PROFESSOR TOSHIKI TAJIMA

AN INTERVIEW FOR THE ROMANIAN MAGAZINE „SCIENCE AND TECHNOLOGY”

FIVE QUESTIONS FOR THE INTERVIEW

- I would like to start with one of the conclusions of you lecture at the ELI-NP-IZEST Conference in Bucharest (last July, 6-7). You said that ELI-NP incubates the convergence of medicine and lasers and launches the integration of science and entrepreneurial value creation. So, **„Entrepreneur in science”: Is this something that could really exist?**

Yes, absolutely. I practice this as part of my research activities. With my appointment as Norman Rostoker Chair Professor at the University of California at Irvine I work on more academic research. At the same time I also serve an entrepreneurial research company (TAE) as Chief Science Officer. In my latter capacity I advance entrepreneurial research development.

I am also interested to help produce research results to become useful for societal applications through entrepreneurial activities. That was why I was very happy to participate in the inaugural meeting of ELAP on July 6-7, 2016 at the site of ELI-NP, Magurele, Romania. I found that this ELAP meeting was right on money to make societally relevant contributions and spinoff of ELI-NP coming out and launched into the society.

- *Professor Horia Hulubei, the founder of the first research institute in Romania, 60 years ago, spoke already in 1957 of the need to go from the laboratory to the „factory”. And, he said, both parties should consider that there is a risk, which they should also assume*. **How large is the risk -if any- in approaching new ideas and new technologies? How should we manage it?**

Risks are part of our life. Even in an academic life there exist risks. For example, instead of working on a trendy and popular project of fashionable topics that may attract mainline people’s research, if you choose a wayout daring project hardly anybody works on, you take a considerable risk in doing so to be reckon with, even though a reward if you succeed could be huge. I have done so many times in my academic life and often times I have felt very lonely. Many people cannot tolerate this loneliness and disdain by your colleagues labeling you as a „Don Quixote” for a long period of time in working on. My PhD advisor and mentor, Professor Norman Rostoker, was just like an academic maverick who does not seem to be bothered by such loneliness and he swept those wild bushes by himself and launched an entrepreneurial company himself once he was convinced that the conventional federal bureacracy never funds his idea. I have been inspired by him ever since I met him in 1973. That is why I cherish my title of the „Norman Rosstoker” professorship.

Thus to learn to be not scared by and patient with the loneliness is one of the characters you might need to do risky research.

In doing an entrepreneurial work, I do believe that it is important that we reduce or mitigate risks by taking what I call „low-lying fruits”. I personally love a big goal that motivates myself (and perhaps some others) to work hard and long with considerable toughness and loneliness. Thus I believe that to hoist this high and lofty goal is important to bring in a lot of energy, people, and enthusiasm (and even money), which I may call the „golden apple” at the top of a tree. But if you simultaenously identify lowlying apples hanging in that tree, you can take such fruits with a shorter time and less risk, and you can even eat them while climbing the tree to reach for the golden apple.

One more thing I mentioned during the round-table discussion at ELAP was the philosophy of „fail fast”. When I mentioned this word, the audience made perplexed face, as they must have shocked by it. The typical academic work is, of course, an exact opposite, to make the project perpetually long lasting so that it may become some academician's lifework. What we mean by this philosophy and spirit is the following. In entrepreneurial research and work, we wish to identify the riskiest element in the work. We carve out such element first and test it if this risk may be eliminated or not. If it is not possible to remove, we should fold that part of our effort quickly („fail fast”) so that we do not make further wasteful endeavor and „learn fast” the lesson to start over anew quickly. It is imperative to start over as may times as possible, which would lead to an ultimate success. A success arises from many (and rapid) failures.

- Professor Tajima, you are world known as one of the founding fathers of the high power lasers of today. Already in 1979 you came with an exatraordinary idea: the wake field acceleration. **Would you care to explain it for the readers of S&T?**

In 1979 very few noticed this wayout work that would produce 3-4 orders of magnitude stronger accelerating fields. Some even said that Toshi, plasma is unstable and won't sustain stable strong wakefields.

Prior to my work on laser wakefield with Professor John Dawson, I noticed while still grad school in the UCI laboratory of Professor Rostoker that his graduate student's experiment of collective acceleration using an electron beam to excite plasma wave to accelerate ions did not live up to what Prof. Rotoker wished. What I learned from this lesson is two things. In order to accelerate heavy particles like ions, first, the phase velocity of the accelerating waves should be low so that the wave can trap ions. Second, however, if the phase velocity of the accelerating wave is low, the plasma gets unstable and destroy such waves. In this case plasma plays a mischievous role. This is the daily experience of palsma physics (which many plasma physicists encounter).

My learning from this lesson and what we (John and I) invented, however, was the following idea. Instead of low phase velocity, let us take a high phase veolicy of waves, which therefore cannot resonantly interact with plasma and thus such waves remain stable and plasma cannot destroy such waves. Such waves that are excited by laser have the phase velocity nearly at speed of light c! Thus no plasma particle resonance and no plasma instability. Such a wave, we called wakefield of laser, can grow to be extremely high intensity, precisely because the plasma can support such a wave without being destroyed. Since the plasma is already a broken-down material, no matter how strongly we aplly such laser, it won't break and thus can sustain exteremly robust and strong accelerating wakefield.

You might see a good analogy of this wakefield to the tsunami waves. Tsunami waves offshore have a high phase velocity and these do not wreck boats in the open ocean, but rather simply bob them up and down. Only when tsunami waves approach the onshore and its phase velocity approaches zero, these waves violently interact with the shore and whatever lies onshore (such as the swimmers or sands in the shore).

- Visibility in science is necessay and useful. But, **how much visibility should one try to achieve for a new scientific idea?**

Visibility is good. However, overcompetitive cutthroat attitude for seeking visibility and credit are not healthy. Also the funding competition with too tight opening and too short lifetime for the grant period makes unnecessary bureacracy and the repetitions of stop-and-go. I am really glad that Europeans invented a new way to fund science out of the infrastructure fund with a substantial size (such as multi-hundred-millions of Euro), even though it has its own chagrin. It could make a difference.

I do believe that ultimately, however, people will look at a lofty big idea with an eventual awe. These are the ones we care and cherish.

- Last but by no mans least: **could we imagine, already at this stage of the ELI-NP development, a timeline of its future applications?**

I see some of the „low-lying" applications should bear fruits in a couple to a few years. In the July ELAP meeting, I saw a possibility such as laser driven radioisotopic production applicable to radiological medicine. Some of bigger ideas may come to fruition in several years. In order to make the above a reality, it is very important to amintain a good communication between the fundamental research (and its required facility buildup) and applications research. I also believe that „love" is important. If your ELI-NP can lauch societal applications such as we discussed above, your Government and people would „love" you and the project.

FROM THE ROUND TABLE

TOSHI TAJIMA

- We must first decide what we can do in two years
- „FAIL FAST". This is the difference from academic research

GÉRARD MOUROU

- To really discover something, you must be ready for it!
- If something happens in the US, it's a big deal

NICOLAE VICTOR ZAMFIR

- One of the reasons in building the ELI-NP is changing the mentality. In Europe, very few understand this mission: not only to do science, but also to do things for society. It must be a continuous dialogue (medical) doctors-physicists. To convince them to have trust in each other.

TT (to NVZ)

- Start before start - time is money! Small is beautiful - get the lower lyng fruits and take them to the streets, or you will rapidly remain without money.
- You have to make the Government love you. To be loved by the Government, you must give them what they want.
- Serving the goal is the most important. We are not money mongers, we go for the social good.
- The academic world solves the easiest thing first and then goes to the next easiest...

GM

- For a start-up you need young people ... You do not need many people - you need only a few, but clever people.

TT

- You have to take risks, but calculated risks, not unnecessary ones. So, risk reduction is a very important thing...

FEDERICO CANOVA

- ... will industry accept to share the risk? One of the techniques to reduce the risk is the Government. You don'tlove it, but it can help, especially at the beginning.

JOHNATAN WHEELER

- You broke something ... the problem is not that you did something wrong, it's not over - the problem is what's next !?!