be called statistical neuroscience, a theory that will account for the extremely high robustness of neural systems to noisy disturbances, and for the appearance of emerging phenomena, such as sensory perception, that are certainly not immediately predictable when one observes the neurons in primary human visual area V1. I will also illustrate some of these ideas in my lecture at the University of Tokyo tomorrow.

As in computer vision the role of mathematics in computational neuroscience is absolutely central, perhaps even more so because clearly new mathematics have to be developed to analyze the human brain. The role of numerical simulations is also absolutely central, because it offers the possibility to accelerate the discovery of new treatments and the testing of their effects while partially or entirely avoiding experimentation on human beings.

This is one of the dreams that are pursued by the European Human Brain Project, the US BRAIN (Brain Research through Advancing Innovative Neurotechnologies), and the Japanese Brain/MINDS (Brain Mapping by Integrated Neurotechnologies for Disease Studies) projects. But there are many other dreams behind these projects, which I cannot, in the interest of time, address here.

I want to express again my sincere gratitude to the Okawa foundation for the honor of being awarded the Okawa prize. Last but not least I want to thank my wife for her patience and support during my life as a scientist.