Surprise and sociology in multi-disciplinary sciences*

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I have been asked to talk about the nature of science – how we really do it and, in particular, about the interaction between different disciplines, in multi-disciplinary sciences. I think that in a sense, every science is multi-disciplinary because almost every subject calls upon ideas from many different disciplines. Mathematics may be something of an exception.

I'm sure most of you are familiar with the usual paradigms of science – that pure science, for example, discovers new ideas and then passes these on to applied science which adapts them for industry; that we have to plan where we are going and decide what is important, where to spend money and what people ought to be doing, in sympathy with the plans and needs of the nation or society. Another part of the paradigm is that science is very specialized and so scientists themselves become very specialized if they are to be successful. Basically we have a picture of the lone scientist – highly specialized and knowing his field thoroughly, but not necessarily too much else. Yet another part of the paradigm concerns the scientific method, or rather the usual popular definitions of the scientific method – you have all heard most of them I'm sure. These paradigms are not entirely false, but I want to talk particularly about the places where they badly represent the real situation.

Percy Bridgman was a very famous physicist at Harvard in the first part of this century. I think somewhat to his own surprise, he became known as 'quite a philosopher' in the latter part of his career by philosophers themselves. One of them asked him: 'How would you define the "scientific method"?' His answer was that the scientific method means working like the devil to get the answer, with no holds barred! In a way, I think that is a more direct and accurate answer to what a scientist does, rather than following a systematic, pre-coded procedure as usually stated. So much for the scientific method. Now what about planning?

I would like to illustrate the problem of planning with a particular case. Perhaps the most striking *real* effort to plan a nation's technical program was initiated by Franklin Roosevelt, back in the 1930s. He called on a group of US scientists and engineers, the scientific and engineering statesmen of the day who were recognized experts in their fields, and asked them to provide a report on what would be the most important scientific and technical areas for the country during the next 25 years. After meetings and conferences, they produced a report in 1937. One of their recommendations was to stress the importance of plant breeding; it would change agriculture, which was a very important area – perfectly right. Another was that rotating machinery could be made more efficient and hence save electricity – again, somewhat important and correct. Yet another was to develop synthetic gasoline and synthetic rubber; the Germans were already using synthetic gasoline rather freely, which was not entirely new but was considered an important area. But as I read the report, what struck me was what the experts had left out! For example, they left out antibiotics. Fleming had already done his work on antibiotics about eight years before the report was written but it was overlooked; further work by Florey came one year later and put antibiotics on the map. They missed nuclear energy. Perhaps they were influenced by Lord Rutherford, who just a few years previously had said:

> The energy produced by breaking down the atom is a very poor kind of thing. Anyone who expects a source of power from the transformation of these atoms is talking moonshine

– which was completely logical. Lord Rutherford was one of the great scientists of the century and he knew that field perhaps better than anyone else. He happened to make that speech in the United States and many American scientists agreed with him. One in particular was quoted in the newspapers as saying: 'I am pleased to see Lord Rutherford call a halt to some of the wild, unbridled speculation in this field.' The report to President Roosevelt was in 1937. One year later, Hahn and Meitner came along and the whole situation changed. What happened? Fermi had, of course, been sending neutrons through materials. He had discovered what were supposed to be, at the time, trans-uranic elements. Physicists were much impressed. Hahn, a chemist, decided he would like to find out some of the chemical properties of these trans-uranic elements. He found out that the newly created atoms were, in fact, really in the middle of the periodic table; and so fission was demonstrated. Immediately everybody started

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talking about the resulting energy and how much might be produced if one could make a continuous reaction. Fermi calculated how much uranium it would take to blow up Manhattan, and so, on. So, suddenly, there was a new realization.

Roosevelt's committee of experts also forgot, or did not think about, jet-engine aircraft – which came along shortly after World War II – or rocketry, which happened to be under development at Caltech at just that time. I was a student there and two of my good friends were working in that field; but it was only because I knew them well that I knew about it, because it was 'hush-hush,' though not for military reasons. The famous aeronautical engineer Von Karman was sponsoring the work because he thought rocket propulsion could be important. But rocket propulsion had become the subject of much science-fiction. It had become so discredited that he felt that it would not do for the public to know that Caltech was working on rocketry, and the students were warned not to talk about it. He was right of course. And rockets led to satellites, and space work. Surveillance satellites have been enormously important to us as have other forms of space work. Radar, which burst into importance during the war only a few years after the report, was also missing from the report. Then there was the transistor development. And computers began to change our lives. The laser came within 25 years, and also genetic engineering. The list could go on further. Basically, the committee members missed all the importantly new and most exciting things. This is not because they were stupid people. They were perhaps conservative. But governments and committees are characteristically conservative, and unlikely to make predictions about the new and the surprising. So much for planning and how to do it.

Let us look at the flow from pure to applied sciences. The paradigm is of course that applied science depends on pure science, which is what most of us here do. It is a nice feeling to say we scientists make all these big contributions to applied science. Actually, the contributions coming in the other direction are just as important. Firstly, some very fundamental work comes from applied science. Let me take just one particular field. At the Bell Telephone Laboratories, engineers were of course very interested in communication. As a result they wanted to know what produces electrical noise and where it comes from. So they assigned an engineer to look into noise in electrical circuits; he was J. B. Johnson, who discovered what is now called *Johnson* noise – a fundamental phenomenon present in all resistors and a general thermodynamical result. The next notable look at noise was by an engineer named Jansky, who investigated the sources of radio noise. He was asked to find out where the external noise received by antennas was coming from, and at what frequencies it might be minimal. Jansky did it and, as you probably know, he discovered radiation coming from the center of our Milky Way. It was the beginning of radio astronomy which we now think of as a very important field of basic science. While searching for sources of noise might be regarded by most scientists as a rather messy and applied subject, the next occurrence of note is perhaps even more fundamental; that is the work of Penzias and Wilson. They set out to look more carefully in the microwave region for what kind of noise was there as a function of frequency and direction. They did indeed look carefully and found a strange thing: there was noise coming in uniformly from everywhere! That was, of course, evidence of the Big Bang. What can represent any more fundamental a discovery? And of course, all these basic discoveries came from applied science.

Secondly, we scientists are tremendously dependent on the availability of technology for experimental work. When Millikan did his oil-drop experiment he made, with his own hands, one or two thousand lead cells. To get a high enough voltage (a few thousand volts), he had to make them himself. Today, of course, for a very modest sum of money we can get voltages more or less as we want. And almost all of our experimental science depends on the availability of sophisticated high-technology components and equipment. So really, science and applied science interact strongly and are highly dependent on each other. I think that, in the long run, there will never be a flourishing science without a flourishing technology and *vice versa*. Well, so much for that paradigm.

Let me talk now about specialization and the 'loneliness' of science and scientists. In fact, scientists are social beings and much of our development depends on social interaction, cross-fertilization, and how we live with each other. There are many different kinds of cross feeding between sciences. Consider Boolean algebra, for example. I remember when Boolean algebra first came into the communication business. Claude Shannon, a mathematician, had somehow started playing with communication theory and had demonstrated a few relations. He was hired immediately by Bell Laboratories and, from then on, Boolean algebra became an important part of communication theory. Einstein, you may remember, had to learn differential geometry before he could really properly frame his general relativity. There was another mathematician at Bell Labs named George Stibitz. Some of us were doing automated computing at that time, but it was all analog computing. Stibitz argued that real accuracy could not be had from analog computing, it had to be digital. Well, there were no digital computers at that time and he failed to convince anyone to build one. He made a big rack full of relays and wheeled it around from laboratory to laboratory to show that here was a mechanical relay rack which could compute things accurately. Most people sort of laughed; they did not want to build up a big relay rack to do their computing. Of course, as time went on, it became clear that this mathematician had been basically right. But instead of relays, we were able to move to vacuum tubes and then to transistors, and now George Stibitz is in the Inventors' Hall of Fame in Washington.

One failure worth noting which was due to lack of communication and interaction was associated with Gamov, Alpher and Herman who worked on early cosmology. It was not that they failed completely; they made important calculations on the existence of Big Bang radiation and predicted how strong it should be. But they did not know it could be detected; they thought it was out of question at that time, and the appropriate experimentalists who could do it and might have been interested apparently did not read Gamov *et al.*'s paper. So the work did not proceed further at that time. When Penzias and Wilson discovered this radiation, they did not know about the Gamov *et al.* paper, nor that Dicke was developing equipment to look for it. Dicke did not know about the Gamov *et al.* paper either; he had reinvented the whole thing. This is a classic case of serious lack of discussion and communication.

One of the most striking periods of strong interaction between pure science, applied science, and engineering, was immediately after World War II. During the war, most of the academics who were good had been pulled away from their normal academic research into war research. People were found who could stay in the universities to do some teaching, but there was very little research done there. Many of the academics were in various kinds of establishments trying to make radar and nuclear bombs and all kinds of things to do with the military. That group became well acquainted with applied science and with many engineers. When they went back to the universities they had been indoctrinated with a great deal of new technology; physicists did not know very much about the latest in electrical engineering before World War II. After World War II they recognized the opportunities this knowledge provided. And so we had nuclear magnetic resonance discovered simultaneously (in two places) by people with such backgrounds. Another important contribution in this field was from Erwin Hahn who, as a student, was playing around with resonances, when he saw an unexpected effect. He stuck with it enough to recognize that he had spin echoes which, of course, chemists have used heavily and biologists have latched on to as a way of imaging the interior of the human body. Microwave spectroscopy, my own field, also developed immediately after the war, really because there were not only physicists with appropriate experience but also a lot of radar equipment available and free; it was regarded as junk and we could use it. And from this came molecular and nuclear studies, the Lamb shift, and use of the hydrogen fine structure line for astronomy.

Radio astronomy also grew remarkably as part of the radar inheritance of World War II. Radio astronomy is an interesting example of some lack of understanding and imperfect exchanges between fields, indicating that our ideas are often a bit too fixed. After World War II, I had been thinking what should I do. Microwave spectroscopy was one good possibility, and radio astronomy another – both seemed very interesting. I went to see an old professor of mine, I. S. Bowen, a wonderful person and a good astrophysicist. At that time he was head of the Mt. Wilson and Palomar Observatories. He had always been very kind to me and I admired him. So I went to him and said: 'Look, Bell Laboratories [where I was], seems like a good place to do radio astronomy. I myself find it an interesting possibility, I think the Bell Labs would support me, and they have got all the equipment and lots of knowledgeable people. What do you think would be the most important things to try to do in radio astronomy?' Well, Ike looked at me and said: 'I am sorry to disappoint you, but I do not think radio waves are ever going to tell us anything about astronomy.' Now that was not an uncommon attitude, in particular in the United States. In fact, the United States really fell on its face as far as radio astronomy was concerned. It was left to the Australians, such as returning radar people like Taffy Bowen who, I guess, redeemed the name of Bowen so far as radio astronomy is concerned. And there were many others such as Bolton, Pawsey, Mills, and Wild. Of course, after the war England quickly went into the business of radio astronomy too, and so did the Dutch. There were some astronomers who appreciated the possibilities, but the field depended primarily on engineers and physicists setting up and doing things which astronomers, at least in the United States, were very uncertain about. I know one astronomer in a big university department in the United States who was at that time looked down on, even ostracized, for deciding to do radio astronomy. I think perhaps it was Oort's distinction and interest in radio astronomy, plus the success of the Australians and the British, that woke up the United States and got it going; and of course they then did some excellent things. But there had been a lack of communication and a fixed mind-set – which should not be interpreted to criticize anyone. It is a problem we all have; it is simply human nature. I cannot think of a wiser, nicer person than Ike Bowen, and he was a wonderful astrophysicist. The same good things can be said of Roosevelt's panel who were also thoughtful, intelligent people; few people at the time would have seen things much differently from the way they did or written a much better report.

Let me now come to something a little closer to my own experience. I want to mention two things that happened at Bell Labs; one they did right, in my view, the other they did not. What they did right was in solid-state physics. Back in the 1930s Mervin Kelly, the research director, recognized that solid-state physics was beginning to be understood. It was a burgeoning field of physics and people were really beginning to understand solids and electron conduction. Kelly realized that Bell Labs had a lot to do with solids; they had resistors and conductors and so on, and he felt that they ought to hire some physicists who really knew about modern solid-state physics. Some people have claimed that he foresaw the transistor but, in my opinion, that is not true. Bill Shockley was one of the people he hired. During the very early part of the World War II, Bill tried to make an amplifier using solids (or a transistor), and talked with me about it. He was a theorist himself, not an experimentalist, and so he got another person at Bell Labs to try it out; he worked hard on it but it did not work. I remember Bill telling me he did not understand why. 'You know,' he said, 'I thought I had a great idea, but it did not work so I guess it just does not. I do not know what else to do about it.' A few years after that, the transistor was invented basically by accident, but as a result of thinking about and working on solids. Walter Brattain was in the lab doing some surface physics and making measurements and suddenly he recognized he was seeing something peculiar. He called in John Bardeen who was next door, and John, a theorist, looked at his measurements, thought about them, and came up with an explanation. 'Wow,' perhaps he said, 'this is amplification!' Immediately, everybody jumped on it. Bill Shockley was abroad at the time, but when he came back he worked on some further good ideas he had, and the transistor revolution was underway. That, I would say, was wonderful planning for a research director.

Now let me come to the maser and the laser. Pardon me for talking about my own field but, after all, that is the one I know best, and I can tell you a little more definitely about just what happened. Many of the important details of history and interaction in science are often forgotten; we know about such-and-such an idea, that somebody had it at some point, but we do not know just how it happened, who said what to whom, and so on. Those things can be terribly important in the real development of science. So let me tell you something about the history of the maser and the laser. Basically, I wanted to do microwave spectroscopy and study molecules. I thought it was a wonderful high-resolution spectroscopy, something really new. And I wanted to sell it to Bell Labs. I wrote a memorandum showing that molecules interact more and more strongly with radiation as we go to shorter and shorter wavelengths, and that they could act just like circuit elements. Hence, in the long run, they could be important in communication work if the field went to high frequency. I knew the second law of thermodynamics and I pointed out very carefully that, unfortunately, you can not get much energy output from these circuit elements because when you heat the molecules up they produce just as much energy as that temperature can give you from a black body and, before you get any very useful energy, the molecules will fall apart. So, you cannot get energy but at least you might have some circuit elements. Well, as a result they let me work on microwave spectroscopy. It became a burgeoning field and sprang up in a number of different places. Furthermore this molecular work turned out to be the field that was necessary for the development of the laser. Now why can I say that? The idea for the maser came up in three places independently: Columbia University where I had moved to (I will mention that more in a moment), the University of Maryland where Joe Weber was working, and in the Soviet Union where Basov and Prokhorov were. They were all microwave spectroscopists! Three ideas, apparently independently and not so far different in time, all coming out of this field. Why did it happen? Well, it was a particular combination of engineering experience, interest in quantum mechanics and physics, and ideas that were forced on us as we looked at these molecular spectra. Now the whole field of masers and lasers could have come out of astronomy if anyone had found, say, the water masers – which they might well have done. There was plenty of sensitivity to do it. If the astronomers had been looking in the right place they would have seen these powerful masers. They may have thought at first that such strong signals must come from extraterrestrial intelligence, but on second thought and with more measurements, they would have figured it out, and perhaps then someone would have tried to make one in the lab. In that case the field of quantum electronics might have come out of astronomy. But astronomers were not looking at those frequencies at the time, or thinking much of the possibility of molecules in interstellar space.

I had left Bell Laboratories by the time masers and lasers were invented because industries were getting out of the field of microwave spectroscopy – they did not believe it very useful. Bell Laboratories' attitude was that it was a nice science and, since they liked to do nice science, I could keep up my research, but they were not going to expand in this area. At General Electric, a friend of mine was told to stop doing microwave spectroscopy and, instead, measure dielectric constants of solids at microwave frequencies because that would be useful to the company. RCA shut down microwave spectroscopy and the person there also went to a University. Westinghouse also gradually shut down microwave spectroscopy because they did not see any great interest in it. Fortunately for me, I had gone to Columbia University where people were more interested in those kinds of things. I wanted to get to higher frequencies and struggled for quite a while to do that when, suddenly, I realized that the second law of thermodynamics does not have to apply!

I have often said that none of the individual ideas behind the maser was new. It was a combination of ideas, most or all of which physicists already knew. The combination was different enough that I think it required a mixture of engineering, electronics, and an interest in microwave spectroscopy and quantum mechanics. The engineers knew all about resonators and amplifiers, yet they had never heard of stimulated emission. The physicists, if they were in the field, knew about stimulated emission but they were not very interested in amplifiers or resonators and usually did not think in those terms. And when the field was extended to lasers, it was impressive that most physicists liked to think in terms of particles, not waves. It was striking that the biggest objections I had to my conclusions were from physicists, including very renowned physicists such as Niels Bohr, Von Neumann and L. H. Thomas. They argued that you could not get an oscillator with the narrow frequency I was claiming; the frequency could not be defined that accurately because the molecules go through the maser cavity in a finite time. In other words they were saying: 'What you are telling us cannot be right.' By then, I had an oscillator operating with a very, very narrow frequency width and I had worked through the theory and knew perfectly well, as any engineer would, that if you get enough amplification and feedback you are going to get a very sharply defined frequency from an oscillator. Engineers would not have worried about it; the physicists did, because they were looking at things as particles, and at the uncertainty principle. I remember Von Neumann arguing with me strongly at a cocktail party; he went off, had another drink, and came back in about fifteen minutes and exclaimed: 'You're right!' These people knew all the principles, they just had not thought in the appropriate directions.

As for me, I benefited very much from my surroundings. First there was a molecular beam laboratory right next to mine which is why I thought of molecular beams. Second, just a month before the maser idea came along I had heard a talk by Wolfgang Paul of Bonn on a new way of increasing the intensity of a molecular beam using quadrupole focusing rather than the old-fashioned way. I immediately caught hold of that and it's part of what made the maser possible. At Harvard, Purcell and Ramsey had published papers on negative temperatures, so population inversion was not unheard of. Thermodynamics, in a way, got turned upside-down by their negative temperature definition, but still worked in a sense. Everybody in the field knew about these things. Yet something was required to put it all together and suddenly, fortunately for me, it came together. I owe a great deal to all these interactions I had with other people.

I went to France on sabbatical leave not long afterwards where a post-doc (one of my former students) was working in Albert Kastler's lab. This young man, Arnold Honig, had found a very long relaxation time for an electron spin resonance in solid material. I said 'Great! Now we can build a tunable amplifier.' I had thought about using electron spins for a maser amplifier back at Columbia, but did not know any system with what I thought would be a conveniently long relaxation time. So we started working on it for a brief time in France, and I told the Bell Labs people about it. Meanwhile, Woody Strandberg at MIT had become interested; he'd had a similar idea and gave a talk about it. Nico Bloembergen was in the audience and asked Woody why anyone would want such a thing. Woody replied: 'Because this is *the* most sensitive amplifier you can ever get.' Now, Nico had been working on electron spin resonance in solids, and knew a lot more about them than I did, or Woody Strandberg. I thought of electron spin resonance as that of a completely free electron in a magnetic field, though in principle I knew it existed in crystalline structures. Nico had been working with these crystals and so he immediately hit on the idea of a three-level system with pumping – you pump in energy at a frequency higher than the one to be amplified – which was the right solution for an amplifier. This is practical only when there are certain types of crystalline fields which modify the electron's frequencies.

Further along on this sabbatical leave I moved from France to Japan where I ran into a biologist friend of mine in Tokyo, Francis Ryan, an American who was also on sabbatical leave. We talked a bit and he told me he was reading a paper by the Britisher Coulson who had produced a mathematical theory of microbial populations. Now, Coulson was a theoretical chemist, and he had written down some equations of probabilities; these gave a microbe a certain probability of dying and a certain probability of dividing and thus multiplying, thus providing a creation and a destruction mechanism. He predicted the characteristics of fluctuations in populations of microbes – very interesting to a biologist at that time. I said to my friend that I had been trying to figure out how to model mathematically the noise and noise fluctuations in a maser amplifier, and that all I had to do was put in one more term, namely spontaneous creation of a microbe, and we would all have the same phenomena. So, of course, I studied Coulson's paper and tried to solve the equations. At lunch in the equivalent of Tokyo University's faculty club, I talked with my Japanese friends Shimoda and Takahashi about it. Takahashi was a pretty good mathematician; it was he who would really solve the equations for me, and so we arrived at the first good, rigorous way of describing fluctuations in maser or laser amplification.

Maser research was the subject of intense interest for quite a while, but during the time we were first building it there was by no means any great interest. All kinds of people came into my laboratory; I would show it to them and they would say something like, 'Well, you know, that's a cute idea,' and promptly forget it. There was absolutely no competition in making the first maser. Apart from my student Jim Gordon who built the maser for his thesis, nobody else seemed to want to build one; it just was not that interesting to them. When it worked, it did get some prominent notice and by the time the maser amplifier came along after a few years, it was a hot topic. People still were not thinking much about going down to shorter wavelengths but, since that had been my primary eventual aim, I thought I'd better sit down and think about how to do it. If you are going to go to shorter wavelengths, the next step is to get into the far infra-red. But after jotting down a few equations, I suddenly realized that it was just as easy to go all the way to the optical region! We already had all the techniques ready at such wavelengths. The only real problem I struggled with was to find a way of controlling the modes. I had some poor ways of controlling the modes which would awkwardly get a single mode. 'But,' I asked myself, 'suppose it is not a single mode; it will hop around from one mode to another, but it will still be interesting to get an optical oscillator.' I went over and talked to Art Schawlow, who was at Bell Labs at the time; Art was very interested and he said: 'What about a Fabry-Perot?' That was the answer! Why didn't I think of a Fabry-Perot? Perhaps Art thought of it because he had used a Fabry-Perot when working on his thesis. I knew all about Fabry-Perots, but somehow I just did not think of them. Well, we sat down and figured out how to pick out a single mode. We also had to find the most appropriate media for an 'optical maser' or laser, of course, but we had made the breakthrough.

By the time our preprint on 'optical masers' was out and available, other people were showing great interest. There followed a high level of competition to build working systems. Industries had already been hiring students of microwave and radio spectroscopy to build maser amplifiers and do related research because they had decided that there was something in this field after all. And if you look at the records, you will find that all the initial new lasers were built by people who had worked in microwave or radio spectroscopy, but who were then in industrial companies. This shows what industries can do when they get interested!

I could tell many more stories, but I think you can see the importance of intense interaction – the way ideas get traded around so that other people can see what you are doing, and then something in their background catches fire. This is what builds up a complex field of scientific development. Now, one might ask how can you plan this kind of thing? I think I foresaw the importance of the laser in communications pretty well. But it has also turned out to be important as a surgical tool, for example in reattaching detached retinas. I did not even know what a detached retina was at the time, but other people applied the ideas in this and other directions, and the field grew. Now suppose someone had said. 'We need a bright light and I would like to have my research laboratory build one.' What research director would have asked his laboratory team to start by looking at the interaction between microwaves and molecules in order to get a brighter light? No research director would have – nobody would. Instead we would have probably turned to General Electric or some other company which was making lights and said: 'Build as bright a light as you can,' and General Electric might have increased the intensity by a factor of two rather than the billion or so the laser gives us. And with such a development we would not have a new surgical tool or a new surveying instrument such as a laser provides. So, this is the problem: we know it is a great idea to keep our eye on the ball – everybody says so. Think about what you are doing; do what you need to do and keep your eye on the ball. But keeping your eye on the ball can at times amount to gross myopia, so that you are just not seeing anything else! This was well put by a Zen Buddhist philosopher by the name of Takawan, back in the middle ages during the time of the Samurai. He said: 'When you are struggling with your enemy, if your mind is fixed on the point of his sword you are no longer free to master your own movements. You are controlled by him.' So be careful! Look around you. Do not overdo the narrowness of your focus.

Now, I want to mention another interesting phenomenon that I think is important to science, as well as to other aspects of our lives; that is what I call the $1/f$ law. Those of you who have a little contact with electrical engineering will have heard of the $1/f$ law; it is a voltage fluctuation or noise where the power is inversely proportional to the frequency. At one time, Bill Shockley started to sort through the Bell Labs records to see how many scientific papers people were writing and how many ideas they were patenting. What he found was that the probability of a certain number of scientific papers being written was proportional to one over the number! In other words, if there are ten scientists who have written ten papers, there will be one scientist who has written one hundred papers, and a hundred scientists who have written one paper – the $1/f$ law. That is, the probability of a level of productivity varies as one over the productivity level. The same thing was true with patents. Bill did not know what the explanation was. I do not know exactly why he was gathering these statistics, unless he was pointing out that the people who produced more papers really ought to be paid more, and was one of those! Of course, some people are interested in money rather than science. And if you look at personal wealth, the same $1/f$ law applies: the number of people who have a personal estate of ten million dollars is a hundred times larger than the number who have a personal estate of one billion dollars and so on. John Bennett points out that this $1/f$ law, familiar to many engineers, may be closely related to Zipf's law, which was first applied to the frequency of use of words and then also to the distribution of wealth. Earthquakes, and storms, obey the $1/f$ law too—for example, the frequency of earthquakes of a particular size is proportional to one over the size. It is a fairly general phenomenon, though hard to understand. What produces such a law?

Ordinary reasoning might argue that productivity must depend on intelligence and, after all, intelligence has a Gaussian distribution; the $1/f$ law hence does not make sense. Of course, there are fluctuations, but how do you get these thousand-to-one fluctuations and a linear slope of the logarithm from a Gaussian? Perhaps the answer was given by Montroll and Schlesinger, who not so long ago showed that, if you have a whole series of Gaussian curves, each with a different spread, and all multiplied together – in other words, if what happens depends on the product of a lot of different kinds of things, each with its own distribution (more or less a continuous set of Gaussian functions), then that will lead to a $1/f$ law over a very wide range of the variable. So, what this may be saying is that there is a lot more to science than intelligence. There are a lot of other personal and social characteristics – and perhaps also accidents – that count. Surroundings count, the place where you are, work habits, attitudes, ability to carry through an idea, etc. It is a very complex business, and somehow from all these factors we get the $1/f$ law. That may well explain some of the electrical noise phenomena too. This multiple-factor effect is probably a very important one in most complex situations.

I recently did a little bit of statistics on the Physics Department at Columbia University. I was on the faculty at Columbia for twelve years. It was a nice place, and stimulating. I did not quite realize how stimulating until I started adding up some of the numbers. When I got there, there was one Nobel Laureate on the faculty, Professor Rabi. Nine more physicists who were on the faculty while I was there were to receive Nobel prizes, and among the students who were there when I was, five received Nobel prizes – all in physics! Two of the postdocs who were there when I was also now have Nobel prizes. Now how did that happen? Did Columbia chance on the combination of things which gave $1/f$ a very large value? What was so special about Columbia at that time? Of course, Nobel prizes often depend on a bit of luck. And neither they nor the numbers of papers are necessarily the true measurement of scientific achievement, but they at least serve as some kind of numerical measure. This is the kind of thing we all need to ponder – how to make things like that happen, both in pure and in applied science.

What really is needed for our communities to be productive? I believe that first on the list of requirements is a strong general interest, throughout society, in intellectual ideas – a curiosity, and a sense of excitement about ideas and discovery. I think a second important factor is diversity: of approach, of types of institutions, types of support, and also interactions with diverse people; in other words, diversity in general. A third, I'd say, is the support and encouragement of clever, productive researchers without trying to do too much planning for them. We should trust their insights about what is interesting and fruitful regardless of whether what they are doing is in the mainstream of science. We also must support those who are working in fields where really new understanding is developing. To this I would add the need for a certain kind of intensity: in research and in enthusiasm. These are some of the things that may pay off most strikingly.

And along with these, particularly in the areas which are multidisciplinary but I believe in almost any kind of intellectual development, we need interaction – a society which is interacting, open for people in different fields and with different ideas to know each other. Perhaps the best way of all to get technological transfer is for individuals to transfer from one field to another, and from one institution to another. The mix of people and ideas that occurred during and after World War II provides such an example. Scientifically and technically that was a very rich period. We must not reproduce it by wartime crisis. But we should look for ways of encouraging the ferment, the interaction and sharing of ideas, the dedication, the intensity, the freewheeling openness to ideas, and whatever other conditions are needed to provide a very large value of $1/f$. If successful, it can enrich our society with both productivity and fun.

Thank you very much for listening to some of the tales of science as I see it.