Vortrag Professor Haim Harari beim Symposium "The Role of non-universitary-based research in a country´s scientific landscape", 24. Oktober 2011, ÖAW

From Basic Research to Economic Value

Thank you very much, President Denk, for your kind introduction.

I was asked to discuss certain aspects of the route which leads from Basic Research to Economic Value. Please allow me to start with a few comments, which are known to everybody in this audience. I do not really need to explain these issues to anyone here today, but most members of the general public still need to be convinced of these facts, and it would be good to commence our discussion by referring to them.

We live in the "age of knowledge", when most economic value is created by knowledge. This knowledge is mostly, but not entirely, scientific and technological. Other types of knowledge are also extremely important: not only the humanities and arts but also management and entrepreneurship and a vast variety of additional talents and abilities. But science and technology are clearly the leaders in creating values.

Singapore has 5 Million people and a GDP, a Gross Domestic Product, of 230 billion U.S. Dollars. Pakistan has 170 Million people and a GDP of 170 billion Dollars. In other words: Singapore, in absolute terms, not per capita, produces 30 Percent more than Pakistan. 5 Million Singaporeans produce much more than 170 million Pakistanis! This fact is, of course, the subject for a different lecture on politics, poverty, wars, and extremism, but it is also an indication of the power of knowledge. The people of Singapore have only knowledge. They have absolutely no other resources.

In 1973 oil has become, for the first time, a very scarce commodity, a very expensive resource, and a topic of international political friction. In that same year, Larry Page and Sergey Brin, the founders of Google, were born. They were babies when OPEC was using the oil weapon against the Western World. Today, in 2011, the oil sales of Venezuela - one of the leading members of OPEC - are approximately the same as the sales of Google, which are obtained almost entirely from advertising, derived from a service based on a mathematical algorithm invented as part of a basic research PhD thesis at Stanford University.

These facts give you a certain kind of a proportion between the value of knowledge and the value of natural resources. But what one should add, in addition, and this is not widely believed and understood by the general public, by the politicians and by the community leaders, is that science actually creates new value. Science is not something that takes a certain thing and converts it to something else. Science is able to create new additional economic value. I am not talking here about intellectual

greatness of science, about the beauty of science, and about all of these other features of science that we might discuss at great length, and they are all true. I will narrow my attention to its economic value. New knowledge brings new value and productivity is largely based on improved technology.

Whenever you have a major economic crisis, as is the case now, in Europe and in the United States, as was the case in the fall of 2008, and even when the so-called "dot-com bubble" burst around the year 2000, science and technology continue their march, ignoring the economic crisis. It is very important to understand this fact. The immediate market value of certain technology companies may fluctuate widely, but the progress of science and technology never stops. A country that wants to succeed economically must continue supporting science and technology, even during a crisis, because this is the best investment in the future.

One should also understand that if everything I said until now is correct, and it is correct, then the best investment that one can make, for the creation of knowledge, is to enhance science education. And when I say science education I mean not only from childhood to the end of the PhD, but also educating the general public. We cannot declare anybody over twenty or twenty-five to be lost to science education. This is another angle that we all have to remember and I believe that every national academy has to take this into account in a major way.

One other thing that one has to remember is that everything that happens in science is for the long term. Nothing happens instantly and nothing happens overnight. An intellectual investment may give fruits in 5 years, or 50 years or never. The fruits may be surprising or not surprising, major or minor, or even negative. It is a very slow process and one of the biggest mistakes that governments make is to decide to invest an enormous amount of money for a short period in a specific scientific area, and if it doesn’t give fruits within three or four years, to cut it abruptly.

On this issue I should give a good mark to Europe. The Americans and the Japanese are especially inclined to make these enormous abrupt changes in research funding of specific topics. When US President Jimmy Carter faced major energy problems he created an enormous apparatus of solar energy research and renewable energy research, and three to five years later everything was completely decimated by the US Government. If that would have remained as it was for another twenty or thirty years, we would be in a different place today, in the world of renewable energy.

The Japanese decided in the late 80s to go for a so-called “5th generation computer”, I don’t know how many of you remember this. You have to be above a certain age to remember this expression. Nothing, of course, came out of it. It was the idea that with an investment of many billions over a few years, they will pass the United States computer industry, something that never happened, and it just shows you that planned basic research, directed basic research, never works. The planning of

specific directions should begin much later in the process, not at the level of basic research.

The entire complex of research is very similar to the life of a human being. A person is born, a baby, and you have to feed the baby and take care of the baby without having any idea what will come out of it. Just like a scientific idea in basic research. A few years later, sometimes a specific talent is observed. For little children you can observe mathematical, musical and sport talent. You can never spot, at age seven, the great lawyer of the future, or the great biologist, or the great dentist. But mathematics, music and sports, yes. Similarly in basic research, some projects, after three, five, seven years, show a clear talent and direction. Some other projects take thirty years.

And then there are stages were you are learning only for curiosity, or driven only by curiosity, both for children and for basic research. Later, you are beginning to study not only out of curiosity, but also because you want a profession and you have a goal. The same may happen in research. When you get older, you sometimes switch from one field to another and sometimes you become a generalist with different fields interacting, and sometimes you only know how to pull teeth as a dentist and nothing else. The same happens with research: as the idea gets older, it either proves to be damaging (rarely), or useless (often), or useful in an expected way or useful in a totally surprising way, or useful for one little corner or useful for a wide variety of applications. Just like an adult human being.

It is always healthy to keep this analogy in mind. You must also have a good balance between generations. A country which has too many children and the average age of population is eighteen, usually faces many destructive issues. A country, like many European countries, with a high average age of the population, is also in trouble. The same is in the world of research. You have to have a good balance between basic research and the applications of your research. The precise definitions do not mean very much, in both cases. The exact definitions for being old, being an adult, being a young person, a child and a baby, are rather flexible. The same applies to scientific projects and ideas. As you grow older with your research project, it diversifies and it goes in many different directions, and you have to keep a good balance. The non-university institutes are precisely what can help keep this balance. Some of them are dedicated to basic research and others are devoted to the next steps. They are like "adults".

The knowledge created by basic research, to whom does it belong? It is largely funded by the general public, by the tax payer. General knowledge like a new law of nature, or the structure of the DNA molecule, or the discovery of the most fundamental building blocks of matter, is not the intellectual property of an individual or even of a country. It belongs to the general public. You cannot patent it; there is no need to patent it. It would be morally wrong to patent it even if it could be done, because it really belongs to everybody. The development of new general scientific

methods is also in the public domain. The wisdom and the experience of the individual scientist, as a consultant to industry or to public Institutions is the property of the scientist. It is not any different than the wisdom and the experience of the lawyer or the accountant or anybody like that. But there are many inventions and many scientific discoveries that must be patented; otherwise they will never be used. The best and most clear example is drugs, medications. If a new biological, medical process, either a drug or a medical procedure, would not be patented, nobody will invest what is needed to produce it, and then it will never get into the market. It is morally correct and practically necessary to patent such a thing. It is an absolute must to go through the process of patenting and exploiting the invention commercially. But the commercial development cannot be done by the institute of basic research.

Different levels of the knowledge created by basic research should be treated in different ways. Some remain in the public domain; some really belong to the scientist, especially his own personal wisdom and experience. But, according to the law of every civilized country, new applicable discoveries belong to the institution, which funds it, and not to the scientist. The scientist is the inventor, but not the owner, and the invention has to be protected either by a patent or by copyright or by something of that sort.

If you think about the applications of basic research for creating economic value, you are thinking about the connection between the most "useless" intellectual activity, and the most useful application of it. This is a very complex interaction which, in many countries and in many universities and institutions, fails completely. The failure can be caused by lack of competence of the scientists or incompetence of management, or greed of both, and there are many, many pitfalls in this road. Allow me to devote the rest of my talk to the process of extracting direct economic value from basic fundamental research.

In order to convince you that I have the right to speak to you about this subject, permit me to introduce myself in that context in one or two minutes. My own scientific field is particle physics. You can argue that this is the most fundamental and the most useless field of all the natural sciences. But that is the field that created computer tomography, magnetic resonance imaging and the World Wide Web, among other things. So maybe it’s not so useless.

I come from a country which is plagued by endless number of difficulties from the day it was formed, from Israel. It is a country that has an economy with a per capita GDP roughly equal to the average of the European Union and larger than Saudi Arabia. Its economy is entirely based on Science and Technology and almost nothing else. And it is the only Country in the world that invests more than four percent of its GDP into research. It’s the highest rate of investment anywhere in the world in research, and it was surprisingly high even in the fifties, when I, as a teenager, and all of us did not have enough to eat. It was a very difficult period, and, in those years we were

building at the Weizmann Institute what was then the second fastest computer in the world, at a time that computers were only computing. When did you last see a computer compute? But these were still the days when computers were just computing. So that's the Country.

I come from an Institute, which is entirely devoted to basic research. But it's the only institute of basic research in the world, which has a very significant fraction of its research being funded by royalties. Finally, at the personal level, my own research has never produced anything which can be sold even for one penny. But I happened to have been the president of that institute, and I became its president when we were making less than one million Dollar per year from royalties, and when I left the presidency, 13 years later, we were making almost one hundred times more, per annum, from royalties, and now we are earning even more. All of this was not because of me, I just happened to be there at that time and to be an active participant in the process.

We created the company that is exploiting our intellectual property in 1959, one of the first in the world, and in 1988 we were still making only below one million Dollars per year. So we were quite patient, I must say, and when I say we, I refer, of course, to my predecessors, the forefathers of our institute, because in 1959 I was just an undergraduate student. Today, the total sales, by industry, of goods which pay royalties to the Weizmann institute exceed Fifteen Billion Dollars. This sum, of course, produces only a small percentage of royalties to the Weizmann institute. But it is not one product and not three products. It's quite a number of successful products, each with its own story and its own interesting history. So you cannot say it is just one lucky break.

How does it happen? It requires three components. First of all, it requires scientific excellence. We are a very good basic research institute, but we are definitely not the best institute in the world, and not anywhere near it. We are not as good as the Max Planck Gesellschaft, or as Harvard or Stanford. But we earn more than them from intellectual property. The second component is a correct management attitude towards the exploitation of intellectual property. And the third component is luck. But if somebody would force me to divide the total among these three components, I would say that, of the total success, half is scientific excellence, half is luck and half is the correct management. I am aware of the fact that one half plus one half plus one half is not equal to one. It is the overlap and the synergies which are creating it. If you would have only one or only two of the three components, it would lead you nowhere.

Having said all of these, I will now proceed to list ten basic rules for exploiting the intellectual property of a basic research institute and some of you already know that I have a special affinity to the number ten. I always like to present my thinking in terms of "10 commandments". This must be a habit in the part of the world from which I come.

The first item is the combination of scientific quality and luck. In order to achieve quality in basic research, paradoxically you should not pre-select the fields of research. You should choose the best people and let them do what they prefer to do. It is proven that it is not useful to say: we are going to make a breakthrough in cancer research by studying the cancer of the liver, or the pancreas, or melanoma. You never know in advance where the breakthrough will come from. Nobody would have guessed that studying superconducting magnets would save lives of cancer patients by MRI before and after the treatment.

So you choose your scientists by their quality, and multidisciplinarity is extremely important. Institutes which are devoted to one narrow field are not very likely to create something like this. No one would be able to use lasers for eye surgery or magnets for medical imaging, if physicists and medical experts would not collaborate. In that context, I have to repeat what I heard recently from somebody: Today the economists think that they are psychologists, the psychologists believe that they are biologists, the biologists think that they are chemists, the chemists think they are physicists, the physicists think they are god, and god thinks that she is a mathematician.

So multidisciplinarity is very important, and as far as luck in concerned - every successful scientist will tell you that everybody gets lucky. The successful scientist is the one that, when he or she gets lucky, they know how to exploit it and how to push it. There hasn't been a scientist in history that didn't get lucky on several occasions. So don't always believe too much that luck plays a big role in science. Luck creates some opportunities, but talent exploits the lucky break only with hard work and dedication.

Item number two is how you allow the commercialization to influence your basic research. And the answer is: you don't. You should never, in an institute of basic research, allow commercialization, or the desire to make money, to influence your direction of basic research. You should have an open publication policy. You can delay a publication only a short time until a patent application is submitted, not until a patent is registered, which takes a long time. And even that delay should require the agreement of the authors. If you are a first class institute of basic research, you should refuse to accept from industry grants which ask you to do mundane routine work. For instance, if you invent a new drug, somebody has to check if it is toxic. That's the business of the Pharmaceutical companies, that's not the business of the basic research institute, because this is a routine, uninspiring work. And the reason for this is not because it is underneath your dignity to do such research. It is an important part of any development. But, intellectually, it is usually not a challenging problem, and it will drive away your best people, who do not like to do such work. You must always keep the best students, the best postdoctoral fellows, and the best professors. Keeping them happy requires intellectual challenges, not routine work.

Item Number three: it is crucial for the institution to have common economic interests with the scientist. The scientists are human. If they smell the possibility of making a profit, they will go after it. And the last thing you want is any kind of conflict between the institution and the scientist. The correct formula is: the ownership is of the institution; the institution must develop the tools and the mechanisms and the knowledge and expertise to exploit the discovery commercially. But there has to be a clear transparent formula by which the inventor scientist receives a fixed percentage from any financial consideration. In intellectual property, a financial consideration can have many forms: royalties, license fees, shares in a start-up company, shares in a public company, options, compensation from another company for not attacking your patent, and numerous other forms of financial reward. I can tell you stories that will last for many hours, about different forms of converting intellectual property to income. Any such form has to be divided between the institute and the scientist. At the Weizmann Institute we give to the scientist or scientists forty percent of any income. In America it is usually one third to the scientist, one third to the department and one third to the university. I strongly oppose this, because sometimes the big income comes thirty years after the invention. The scientist deserves it even if he has been retired for a long time, but there is no point of giving one third of the big income to a department that may be completely irrelevant thirty years after the fact, or completely incompetent by then. It may have been the greatest department thirty years ago, and not very good now.

So I think that it's very important that the scientist receives a fix percentage from any income, no matter what form it has, and then the scientist has no interest to convince the institution: "let's create a new company, let's not make a company, let's give it to this pharmaceutical company or let us give it to another." Both the scientist and the institution have the same interest. They both want to turn the idea into an invention and to do it in the optimal way.

Number four: There have to be very clear rules regarding intellectual property in a basic research institution and, even more important, strict policing of the rules. Scientific institutions, like the opera and the ballet, are full of prima donnas. And if you allow one prima donna to violate the rules of intellectual property, you can forget about the rules. So the rules have to be described in clear documents and they have to be enforced, unambiguously. There will always be grey areas, because intellectual property is so difficult to define, so easy to steal and so easy to fool around with. But it is the leadership of the institute that has to take care of it. How to do it? I will describe in a few minutes.

Number five. There has to be an entrepreneurial spirit in the research groups, and this is missing in most places in Europe. In most places in Europe the professor knows that the government, through some mechanism, pays the research budget of his or her group. If the support is sufficient, fine, and if not, it is not so fine, and that's it. It is very important to have the atmosphere which is so normal in the United States, and also in our case in Israel, that every research group is funded by half a dozen

sources: By government, by foundations, by foreign international competitive grants, by private donations from philanthropy, by intellectual property, by this, by that and the others. So every research group is like a little start-up company, constantly trying to get funding from different sources. When that atmosphere exists, there is also the atmosphere of trying to exploit things commercially whenever possible. I believe that this is one of the major problems in Europe in converting basic research to intellectual property income for the institution.

This completes the first five points. As you know, the Ten Commandments were written on two tablets, so this is the first tablet, which reminds me of a story I have heard recently. When Steve Jobs passed away, he went into heaven. God approached Moses and introduced them to each other and he said to Moses. ”Moses, this is Steve. He will improve your tablet.”

The sixth item is how you manage the intellectual property exploitation within the research organization. Here, there is something which looks bureaucratic and minor, but is extremely important. From the top of the organization, from the President, or the Rector, or the head of the organization, and not from anywhere else, an almost daily mantra must come: “We are doing basic research. That is our only goal. We are not here to make money. But if, in doing basic research, we encounter something that could be useful we should not be ashamed to exploit it."

The wonderful scientific prestige of CERN was not diminished at all by the creation of the World Wide Web, even though this was not an achievement in particle physics. To create something commercially important or medically important should be a great pride; it’s not underneath the dignity of the basic scientist. Some basic scientists think that way. And the head of the organization should remind them, day and night, that you are not there to make money and you are not there to do practical research. But if it happens, go for it.

This can only happen, and this is crucial point, if the management of technology transfer, commercialization and intellectual property, is led by a scientist, not by an administrator. That scientist has to report directly to the head of the organization. If you have a technology transfer office somewhere in the corner with a person who is four layers in the administration below the director, or the president of the organization, nobody will listen to that office. That office can send a hundred letters and nobody will obey it. Only when the full prestige of the head of the organization is behind the implementation of the policy, it can succeed. This is a proven point. You just look at every organization in the world and you will see it. And it has to be in the hands of a scientist, because otherwise the great prima donna scientists will say: “Oh, these administrators, all they want is money, they don’t care about science. We are pure scientists, and they are just bothering us, we will do whatever we want". But if the leader, who himself is a good scientist in basic research, is controlling the process, it’s a different situation.

The next item, number seven is – you should, whenever in doubt, issue a patent. The patent should be global, of course, not only in your own country but European, American, Japanese, covering the world. It costs money. But relative to a great success, that money is negligible. And all the great failures are the ones that were neglected to be patented. Once you patent it, try to pursue it to the level that it really becomes attractive to commercial interests. If you have a commercial entity, which comes to you and wants to buy it or get a license for it, at a very early stage, and you can still develop it, for two or three years with your own means, don't sell it. This may be the difference between commercializing a ready product or a ready prototype and a situation that you are selling a theoretical idea. As a result, there may be a large difference in the royalty percentage that the institution and the scientist may share. It might lead to a tenfold ratio between the level of income that may be derived, from the final sales, which may happen only twenty years later. So as long as you can continue with the development, it looks hopeful, and it is scientifically interesting, you may wish, after patenting it, to take a deep breath and wait another couple of years before you license it to commercial interests.

What should the institute do with the income? If the income is very high, you should not spend it in your operating budget. Income from intellectual property can be huge this year and zero in the following year. Your bestselling drug can kill somebody and drop out of the market tomorrow. Somebody else may invent a better mouse trap than the mouse trap that you have been making money on. So the money should be saved and, in the case of the Weizmann Institute, all of this money goes to an endowment, and it is the income of the investment of the endowment which is funding the institute among other multiple sources.

Number eight is the handling, in a delicate way, of the grey zones. I mentioned earlier that there will be always be grey zones in applying the rules and regulations of intellectual property, within an Institution. They require great sensitivity and a lot of attention from the top management. Without a proper handling of these issues, the scientific prima donnas of the institution will run away with the intellectual property, to their detriment, not only to the detriment of the institute. Because, ladies and gentleman, forty percent of a lot is much more than a hundred percent of very little. If the prima donna runs away with intellectual property, he or she will have a hundred percent of very little, but if the institute knows how to exploit it, it will be 40 percent of a lot.

Number nine- Will the government or the potential philanthropists say "Hey, you make all of this money from intellectual property, we don't want and do not need to support you anymore." It turns out that it works the other way. When you tell the government and the philanthropists that you are a fantastic scientist because you discovered a new law of nature, they look at you like "Ah, well, law of nature. We are only interested in the laws passed by the parliament". But if you say "Our basic research discovered new laws of nature and new things, and eventually these discoveries were so important to the economy that they created so and so many jobs

and sales of 15 billion Dollars", that’s a different story. At least it increases the prestige, if not the funding.

And finally number ten. Intellectual property rights are so obscure, and they are often ill defined. A patent is a patent, but if somebody invents something very similar, there are always disputes. Courts all over the world are constantly dealing with such disputes, and sometimes there is an enormous amount of money hanging in the balance. It is the responsibility of the institute, and the individual scientist cannot do it, to go after every such opportunity and make sure that what is yours is yours. Do not let somebody else in another institution, or another person, or another scientist, or another drug company, or another electronic company run away with your intellectual property. This sometimes means that you have to invest some of your financial income in battling such situations, but let’s face it: This is the nature of managing intellectual property. Many industrial companies feel that academic institutions are "soft targets" for a variety of maneuvers, which can divert away the intellectual property income. You must prove that you are not such a "soft target".

These are the ten points, which we have been following quite successfully.

I believe that by trial and error, the world is moving in that direction, in an inevitable, irreversible way. I can see a situation in which in twenty or thirty years, every high quality basic research institute in the world will be in a similar situation of contributing directly to the economy and benefiting from its intellectual property. In IST-Austria, the Institute of Science and Technology - Austria, we have decided, from the very beginning, that we would follow these rules. Of course, this Institute is still a baby. It will be years before this becomes a major source of income, but you have to start working on it from the very beginning. If you are lax for the first two or three years, you would lose the opportunity to get good results. There is no reason why not all institutions will achieve that kind of success. It will be good for the economy, it will be good for industry, it will be good for the country that finances this institution, it will be good for the institution itself, and, last but not least, it will be very good for the scientist himself. It is one of those rare situations, in which everybody wins, rather than having a zero sum game. That is exactly how knowledge is converted to economic value.

Thank you very much.