

Im Dialog:Kim **Nasmyth** und Arnold **Schmidt****What is Science All About? What is the Aim of the Game?**

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Begrüßung:

Rudolf Scholten

Kim Nasmyth wurde am 18. Oktober 1952 in London, GB, geboren, besuchte das Eton College, Windsor, studierte Biologie an der Universität York und dissertierte 1977 an der Universität Edinburgh über „*DNA replication in the fission yeast Schizosaccharomyces pombe*“. 1987 kam der Molekulargenetiker nach Wien an das neu gegründete IMP (Research Institute for Molecular Pathology). An dem zu Boehringer Ingelheim gehörenden Grundlagenforschungsinstitut arbeitete Kim Nasmyth zunächst als Senior-Gruppenleiter, 1997 übernahm er die Geschäftsleitung. Es gelang ihm, das hohe wissenschaftliche Niveau und das internationale Ansehen des IMP weiter auszubauen, ohne sein Forschungsgebiet Hefegenetik zu vernachlässigen. Kim Nasmyth ist Mitglied renommierter Forschungsgesellschaften (u.a. Royal Society, GB, Österreichische Akademie der Wissenschaften, American Academy of Arts and Sciences) sowie Träger zahlreicher Auszeichnungen (Wittgenstein Preis der Österreichischen Bundesregierung, Boveri-Preis der Universität Würzburg, Croonian Lecture/Medal of the Royal Society, Goldmedaille der naturwissenschaftlichen Fakultät der Prager Karls-Universität u.v.m.). Im Jänner 2006 wird er den namhaften Whitley-Lehrstuhl für Biochemie an der Universität Oxford von Edwin Southern übernehmen und 2007 auch Raymond Dwek als Vorstand der Abteilung für Biochemie ablösen und damit eines der größten Biochemie-Institute der westlichen Welt leiten.

Arnold Schmidt wurde am 7. August 1938 in Wien geboren. Er studierte Physik an der Universität Wien und promovierte 1962. Nach zwei Jahren am neu gegründeten Ludwig-Boltzmann-Institut für Festkörperphysik in Wien, arbeitete er als Research Associate bis 1971 am Physics Department der Universität York, England und anschließend am Department of Physics der University of California, Berkeley. 1975 kehrte er nach Wien zurück, habilitierte sich 1978 und wurde 1986 ordentlicher Professor an der TU-Wien. 1999 wurde das Institut für Photonik unter seiner Leitung gegründet. Sein Forschungsinteresse galt schon früh der Quantenelektronik, insbesondere der nichtlinearen Optik, Festkörperlaser und ultrakurzen Pulsen. Derzeit konzentriert sich sein Interesse auf Femto- und Attosekundenlaserpulse und deren Anwendungen. Arnold Schmidt ist Mitglied der Academia Europea und der Österreichischen Akademie der Wissenschaften. Im Jahr 1996 wurde er Fellow der Optical Society of America und 1998 Fellow der American Physical Society. 1994 wurde er zum Präsidenten der Fonds zur Förderung der wissenschaftlichen Forschung (FWF), der wichtigsten Förderorganisation für die Grundlagenforschung in Österreich, gewählt. Er wurde zweimal wieder gewählt. Sein wissenschaftspolitisches Interesse fand unter anderem Ausdruck in der Mitarbeit in nationalen und internationalen Organisationen und Gremien, wie CDG, ESF, EUROHORCS, ESTA, EURAB.

Rudolf Scholten

Ich freue mich, eine Serie im Programm zu starten, indem wir Wissenschaftler und Wissenschaftlerinnen einladen, die aus Gründen ihrer medialen Zurückhaltung weniger präsent sind, als es ihren wissenschaftlichen Ergebnissen entspricht. Das ist nicht sofort zu verstehen als im Gegensatz zu anderen, sondern das ist einfach so zu verstehen, wie ich es jetzt gemeint habe. Ich bin froh darüber, dass Arnold Schmidt, den ich Ihnen nicht vorstellen muss, ursprünglich übernommen hat, diesen heutigen Abend zu moderieren, und vor wenigen Sekunden übernommen hat, diese Serie zu moderieren. Willkommen, Arnold Schmidt. Now I would like to welcome our guest of honor tonight, Kim Nasmyth, good evening. I will leave the professional introduction to Arnold Schmidt because I admit readily that I cannot even pronounce the title of the thesis. We have a guiding line for our discussions that is that we would like to invite you to meet here for events which Bruno Kreisky would have enjoyed. I am sure that tonight is a special example for something which he would have enjoyed immensely to have you here, and we are proud to do that and to listen to you. Welcome again.

Arnold Schmidt

Good evening, everybody. Science is much talked about these days. It is mainly in connection with boosting economic growth and increasing competitiveness, things like this. Everybody, especially in Europe but also in the United States, has a certain uneasy feeling about the rising countries in Asia. It is obvious that we will have a hard time to compete with them. We will have a very hard time to compete with them in terms of wages, of course. Even if we could we don't like this idea all that much. We can hardly compete with them in terms of social services. And again actually we do not want to lower our standards that much. And we cannot compete in terms of environmental standards. This is the reason why people then actually concentrate on the one solution, if there is one, and this is we need high quality products, we need innovative products, we need products which we can sell at a profit under the restrictions I mentioned before. So innovation is the buzz word. The next one is science. Science, of course, only being part of this innovative process should be able to help us. The next idea people have or the next statement people have is, well science actually is in good shape in Europe, but there is something which people have now termed the European paradoxon, that science is in good shape but it can't make good use about science. So we have geese who lay golden eggs but the eggs are a little bit too small, the feeding is too expensive, and they lay them not every day but in larger distances apart. This is the reason why politicians, civil servants, administrators, people who do science politics concentrate on this thing, what could we do to make science more useful, helping more in this innovative process.

This is not the scheme of tonight. Tonight the scheme should be what do scientists really actually do as scientists. And we talk about the keys so to speak, we are not so much worried about the golden eggs. When I had a while ago a discussion with you you said, well let's talk about science in a very different way than usually people do, as you already mentioned in your introductory. We thought, well, let's invite an eminent scientist who just knows how science works, and who knows how scientists tick, and let's have a discussion with him. So we came up very quickly with your name. And to my big surprise you said right away yes, which I am very grateful to you. Then we met in a coffee house, and we had a pre-discussion of what we want to do here. Actually this was a marvelous one and a half hours. Later on I made notes about the things which we talked about there including a title which is rather baroque, I must say. "What is science all about? What is the aim of the game?" I think that is a good title, this is exactly what we want to talk about tonight. Then I made several notes, some of you actually must have seen these notes, but not all of you. I will not go into details, but just to give you sort of a gist what we thought at that time in the coffee house were first three statements. The first one is a question, what do scientists do, and then a partial answer. The partial answer is, problem solving is not in the center of their work. The second one is a statement, science and its development is the work of individuals. As in arts gifted individuals are rare. The third one, scientist make discoveries. They also produce results which they might try to sell on a surrogate market. I think these three should give you a gist. Thank you very much that you were willing to come here and please start.

Kim Nasmyth

The task this evening is a difficult one because we got I fear a lot of people here who probably will agree with everything I say. So one problem is I have got to say certain things that will be sufficiently controversial so that somebody will be able to disagree with them. I only said I would do it as readily as I did because Arnold assured me I would only have to talk for ten of fifteen minutes. I thought maybe I would be able to manage that. Then yesterday at breakfast I sat down and started writing down some questions which I hope resemble to a certain extent the topic of our previous conversation. I suddenly realised that there were about 30 different questions, the list was endless. In 15 minutes I try and restrict myself to some of the key

points. It may sound pompous and inappropriate particularly here in Austria which is the birth place of logical positivism and Karl Popper and so forth to raise again the question what is science, what distinguishes science from topics which many would agree were not science. Many people would think that the last word has been said or that everybody agrees what the difference is. I have to confess as a young student I was very much influenced by Popper and read all his books with great enthusiasm. I came away over the years I have been a practising scientist, I think part of the way I think about it is partly influenced by what one has read, but much more influenced by ones daily, weekly experience over the years. I think there is an aspect to what distinguishes science from non-science. I don't think Popper ever really put his finger on. Everyone agrees that scientists ask questions, and they try and answer questions. I think most people would agree that the fundamental logic of Popper in terms of refutation as opposed to proof, I think they all agree that he had a point and that it goes in fits and starts, and it is not just evolutionary, it can also be revolutionary. But I think there is one thing that neither of them ever really said. Well, that is my recollection of it. It is just my personal experience. In the two aspects that I mentioned I don't think science really differs so much from other topics. Lawyers pose questions, and historians pose questions. May they answer them or they don't answer them. They use the same fundamental logic. So what is it that distinguishes particularly the natural sciences from other forms of human enquiry? To me at least one fundamental difference is that the type of question you ask – and it is not necessary a question whose answer would be have a sort of mathematical formulation or be phrased in the language of the physical sciences - , it is more in terms of you ask a question whose answer generates more interesting questions than you started with. If that is correct then what you do is you generate a chain reaction of questions and answers. I think it is this aspect of science being a chain reaction of questions and answers, that its knowledge generation is like a chain reaction as in a nuclear reaction. I think many scientists are aware of it. I heard it actually very beautifully put by this year's winner of the physics Nobel Prize. I heard his speech in Stockholm this year. He put it a different way, but fundamentally saying the same thing. Which was, that science is not really about generating knowledge, it is about generating ignorance. So when you pose a question and you answer it, you say, oh well the answer is knowledge. But actually the key thing about science is that that knowledge – if it is important knowledge - makes you realise how much you didn't even know you didn't know. There are all sorts of things you didn't know you didn't know until you answer that question. It is the sort of question that generates that gosh, now, gosh, I didn't realise that we didn't understand ... And that is what generates this chain reaction. I think that is the, to me at least, the fundamental thing that distinguishes probably the natural sciences from other forms of enquiry. I think anything that generates that sort of chain reaction creates this huge proliferation of knowledge which sooner or later, much of it will become useful at some point insofar that knowledge to some extent can be equated with power, and the ability to manipulate and understand our environment. So that was what is science. And I am sure that can generate a discussion.

As a sort of related question, good science and bad science. Maybe I should just say parathetically that I think that within the terms of what I just said, of course quite a lot of the activities that actually many of us would describe as science in fact probably aren't. This is maybe a little bit controversial. But I suspect 90% of what most scientists do does not generate those sorts of chain reactions, and as such their activities are not really scientific in that sense of the word. But I am sure that will generate quite a lot of discussion. So I think not all science is science by those terms.

Then that leads us to good science and bad science. The good science, it is obviously crucial to know whether you answered your question. There are a lot of very good questions, but it is

very hard to tell if the question has been answered or not. I think the wonderful things about good science and important discoveries is that they are very transparent. You start off with complexity, and mystery, and confusion, and really good science you end up with some simple statement about the world which we haven't made before and didn't realise was correct, and suddenly allows you to explain a lot of things which seem to be very confusing. The most important thing is that it be sufficiently simple that as many people as possible and as wide a network of people and not necessarily experts can see for themselves whether it is right or wrong. As a consequence bad science just like bad economics, or bad politics, bad business is when it is unclear what people have actually said, so unclear that nobody can tell whether it was right or wrong. The other ghastly thing about bad science is not that it is so complicated that you can't tell whether it is right or wrong, but even worse that it doesn't really matter whether it is right or wrong because it was unimportant, it wasn't something that generated a chain reaction. That is the other real characteristic of bad science. It was a question, it could be answered, but it didn't matter whether you answered it or not. I am sure that could be controversial.

How is science done? I think important science, science that makes a difference, or discoveries that make a difference are almost universally made by individuals. It is an individual vision, an individual process, even if they work as teams. And many scientists have to work as teams. But at the end of the day the fundamental insight whether it be the person who read off the reading and suddenly realised, whether it is Hubble staring down his telescope and seeing the stars receding, or whether it is Einstein working up the general theory of relativity, whatever they are, it is made by an individual. Discovery is not made by teams as such. It is rare individuals, again as Arnold pointed this out. We accept in the arts that there are great composers, there are great painters, there are a lot of very mediocre composers, most of them we never heard of, and even more mediocre painters. And we accept that. But we find it much harder to accept the idea that only certain people are probably going to make discoveries either because they need a lot of luck, but usually it is a combination of luck and a whole series of circumstances, talent, curiosity, intelligence, having the right language, a whole series of things. But it is only certain people who have the childlike curiosity, and it is certain people who have a dissatisfaction with the state of affairs. You have to be deeply non-conservative. You got to say look. Most of us as human beings are very good at telling stories and being able to explain the world, whether we invoke God to explain the world, or whether we are scientists and we try and explain the world in terms of irrational terms. But we are very good at taking phenomena and explaining them. But usually those explanations are really just stories, and most of the time they are completely wrong, even though they fit the facts, because they are very ad hoc. Really rare individuals realise that the explanation is deeply inadequate. And there are other people who are very satisfied with the current explanation. You have to have a personality who is not frightened of not knowing. A lot of people are frightened of not knowing. Even as scientists. I don't think it is just religious people who are frightened of not knowing. We frequently tend to be over-satisfied with our explanation. So there are only certain people who can do this. One of the great challenges is how you support those people and make sure that they have enough resources, and leisure, and money to do the experiments.

How do we measure the fruits of science? We live in a world that is dominated from this point of view intellectually by an American-style industrialisation. You can measure output. And we believe you can measure output as scientists by measuring publications, and impact factors. I think those are just surrogate measures, and they are deeply inadequate measures. We live in a mountainous country. Therefore I would like to draw an analogy that points out the fundamental problem with the quantitative publication measure of the output of scientists.

The most important thing about a range of mountains is that you have foothills, and the base of the mountains and peaks, and that finally you have a peak that is higher than all the others. If we said that the highest peak is the sort of discovery that really made a difference, the one that set off a chain reaction that didn't exist before, that generated a lot of ignorance. If you accept that then the only thing that matters is that there are some big peaks. Of course, you can't have a big peak without the foothills and the base of the mountain. So you need a lot of activity to support that. But I think one should never lose sight of the fact that it is the big peaks that matter. It is the one or two discoveries that community might make that really matter. You may need a lot of subsidiary activity in order to have a chance. Basically you are putting your chips on the table. But one should never lose sight that it is the big peaks that matter. We are notoriously bad at being willing to accept that is the case.

That leads me to the two last questions that I pose. Is it easy for contemporaries to recognise the big peaks, great discoveries? The answer is a mixed one. There are clear cases where no communities have succeeded in recognising great discoveries. Probably the best examples would be Alfred Wegener with continental drift, completely unrecognised by his contemporaries. Another of course is Gregor Mendel, totally unrecognised by his contemporaries, who has spent his entire life being interested in the genetic substance, what is was that passed down from one generation to another. And this obscure monk came out and said, it is all mathematical formula, nothing could have been simpler. But he couldn't see it. Most of us don't recognise great discoveries, partly because we are unimaginative, but mainly because if this person is right then it means we haven't discovered. So by and by it is very nature, we are not very good at recognising great discoveries. Having said that, Newton was recognised as a genius in his time, Darwin certainly recognised in his time, Watson and Crick recognised in their time. Sometimes it just hits you in the face. If you look at Wegener and Mendel, they were way ahead of their time. They were further ahead of their time than was Newton in his time, or Watson and Crick, or Darwin. We think they are the greats, but actually if you think about it, those discoveries are no coincidence. Wallace actually beat Darwin to a draw. That pushed him to publish himself. It might have been a third person. Wegener, nobody was thinking about it. Mendel likewise. And Newton, well the ideas were around. He had these tremendous fights with Hooke. Hooke said they were all his ideas, and Hooke was just a worse mathematician. But there are a lot of theories on that. It was waiting to happen. Well, Watson and Crick, everybody would agree. Great discovery. It would have been made in the next six months. Sometimes it is easy to recognise, sometimes it is not.

The last thing I like to say. Is it easy to recognise potential discoveries? One of the most important things for our society is to make sure that within the scientific community you try and foster as many potential discoverers, and make sure that people you think have a potential to discover something important. If that be at the expense of those who you think have less potential to do so, then so be it, which you know are difficult decisions. There is no formula to recognise potential discoverers. Sometimes you see people and see that they are not going to discover anything, they are not imaginative enough, or they are just not clever enough. But there are so many different things that go into making a discovery. One is just to behave enough drive, and have enough ambition, and have enough expectation. People can transcend all sorts of limitations. And all sorts of unimaginative, stupid people can make great discoveries. And therefore it is very difficult. And likewise all sorts of people who have got enormous talent squander their talent just because they didn't have enough ambition, or their expectations weren't great enough. So to some extent you can recognise it, and to some extent you can't. That means you have got to allow a Darwinian process. You got to allow quite a lot of young people to have a good go, but there has to be a weeding out process. That really leads one to the whole thing. You can't expect as a scientist to have a career. Somebody said

at age five I am going to be a concert pianist. That would be crazy. Or I am going to win the 100 metres in the Olympics. Likewise as a scientist, as you are growing up, wanting to be a scientist, some of us just bumped into it by chance. I don't think it is something you could set out to do and be successful. At some point the system will judge you to be good at it or not. If you are not good at it, you are not going to have a career in it. In that I don't think you can have a career in science. It is a vocation just as being an opera singer. Only very, very few people can be an opera singer. If you just don't have it, you can't do it. The same is really true about science. Again, I don't think many of us really accept that. I think I should stop there.

Schmidt

Thanks a lot. Well, there is enough material here for discussion.

Question

Helga Nowotny, of the WZW. Your talk was really aimed towards a specific form of creativity. Everything you said about science and not science I think up to a point you could have substituted the word creativity for science. There is an interesting empirical fact if you look at this huge distribution of creativity. Since the 1960's 15% of the top scientists are responsible for 60% of the publications. If you look at technology there are studies that can show you that 5% of new technological ideas are being picked up. The rest is just this big pool of ideas that are not being picked up. If you move to another field that we call creative industries – and I deliberately choose this field where you have a mixture of artistic production, young people trying to work with computers, and work for advertising, and work in entertainment etc., it is even worse there. Because you start with a huge pool of people and empirical studies that show how the machinery works to weed out. The point I want to make is, that in science the criteria for selection are different. In the creative industries you have the market, you have criteria that are even difficult to pin down to insiders. The criteria are very mixed there. In technology we know it has to be technically feasible, the market conditions have to be right to meet financial investment. You can build up your case. If one of the factors is missing, your idea can be brilliant, but you will not get very far. In science the way you define it, I think the criteria are at least purer than in the other fields, and we do recognise good science. People have tried to define excellence, and excellence is practically impossible to define, but you recognise it when you see it.

Nasmyth

It is so rare that it just hits you.

Question

You recognise it because it goes beyond what already exists. This is what makes good science. You spoke about the chain reaction. The way how I would describe it is that it goes beyond what we already know, and it opens up a new space of potential, of possibilities, and you want to know what is it all about, you want to move into it. Where I want to contradict you is with the two cases that you cited, Wegener and Mendel. With Wegener, there are many historical studies that have been done. And Wegener's idea was new, but at the time it was impossible to prove. There was no way of proving his ideas. You had to go a long, long way to geographical survey. It was data that were impossible to get at that time. I think what you would have to add also is, you have ideas, but in order to be accepted we need evidence, and evidence again varies from theme to theme, from time to time, there is no absolute criteria for evidence.

Nasmyth

Yes. What you are saying is very true. It is largely also true for Mendel. It was so abstract what he did that it really required chromosomes to be discovered, and the equivalence between chromosomes and units of heredity to be made. So that people could then really begin to think clearly about it. Chromosomes had not been discovered at the time when he did his breeding experiments. But there was some evidence in Wegener. There were these extraordinary coincidences of the African and South American coastline. Yes, it was clearly before its time. I don't know very much about it. But it is my understanding, a lot of his contemporaries just thought it was crazy. It wasn't just that they said oh well, that is a very good idea, but we can't test it. I think they just said it was crazy which is a slightly different thing. But you are absolutely right.

Question

I would like our discussion to go a bit broader and to look into creativity. And scientific creativity being one particular form. When Arnold started out and saying, normally when you speak about science you want to know about investment and the golden egg. I am of the opinion that it will become more and more difficult to separate the two. Instead of making a sharp demarcation, we should look at it as a kind of human creativity changing form knowing very well that inside you have different selection criteria that are in a sense easier to recognise, easier to implement. But you see this election procedure goes on also in other areas. Then you can start to discuss which conditions can help to spot the young people, then you can work on the supporting mechanisms, institutions.

Nasmyth

I would completely agree with you. Business is a wonderful example. You have this problem of transparency. Enron is a case of clear non-transparency. It is exactly the same problem for investors as it is for other scientists. You can understand what is going on, you can't decide whether it is a good investment and likewise you can't say is it right or is it wrong in science. The transparency, simplicity is just as true in business as it is in science. And likewise for all the people who believe in market research, at the end of the day the people who suddenly make a huge fortune are usually highly creative people who won't do it by market research. They said they had visions.

Question

Your definition of good science being a chain reaction, creating more questions by having answers, I try to understand it from my point of view as a mathematician. Your definition certainly works for sciences which have something to do with the real world. Mathematics in the end wants to have something to do with the real world, but as sort of a constructive thought. In mathematics you can actually create as many questions as you want. So the chain reaction thing is not the only criteria. And there are actually whole schools of mathematicians who create their own theories, asking new questions which only a small circle in the world understands, answer these questions, publishing in journals, but the relevance question always ringing. So at least in mathematics something more than just this chain reaction thing must be true. I don't know what it is. I think it is somehow relevance, not in the sense of the golden egg, of the immediate application. But I think most good mathematics has always in the history been created by some relevant questions from outside mathematics. For instance quantum physics created questions to mathematicians, from this whole theories developed. So there must be something more than chain reaction.

Nasmyth

Yes, I suppose you could say part of the really important chain reactions stem from the mathematics, laid out of that particular small area. I think you could pursue the chain reaction analogy if you said a really important discovery in mathematics led you out of a small area, led your chain reaction into other areas. That is one of the banes not just of mathematics but also of many physical sciences. And obviously the big discoveries get people outside of the narrow areas. They almost always start with a very narrow area, but somehow have a universal relevance that whether it is to the real world or to technology or to some other field of inquiry that creates a much larger chain reaction than the other ones.

Question

Ingela Bruner with the Salzburg board of research and science, an advisory board to the province of Salzburg. I'd like to put forward two questions. The first one is a bit tongue and cheek to take your own description from the beginning. You introduced yourself as a practising scientist. That is an expression that one generally hears in connection with other professions. Why did you introduce yourself as a practising scientist and not as a scientist? The second question relates to the very interesting differentiation that you put forward between the real discoveries, the real big ones, and what was in the air, even if they were big ones also. You mentioned in your presentation the importance of creating the environment for scientists. You mentioned leisure, money and so on. Do you think we should take into account the two different kinds of discoveries? Do you think that there is a different environment enabling the two?

Nasmyth

Practising. I have to make a confession. The reason why I ended up as a scientist is because I wasn't very good at any language. So don't read too much into my precise use of words. But you are right. Why did I say that? I suppose I was trying to distinguish the part of me that had studied to be a scientist, had read a lot about science in different areas, or philosophy, we could all become arm chair experts at one thing or another without actually doing it for a living. I suppose practising as opposed to hobby. That's really all I meant. I spend a lot of time working at a bench pipetting, and much less time running down the corridor shouting heureka, and more time talking to students who occasionally run down the corridor shouting heureka. That's what I meant. The environment for different types of scientists, that is a difficult one. Maybe what you are saying is, does everybody have to aim really high. Is it worth going into science without aiming for the Großglockner, or even saying Großglockner is not big enough for me, it has got to be Mont Blanc or Mount Ararat or Mount Everest? My experience as a practising scientist has been that expectation/ambition is probably more important than any other factor. And therefore at least at some stage aiming for a big peak does make enormous difference and can compensate for all sorts of talents that you don't have. Of course, it is not absolutely essential. I am sure there are scientists who made very important discoveries who are never tremendously ambitious. He knew he was better than anybody else, and he knew that his standards were higher than anybody else's. I think that is true for an awful lot of scientists and artists. None of us will admit it. Even the most modest scientists, a lot of them are somewhat introverted, not outgoing, but I am sure deep down there is an engine of ambition which may be beautifully masked by good manners, or characteristics which we could describe as humility. But I am sure inside the most modest scientist or artist deep down there is a burning ambition. I challenge anybody to say it is not there.

Question

I am Giulio Superti-Furga from the center of molecular medicine of the Austrian Academy of Science since three months. I had the privilege of being a student in a laboratory next door to Kim. When you had your presentation and you discussed about how to reach certain peaks, and how certain people were unrecognised for a long time, I was wondering whether you think that there are in fact good important discoveries that for one or the other reason actually are not detected forever. They just are made and may be published, but nobody detects, nobody makes the connection just because the main people may not be particularly good in self promotion or in communication. Great discoveries are always discovered or do they sometimes maybe go down.

Nasmyth

That is a good question. There definitely are cases like that. Mendel's discoveries were ignored. They were published, not many people read them. He did send them to the one person who could have and should have appreciated, Nägeli, and he didn't want to recognise it. But let's say, haven't sent it to Nägeli, then it would have fitted in a category of a great discovery simply not being recognised. Nobody spotted it. Except, of course, 40 years later at least two, possibly three people independently discovered it. This gets down to, is there a degree in inevitability of great discoveries in science, and is that something that this degree of inevitability, that there is a certain logic, distinguishes science from the arts. You might say it was not inevitable that Mozart wrote Figaro. It wasn't just a matter of time before somebody did that. Whereas you could argue for all of the greater scientific discoveries it was just once, the genie got out of the bottle and the Greek started posing questions. Allright you could have dark ages intervene, but sooner or later, the way our brains are wired up, we would have gone to discover Newton's law of gravitation and Einstein's laws. I think to some extent there is a certain logic. I don't think it detracts from the experience of individuals who find themselves at crucial points in time when you are the right person to exploit your predecessors discoveries. Even in the arts nobody would deny that maybe Mozart was lucky. Music had developed to a point where his particular talents generated what he did. From that point of view it is maybe not so different. There must be enough other people who share enough experience with that person, that sooner or later somebody is going to come along and have the same idea, or do the same experiment. My guess is, probably not very often. But I am not a historian of science. By definition we don't know about them, and one would have to go off and spend an awful lot of time reading a lot of really dreary papers of centuries past and see how many good ones are out there.

Question

Günther Kreil, Salzburg. Coming back to these discoveries which are not immediately recognised. I think it still happens today. I want to come back to another point which was the chain reaction on one hand and then the remark that really good science creates new horizons of ignorance. With the first part I can agree. A really good piece of science causes many others to jump on it and continue with hopefully good work as well. With the ignorance, I am not so sure. Let's take examples, the DNA sequencing method developed by Fred Sanger was a brilliant idea, which got him a second Nobel Prize, rightly so. Did it create ignorance? It created a chain reaction. He even pushed the human genome project. But is that really science or was it just technology? Is it just putting a man on the moon, getting the human genome sequence? Science as far as I am concerned, really brilliant science, that was Fred Sanger.

Nasmyth

Let's take the DNA structure. At the very beginning one didn't know there was a very defined structure. And then once one discovered there was a very defined structure. Yes, but how does

it get to that structure? And that is a problem that is still completely unsolved. And many chemists would say this is one of the biggest challenges in structured biologies. Was it just technology, was it just like putting a man on the moon? Well, you put a man on the moon and nothing happened, right. Nothing happened, and nothing will happen when we put another man there. But Sanger generated the human genome, and biologists have been scratching their heads ever since. So what were all those genes doing? So it generated more ignorance. Now, I may sound playing with words here. And of course, one is playing with words, and one is just using words to put emphasis when you say you generate more ignorance than knowledge. I think you take any big discovery, and it reveals all sorts of things you didn't know you were ignorant about.

Question

I mentioned the man on the moon because this is a technological problem and not a scientific one. The same is true for getting all these genome sequences. I do not want to belittle this at all. But at the present time this thing is just being used over and over again, hoping that somebody will come up, asking the right questions with all the information that is being produced. And of course, the human genome etc. will create eventually, literally our understanding of the evolution of man and other creatures will certainly improve a lot, and be a great help to everybody who works, having these sequences ready. The science was really to get the method going and showing it can be done.

Nasmyth

One needs to be very careful when defining the sort of questions that you hope that discovery would generate. There are many, many questions that we compose as scientists, as human beings that are unanswerable questions. This is probably Wittgenstein's greatest contribution to clearing this hole up. There are only certain sorts of questions that are even in principle answerable. And a lot of scientists, and a lot more philosophers, pose questions that even in principle are unanswerable. To me one of the most frequent errors that students make, they start getting into the lab, and they do experiments, they get up and get a talk, and at the end of the talk they then pose a sort of future directions, and there will be some huge questions, because they have been taught to try and pose a big question and try and show how their work made a big difference. Usually a question is a hopelessly grandiose question, but that is completely unanswerable by them, and possibly even unanswerable by an army of institutes. How does development work? What do all the genes of the human genome do? These apparently are their questions, but they are all fundamentally unanswerable questions, just as unanswerable as, what is God, or what does God do? I think this is what Wittgenstein really said amongst other things, that the whole key thing is to be able to pose questions that are answerable. That is as much a practical as a philosophical issue.

Question

This is correct. But the boundaries between the answerable and not answerable are not strict ones. They are moving. It is a moving boundary over the centuries. Peter Medawar calls science "The art of the soluble", (Die Kunst ist lösbar). What can be solved at a certain period of time is moving. I remember the times, around 1970, the romantic phase of biochemistry was over, and the next steps, e.g. isolating and studying genes seems hopeless. And we thought the whole area of research will enter into an academic phase where no really interesting things are happening. And we were completely wrong. Because quickly within a few years all the technology came along.

Schmidt

You mentioned, and the whole discussion stressed now the fact that these individuals are very exceptional, and they play a very big role. However, especially in science, of course, it is not only individuals, it is also groups of people. And in part it is also a sociological phenomenon. I know you are affiliated with Oxford, but may I say something positive about Cambridge. Cambridge now for about 150 years is good in physics, but always in another type of physics. They were once very good in nuclear physics. They left nuclear physics at a time when it was quite clear that they would no longer be able to compete with the people from the United States. And people thought that is the end of Cambridge, what will happen. They moved into solid state physics, became great in solid state physics. As far as I know they invented radio astronomy and opened up other new fields. So there is a place where somehow in the soil there is something which makes them imaginative, and creative, and move on. This is one point. The other thing is, most scientists, not Wegener, not Mendel, and not Einstein, but lots of other ones, they come in family trees. An important scientist is obviously able to attract young people and train them very well. You find these whole chains of relatives who do science. This is something I just wanted to throw in because it was very much stressed up to now that individuals play such a big role, which I agree, of course, but there is this additional thing. I suppose you would agree.

Nasmyth

I do completely. Scientists have to work in communities partly because ...

Schmidt

The question is, of course, what makes these communities tick.

Nasmyth

One is history, cultural history. You have a community of people who think in a certain way and who share an appreciation of what is an interesting question and what is an interesting solution. I think it is a cultural thing that Cambridge has in common. It is what you can destroy at a stroke. Cambridge was very strong in physics but so was Göttingen. It is not any longer in the same league. You can just completely destroy that, and it doesn't start again. That clearly is a historical process in that sense. But then in addition it is the intellectual community and the history behind it. If you are an experimental scientist generally speaking as time goes on the experiments get harder and harder, and therefore you need more money to do it. And then that requires the community as capable of raising the money, and the community as capable of organising the physical infrastructure. This is less true in the theoretical sciences. Obviously there is a sort of split between theory and experiment in this regard. Why can't all be theorists? That is an interesting question. The bottom line is most of us are not smart enough. It was Bacon who said that the subtlety of nature is even greater than that of human imagination. And that is why most of us have our best chance to discover anything by interacting with nature. There are very few people, Einsteins as well, but even they required somebody else to have interacted with nature. But they were able through their extraordinary imagination to create something new themselves. But most of us will realise we just don't have the talent to do that, but as long as we go off and measure nature, nature will tell us if we are smart enough to recognise it when it does. I think that is what experiments are all about. It is compensating for our lack of imagination.

Question

I am David Kreil. I just came to Vienna a few weeks ago from Cambridge. Coming back to the question of Cambridge having a special historical background, how things can get destroyed for example in Göttingen. What I really like to know is how can you start

something like that? You have a lot of experience interacting with young scientists, training them, students, leading a new institute. How do you start an environment that nurtures and furthers excellence? You mentioned ambition. Many students have these huge, ambitious questions that no one can answer. So ambition alone really cannot be it. But it is clearly one thing that is needed. So what else is needed? The issue of having some people who are extraordinarily good at science, and a lot of others who just hope to do something that you might or might not call science. Regarding technology for example all the genome sequencing, all this stuff, the 72nd genome, the data are there that allow other people to do very good science. So all together how can you improve the environment that you can easily have for young scientists?

Nasmyth

I think a lot of the things that are essential for creating a new community are obviously in common with things you got to do within the existing communities. I think it is much harder to start it up, because cultural history is very important. When I moved to Vienna nearly 18 years ago I moved to an institute which was set up. There was nothing in that part of Vienna at the time. Vienna's heyday had been a little bit earlier in the century. It was an extraordinary opportunity to be offered a job in the IMP that was set up 18 years ago. In retrospect the IMP has been very successful and has made a difference, and there have been some important discoveries, and it brought in a lot of young and talented people into somewhere that is creating its own cultural history. So what was it that led the IMP to be successful. There has been somebody like Max Birnstiel who was a very established, highly respected scientist who could have gone off and worked anywhere. He had worked in California, he has done probably his most creative work in Edinburgh, and then moved back to Zurich and had been at the pinnacle of Swiss science. If he wanted to go somewhere he could have gone anywhere. Why would a Swiss go to Austria? It is a bit like an Englishman going to Ireland. These things change, of course. The IMP would never have taken off if it hadn't been for somebody like Max for all sorts of quirky reasons. First of all people have the vision to try and create something in Vienna, and secondly Max wasn't getting on with his colleagues in Zurich, all sorts of accidents of history. He came here. But if it hadn't been somebody of that gravitas then it definitely would never have taken place. It took people here in Vienna, particularly people like Peter Swetly and Hans Mayr, the former Vice-Mayor, people with vision who said look, what we got here is not good enough. Just like a scientist says, our explanation is not good enough. It took people to say, we want it to be better, people who weren't satisfied and who had a vision of what it could be, and the powers of persuasion. I think this is the biggest miracle, along with getting Max, was the ability of people like Peter and Hans to put it in Vienna as opposed to Cambridge. That was remarkable. To persuade their colleagues in Vienna that it was worth as an investment as well. Whereas Boehringer and Genentech should have and could easily have said, why Vienna, let's put it in Boston or in Cambridge or in Heidelberg, there is something going there. And in fact, I think the amount of money that was put into it, been put into similar institutes in Heidelberg or Cambridge, it would probably remain without trace. Because putting it in Vienna it had exerted maximum leverage. There was a university with tremendously good students, so that was terribly important. It allowed it to grow without being overshadowed by pre-existing institutes. If you want to get something done it has got to be unique. It is just like a new discovery, or a new business, or a new product. You got to distinguish your product from other people's. Therefore putting it somewhere different was a factor in its success. There is a whole series of factors. The fundamental structure of the institute ended up being one that tended to put an emphasis on giving opportunities to young people. One of the things I could have said was, who does science? Who makes discoveries? We didn't say that. We said what sort of people. Who actually does it is very clear. It tends to be young scientists, or certainly not old

scientists. We certainly live in a world where the older you get the more powerful you get and the more resources you can draw in. It is a big problem in a world where it is becoming politically incorrect to force people to retire. If sexism was bad so is ageism bad. Why should old people be discriminated against. This is a very great danger because the older you get, in every society the more powerful you get, but certainly not more imaginative or innovative. And that is a real problem for science in the future.

Question

Actually the discussion is so tickling. My name is Carlo Rizzuto and I am a non-scientist who is not exercising any more, but I am exercising through trying to make good scientist in the best position to exist. In a sense I am trying to exercise through all the scientists, mainly finding the money for them, which is one of the difficult things. About the progress of science. The figure that they normally use is that of a drop of liquid on a surface. If you keep on adding liquid to the drop the drope enlarges. You have to have this pressure to enlarge the surface of knowledge or the volume of knowledge. By enlarging the volume of knowledge if you enlarge the surface towards the unknown, so you enlarge the ignorance, you enlarge the interface to ignorance. What you are saying in my view is normal in the sense that if you increase knowledge you increase the frontier in which you have to thwart the knowledge. The second point is about who is a scientist. You tended to do what I consider a mistake in trying to sell the picture only of the excellent scientist. Let's try to make an experiment. We have only the excellent scientist and we don't have the other ones, the ones who do uninteresting things, teach the students, make technology transfer, go around looking for money. Would this excellent scientist, left alone, be able to make good science? Without new students, without the environment in which they work, without the continuous increase of obvious knowledge. But this obvious knowledge has to be found and has to be produced. If you take the spectroscopy studies in the late 19th century before the discovery of quantum mechanics. The excellent individuals would not be in the position to make the sudden advances in knowledge. What is the problem? The real problem is that in the academic world the not successful individuals tend to try to be successful by blocking the successful individuals or by avoiding the drop to enlarge without their control. So the exceptional places like Cambridge become exceptional if you are able to impede the bureaucratic reaction against those individuals who have new ideas or who are posing new questions. But you have to have the environment. Otherwise you cannot make it. And then you have to have the young minds or the students, and you must be attractive for the students. So you have to have a place which is very attractive and where you have a very strong accumulation of pressure. But then you have to deal with these places in such a way that the pressure can go through in specific directions. There is a small thing about the European paradox in this. In Europe we tend to believe that if you declare that you are an ex-scientist this is bad. What happens in the United States? There is a production of ex-researchers which is much bigger than what is needed to top up the need for researchers. And the people who have been educated as researcher go into industry, many of them. And they are able to capture the ideas and the developments of the research. This does not happen in Europe, because of the researchers get trapped in academic posts. Very seldom they move. A small number of them. If you look at the statistics the number of research doctorates we produce in Europe is much smaller than in other countries, in the US for example. These are some elements that should be taken into account if you want to find the ways to increase the amount of research which is done but also of excellence. It should not be in a vacuum, should be open, and it should be driven by curiosity.

Nasmyth

Is boring science important? If so, this raises questions just how much science needs to be funded. You don't have to fund just the really good stuff. You have got to fund a lot of stuff

as well. Then there is the question, how do you break out of the prison of boredom? And how is that in America the science factory grows in a way that doesn't in Europe. Is boring science important? I think these are things which one can only look at from a very practical point of view. If you look at the biomedical science, which is the one I know, in America the funding per capita is severalfold higher than in Europe. It is almost tenfold probably. Therefore by definition there must be more boring science in America than anywhere else put together. So you might ask, well, if that is the case is American science much more successful than science elsewhere in the world? Of course, it is not this great fund of boring science that are falling into spectroscopy. The studies say, the answer is no. American science is very unproductive. Switzerland and the UK are way ahead. There are people who go out there and say that the savior of British science was Margaret Thatcher. Science like economics requires a business cycle. You require selection. She created selection, I tell you. She very nearly killed it if it had gone on for another ten or twenty years. But there are those who say we have gone through that and then you could reinvest, which is what has happened under Blair. There has been a lot of investment. You could say that it has been invested probably better than it would have been if that selection had not gone ahead. I think you have got to say, do you need the level of funding of biomedical research that goes on in the United States. Is it even effective at all? Again I can only speak from my own experience in the biomedical science. It may be a little bit different from the physical sciences. Some physical scientists say biology is not even a science at all. It is a very tricky business. I will say something quite controversial, and others may completely disagree with me. But 90% of the stuff out there is just totally wrong, total rubbish, and not even done by scientists. And I would argue that America is funding more of that stuff than any other country put together and is actually slowing it down. It is not helping, it is hindering. There is a literature out there that is fantasy world. It is because there is too much money there. You obviously have a point. You do need the spectroscopies filling in, and you need the people who are going to teach the students, and one or two of them are going to be brilliant. That is all very, very important. You can't just do it on an elite. But I think nevertheless you have still got to control as much as possible the boring stuff. How you break out of the prison of boredom, these are miracles

Question

My name is Nora Aschacher, I am a journalist. I like very much a definition of chain reaction of questions and answers, because from my point of view a lot of research and results are sometimes presented as the final answer of everything. I always have the feeling that after that study nothing else could continue anymore, because now we have the final answer. Also for the public science would be more acceptable if the scientific people would transfer this feeling of a temporary answer. The other point is, I would like to know if somebody is asking the wrong question or perhaps giving the wrong answer, where is the point that this question or the wrong answer is pulling out of the scientific community, where are the boundaries?

Nasmyth

From the point of view of children at school, people reading newspapers. I think you put your finger on a very important point. One does tend to stress what has been discovered, the knowledge over the ignorance. It is a huge challenge to really convey the joy of science. The mystery of science, the reason why it turns people on is very rarely oh, that is how it works. It is this curiosity, here is something which is very interesting, we don't understand. I think that is what turns children on probably. It is not oh gosh, that is how it works. But it is being turned on by suddenly realising. You have been through this machine of education, being told this is what you got to learn, and then suddenly losing your intellectual virginity realising that actually there is a whole lot of stuff out there that people don't understand at all. And you know that just as well as anybody else. I think that can be conveyed at an much earlier age

than it probably is, because our educational systems are pumping our children so much full of stuff that they have got to learn, answers rather than questions. I think that gets to the heart of how do you best educate them. You have got to learn languages, you have to learn mathematics, and you have got to be able to express yourself clearly. How much stuff do you actually need to be able to go off and do it? A century ago we all said you have to do Latin and you have to do Greek and you have to do German in order to be a scientist. We now agree you don't have to do those things. But by implication and by induction therefore there must be all sorts of things that we say you have got to learn now to become a scientist which also is completely unnecessary and is just getting in the way and delay the encounter between the potentially young scientists and the boundaries of knowledge. I think you have got to get people to the frontiers as soon as possible. This is true at schools. It is also true in research communities. In America now the average age of the first grant that people get from NIH crept up to above forty. Forty years old. We got one foot in the grave as far as our brains are concerned. This is just a scandal. Because we are asking too much of people. They have to be too qualified. There is more stuff they have to know. People had to know so much twenty years ago, and then they have also got to learn the stuff we discovered in the last twenty years. You just put people off that way. It is a real challenge to bring people to the frontiers with as little fuss and as little having to take on all that huge knowledge basis as possible. One of the great challenges for educators is to distill the knowledge base and work out what you really need to know and what you absolutely don't have to. Probably it is 95% of the stuff that we are all taught at school. It is wrong answers usually arise because the experiment was so complicated that you couldn't do enough controls to rule out the unlikely explanations for all that. And that is equally true now in the physical sciences as it is in biology. These very complicated experiments in nuclear physics. You frequently hear people have made some discovery, and then they realise there is another explanation for it. Anything that gets really complicated, it is difficult to know whether the answer is right or wrong. Really good science, it should be clear sooner rather than later. How do you weed out the wrong questions? One of them is, is it even in principle answerable. I think 95% of the questions that people write down, if they are writing their grant proposal, probably if you actually sit down and say well, is the question even answerable? And the answer would probably be no. You just say, okay start again.

Question

If it is the wrong answer, the scientific communities will say, it is not science any more.

Nasmyth

You say, can a community get the wrong answers, and then it self perpetuates a series of miss and untruths. And then it just begets a question of fashion, of what a community believes. In other areas it is clear that you can go on arguing forever. You have a fashion that says this is true, and then another fashion who says that is true. To some extent science, if it is good science it is reduced to such a simplicity that is apparent to everybody that it is right or wrong. That is again what Wittgenstein said. I think that is his great contribution to the whole debate. It has got to be clear whether it was right or wrong. It has got to be clear that it was answerable. You don't have to be a philosopher to argue about was it right or wasn't it wrong. If you reduce it to simplicity it is clear to everybody, even to non-experts. But I am an optimist. I believe that there still are a lot of good questions out there to which there are simple answers. It doesn't have to get more and more complicated, so complicated we will never know whether it is right or wrong.

Question

Laurenz Niel from the Austrian Science Fund. I would like to come back to your comparison of the scientific process with a chain reaction. As we all know a chain reaction which is not under control may have disastrous effects. What is needed to have a sustainable chain reaction is what the physicists call a moderator that slows down the neutrons and brings them in a state where they then can be accepted by other entities to have new reactions. So you need something which works there to sustain the reaction. What can be this in the scientific process? I would say it is the discussion within the scientific community. My first question is, whether you agree with this view? And the second is then, the publishing system is not only a moderator, it is also a filter. It tries to filter out the outstanding ideas and to promote those. There, of course, sometimes it fails. We discussed already about Wegener and Mendel. But there are many more, many Nobel Prize winning ideas which at first were rejected by journals. The second question, whether you have some suggestions how we could improve this process to really detect the outstanding ideas.

Nasmyth

Okay, moderators. Certainly being able to persuade your colleague to give you more money to do more experiments is a very healthy moderator and a necessary one. I would add, beautiful theory destroyed by ugly facts, these ugly facts act as enormous moderator. You suddenly think you explained anything, and then somebody does some devastating experiment, and what looked like a wonderful body of theory created by one of these chain reactions, falls to dust. So ugly facts I would include as a serious moderator. The world is very complicated. There are moments where you suddenly, a community or an individual feel you have mastered the university, worked it all out, and then this horrible ugly fact just comes along and says it is all wrong. That is a very important empirical process. Definitely there are times when it gets out of control and ends up in censorship. The great challenge for the publishing system is to prevent it becoming censorship. Certain scientists want to prevent other scientists from publishing their ideas, from publishing facts. It certainly is part of politics. How do you control this act of censorship? I personally as a scientist am on the side of letting people publish their damn silly stuff, if it is stupid enough nobody will read it. And if they do read it and they realise it is stupid, they are seen to be the fools they are. I think the publishing system is not very effective for sorting out rubbish when it comes to publishing. And it does act as a moderator, but often when it gets to censorship I think it actually slows down the reaction in a manner that is really counterproductive. How to improve? One way that I think it could be improved enormously, and this is along the lines of let's try and make it, the output, let's try and aim for bigger discoveries, is simply just to say look, we are not really interested in how many papers you published in the last five years, whether or not we are going to give you money. Track record is one of the things. And what people propose is another thing. These are the two things that are being judged. So with regard to track record let's get away from trying to measure your output. Just say look, what is the one important thing that you discovered in the last five years, and take that into consideration, rather than say, how many papers did you publish in *Nature*, and how many papers did you publish in *Science*, and add it up. Just say, what was their most important thing. And then ask was that good or was it mediocre. And ask them what they think was their most important thing. And just judge them on that. And then you can forget about all the less important stuff, because your own criteria you know it is less good than the most important thing, by their own judgement. When you start adding it all up you end up making it impossible to make comparisons. But when you say okay, what was the best thing, then it is much easier, it clarifies the whole process. If you say well, this is what they think is their best thing, this is what A thought was their best thing, this is what B thought was their best thing. Then generally it is easier to see which one should fund. If you can get it down to a series of binary decisions. We are very good at making binary decisions. We are very bad, as human beings,

at making decisions about quantitative things. If you can reduce it to binary decision you would improve the process.

Question

Just for clarification. Is it always easy to find out what is your own best publication? I refer to Einstein. He didn't like his very best ones. Is that true?

Nasmyth

Well, Einstein was never given the Nobel Prize for his best work. But he was given a Nobel Prize. I think there are situation where if you ask people to say what was the best thing you did in the last ten years, some people would put down the wrong thing. But that is okay. If they can't work out themselves what their best thing is, then that is a criteria for selection in its own right. So I don't think you have to worry about that. It will be true in cases, you are absolutely correct.

Schmidt

Actually we have a binary decision to make rather soon between intellectual food and biological food. May two or three people could still get the microphone.

Scholten

I have two questions. I guess that by far the majority outside of this room would endorse your request to science that they don't care so much if it creates answers or questions but it should create something economically promising. Would you say that this restriction is changing substantially over the years, or is that a permanent companion of discussions like this? In other words, you were mentioning the comparison between art and science quite often. Does it just happen that l'art pour l'art is about art and not about science? I think one of the big differences between art and science is that due to the very individualistic career of artists the phenomenon in sciences – you mentioned Mozart – that the Salieris of this world decide who the next Mozart should be.

Nasmyth

Let's start with the economic consequences. Because of this chain reaction really good science almost invariably in the long run does lead to practical spin offs. Science is about understanding the world around us. It is the world around us that is important ultimately about technology. Good science allows you to fly rockets to the moon or design intercontinental ballistic missiles, whatever it is. Eventually good science invariably does produce useful things by the nature of the process. Again if you look back what scientific discoveries were useful and what weren't at the time, I would have been very difficult to make that judgement. Having said that, of course, there are a lot of very important things which were very important economically which didn't start off as fundamental in research. One has to accept there are clearly practical discoveries that people make that you wouldn't say fall into the category of fundamental research that turn out to be terribly important and having all sorts of tremendous spin offs and longterm consequences. But on the other hand there clearly are very many economically very important things that do stem out of fundamental science. It is really only those ones that one can address. In this sense science is like art. You have got to fund it as a cultural activity. And you know if you do it well and you make good judgement, in the long run it will produce spin offs. I think if you try and measure its potential to create products then that is very difficult, because it is just so hard to predict. Then you fall between two stools. When you boil it down, most of us can agree what is good science and is not good science. We agree it is not impossible. But I think what is absolutely impossible is to judge things that might be useful, judge whether things that might be useful are going to be useful. I think that

is something that is incredibly difficult. Ultimately the market place is really good at doing that, I don't think civil servants, I don't think public bodies, I don't think politicians who are absolutely useless at getting that right. On the one hand you have science where you can say is it true, does it make a difference, is it important, does it help to explain a lot of things. Those are things that are fundamentally answerable questions which is why I think we can judge good science. Then on the other hand there are things like can you make money. People want to buy them, or they generate economic activity, they save lives. Those things, the market place, the market generally is good at judging those. Then there are the things in between which are one of the criteria by which you use. Those are the hardest things and which people continually say, we need more translational science, we need more applied science, we need to fund more. That is the hardest one to judge. That is why I think by demanding that we think we can see the application, and that be a criteria for whether you fund it or not, I think it's the least effective way of ensuring that good science gets turned into technology. There are much better ways of doing that.

Question

The industry has become predominant over the recent years.

Nasmyth

I think it has. If you look a biomedical science people are talking more and more about that basic science has got to be translated into the clinic. In my experience it is the basic discoveries that are translated into pharmaceutical companies and then translated in the clinic not the so-called translational clinical science. That is just my experience. I think the same thing is probably true for industry. I think it is a very important promise how to maximise the chances that the basic science does produce the spin offs. It is so open to charlatans. It is so easy to say well, this could be applied and make a good story. But then, how do you judge it? The best way of judging it is be a capitalist, invest in it, and let the market place decide. You have got to create interface between basic science and capitalists, not basic science and technology transfer.

Question

I feel that the market place does not exclude the charlatans, but it just fixes a price for them.

Nasmyth

Insider trading. And insider trading is like bad science in that it is not transparent, what the rules of the game are. Enron was insider trading and it was bad business just like science that doesn't produce something simple and transparent ist bad science. Insider trading, it is not clear what is right or what is wrong.

Question

Andreas Stadler. I come back to the subject, what is science all about, what is the aim of the game. You seemed to come up with a very individualistic, almost anthropological approach to making science, individual curiosity was in the center. But I ask myself what makes some scientists produce more science and others less science? Why is it that we have 200.000 European scientists in the United States? Why is it that we have in Europe seen a science boom over the last years? It is the core element of the Lisbon Agenda. As far as I know all the terms connected to science productivity have not really gone up, but if they have not become worse they are at least on a stable level. Why do you think that in comparison to the United States in particular we do in Europe still find ourselves in such an uneasy situation?

Nasmyth

I go back to music. Most of the great music we all love required a patron, required money. Who supported Haydn? It was the Esterhazys. Science has to be patronised, and it requires a lot of money to patronise science. I think the reason why so many Europeans go to the United States and the reason why the Lisbon Agenda was a load of hot air is because Europe is not creating enough wealth. America is creating more wealth, it has traditionally created more wealth, and Europe is not doing it. And this has to do with economics. This has ultimately to do with politics as well. I can give you my views why I think the Lisbon Agenda is a lot of hot air, if you really want, but it would be a political view, not a view of a scientist. One of the things that makes economies work is children. There aren't enough children in Europe. The birthrate is so low. There are too many unemployed because it is too expensive to employ people. Look at Slovakia. It is a complete miracle what is happening. They get this flat tax, if it goes through in 20 years time Slovakia may be one of the tigers of the East. Economists say these things make a difference. They may be right, they may not be right. If you are on the left or on the right, you have different points of view. But the fact is the American economy has consistently outgrown the European ones for all the problems that America has. There is just a lot more money in America.

Schmidt

But didn't you blame the waste of money in doing science, in doing bad science.

Nasmyth

The more money you have got the more there is to waste. If you consider a country that has wasted so much money on all sorts of things, whether it be star wars which every scientist would agree is a complete waste of money. They create more wealth, and that is what attracts so many scientists there. We are not creating wealth in Europe. And that is what I suppose the Lisbon Agenda was all about. Who takes it seriously, because nobody was willing to do what was required.

Schmidt

I would rather like that you close on this note actually that Britain is doing so extremely well.

Nasmyth

I would say still better than it was.

Schmidt

I don't really want to stop the discussion here, but in a sense I have to. I suggest that we give a big applause to Kim for this marvellous discussion.