



## Interview with Freeman Dyson

Interviewer: Masataka Fukugita

Freeman Dyson visited the Kavli IPMU at Kashiwa from 15th to 19th of April, 2014, and at Kamioka from 20th to 22nd. Masataka Fukugita organized a special Q&A session with Freeman Dyson on 17th of April for 4 hours. This document excerpts this session, supplemented by an additional interview on 18th and another Q&A session held at Kamioka on 22nd.

**Fukugita:** You were educated at the Winchester College. Would you tell us about this legendary college?

**Dyson:** Winchester College was founded 650 years ago by William of Wykeham, a Bishop of the Catholic Church and

an advisor to the King. William of Wykeham was a capable administrator. He founded 6 foundations, including New College in Oxford. Students are allowed to enter Winchester College at age 12 after a tough, very competitive competition.

Freeman Dyson was educated from 12 to 17 years of age at Winchester College, a famous elite high school of England. He then went to the mathematics department of Cambridge University, where he was supervised by Abram Samoilovitch Besicovitch. Afterwards he worked for the Royal Air Force, while WWII was raging. A year after the war ended he returned to Cambridge, and gradually switched to theoretical physics. He then went to Cornell University and worked under Hans Bethe. He visited the Institute for Advanced Study in 1948, where his great work for QED was completed. Quantum field theory we learn today is the formulation thanks to Dyson. His work serves as the best example of a mathematical mind clearing up the mess of physics to create a beautiful theory. He was appointed as a professor at Cornell University in 1951, and then invited as a professor in the School of Mathematics of the Institute for Advanced Study in Princeton in 1953, where he served as a professor (now

as a professor emeritus) for 60 years. He has just passed 90 years of age, but still actively engaged in science.

During his long research life he has worked on an amazing variety of subjects, often two or more subjects at the same time. Starting with number theory, the subjects he worked on include:

- Quantum electrodynamics – foundations of quantum field theory;
- Statistical mechanics and solid-state physics;

- Random matrices;
- Stability of matter;
- Adaptive optics;
- Nuclear engineering, nuclear power as space propulsion;
- Model for the origin of life;
- Time variation of the physical constants;

Carbon dioxide problem, and also some others.

Now he is working on game theory, prisoner's dilemma, besides graviton detection, which was presented in his colloquia on the 16th of April at Kashiwa and on the 21st at Kamioka.

The exam is for ten subjects, including Latin and Greek. There were two for mathematics, one pure math and the other applied math. The exam also includes a modern language. My father was a teacher at this college, so I was already familiar with it as a college brat from the age of 3. The number of students is 70, 15 each year, boys only. This has not been changed until now. The college is still flourishing the same way as in the old days. I still visit it frequently.

The graduates are not necessarily academic; some become bankers and some go to government offices. England is a very class divided society, commercial middle class for business people and academic middle class including university and government people, and there is an antagonism between them. We, the academic middle class, considered ourselves the top students in the school and looked down on the others. That tradition was intact, until Margaret Thatcher took her revenge, putting the commercial middle class firmly in power.

We lived in the same building originally built in the 14th century. It has thick walls, cool in summer and warm in winter. It is comfortable to live there, not much need for heating

or cooling. Education is carried on in a very free style. In the top form we were in class only for 7 hours a week. So we found ample time to learn whatever we wanted.

**Fukugita:** Have you decided to be a mathematician from the time of Winchester College?

**Dyson:** No. I was much younger. Mathematics was born in me. I loved numbers and very much enjoyed calculating from the time I was around 3 years old. This is the same as with musicians who start playing piano at age 3. It is the same for mathematics. I knew that mathematics was what I can do best. I was already determined to be a mathematician when I entered the College at age 12.

In the college I found "Cours d'Analyse" by Camille Jordan in the library. It consists of 3 fat volumes, containing the whole of 19th century mathematics. It is a classic work, written around 1900 and used in École Normale in France as a text book. It was a miracle that this book was in the library.

**Masataka Fukugita is professor at the Kavli IPMU. He served as a principal investigator at IPMU from October 2007 to March 2012. He is also a part-time member of School of Natural Science of the Institute for Advanced Study in Princeton since 1988. So, he has got along with Freeman Dyson over 25 years.**



My fellow classmate James Lighthill, who became a famous mathematician, discovered this book in the library, and we both read and talked about it.

G.H. Hardy was already an old man in Cambridge and the most famous mathematician in England. He had been at Winchester College as a boy 40 years earlier. In his autobiography "Cours d'Analyse" appears, and I strongly suspect that it was he who brought this book to Winchester College. I cannot imagine anybody else bringing it. Unfortunately, I never asked him about this.

**Fukugita:** Then you went to Cambridge?

**Dyson:** In 1941 I came to Cambridge. I entered the mathematics department. Mathematics and physics were separate departments. Physics was then still called Natural Philosophy. Mathematics was considered by the physicists to be a French disease.

It was in the middle of the war. There were very few students in the class. The others were fighting the war. Lectures were given by Hardy, John E. Littlewood, Abram Samoilovitch Besicovitch, but students were only a few. We did not sit in class-rooms but sat around small tables with a few students and one teacher.

It was a very lucky time to be a student.

I was a student of Besicovitch, expert on measure theory. He came from Russia, spoke English with a very strong Russian accent. Besicovitch gave me problems to work on. I liked the Russian language, so we spoke in Russian. We often went for long walks in Cambridge, and while doing so were not allowed to speak English. I was influenced by the Besicovitch style: it is architecture. Besicovitch had the ability to build beautiful structures from simple components, which were used in his mathematical proofs. I have used the same style for my calculations in physics.

After Rutherford died in 1937 physics was at a low ebb in Cambridge. In physics, the only professor was Dirac. Dirac did not interact with anybody. Eddington was also there, he was an astronomer. We did not count him as a physicist.

At the time I was there, Lawrence Bragg was in charge of the physics department. He succeeded Rutherford. Bragg said: "Rutherford was the best physicist in the world and I am not: so we should do something else". It was amazing wisdom. He made a quite astonishing decision, not to do physics

but to do two new subjects instead: molecular biology and radio astronomy.

Martin Ryle brought military radar equipment and started receiving signals from the sky. For radio-astronomy, English weather doesn't matter, whether it is cloudy or raining. He immediately made great discoveries. In the molecular biology group Max Perutz and John Kendrew became leaders. Both were enormously successful and Cambridge became the world leader in both the two subjects introduced by Bragg.

After two years at Cambridge, I worked for the Royal Air Force (1943-1945). It was a grim time of the war. I was a civilian scientist at the headquarters of RAF Bomber Command, collecting information about the strategic bombing of Germany. After the bombing of Germany stopped in April 1945, we still had a huge bomber force. I would have flown to Okinawa, in order to bomb Japan from there, but the war ended in August; so we did not need to bomb any more. It was a great relief. We did not need to kill more people. We were very grateful to the atomic bombs for ending the war. At that time everybody thought that

this was the cause of the Japanese surrender, and so we were told. I recently read the book by historian Hasegawa who claims that the real reason for the surrender of Japan was not the atomic bombs but the invasion of the Soviet army from the north. Japan could not defend against Russians from the north and Americans from the south. So the Japanese generals thought that it was better to surrender to Americans, while keeping Russians out, using the atomic bomb as an excuse for their dishonorable surrender. Now I agree with Hasegawa and wish that we had not dropped the bomb on Hiroshima. The bomb was not as important as we believed.

**Fukugita:** Cambridge is the great place for mathematical physics, with Newton, Maxwell and Dirac. You were following in their footsteps. Why is Cambridge so successful?

**Dyson:** The image is presented to school children that each country is good at one special thing: music in Germany, painting in France, and in England it is science. This provokes gifted young children in England to go to Cambridge and become scientists. The tendency was enhanced by the famous Tripos exams in Cambridge.

**Fukugita:** What did bring

you from mathematics to physics?

**Dyson:** We saw the news on the day when Hiroshima was bombed: it did not say 'Hiroshima was attacked', but said 'New force of nature harnessed'. It meant that new science had emerged. I did not want to use the bomb, but wanted to use knowledge that came out of the bomb. Nuclear physics became suddenly a fashionable subject, and it gave a motivation to learn physics. Quantum mechanics was still new and exciting. General relativity was also exciting. Many things were going on in physics. I then started to study Heitler's book, "The Quantum Theory of Radiation". Two years later there was a miracle, the experiment of Willis Lamb at Columbia University measuring the energy levels of hydrogen atoms.

I went back to Cambridge to study physics. Nicholas Kemmer was my teacher, from whom I learned quantum field theory. I wanted to go to the United States. Geoffrey Taylor, a great hydrodynamics expert who had worked at Los Alamos, immediately said that I should go to Cornell, where Hans Bethe and the young bright physicists from Los Alamos were all working. I had no idea about Cornell,

but I went to Cornell and worked under Bethe. When I came to Cornell in September 1947, I knew the Columbia experiment finding a 1050 megacycle shift between 2P and 2S states of hydrogen. The old quantum mechanics of Dirac was wrong. I knew that Bethe had beautifully explained it. It was exciting. It was exactly the right time and the right place for me to be. Richard Feynman was then a young professor at Cornell.

In early 1948 Oppenheimer received a batch of the new Japanese journal, *Progress of Theoretical Physics*. He copied it to Bethe and Bethe showed it to me. I found the Tomonaga paper, where Tomonaga had done everything before we did. We also knew the work of Hans Kramers which appeared a year earlier, as a paper to be presented at the 1948 Solvay Conference, explaining the idea of renormalisation.

I remember, when I visited Berkeley in 1948, I was impressed by the chemist Melvin Calvin. He used for the first time a short-lived carbon radio-isotope to understand photosynthesis, how carbon dioxide is absorbed and converted into sugar, by tracing the move of carbon atoms from molecule to molecule

in a few seconds. How chemistry was going on second by second. It was the first application of nuclear physics to biology, and since that time biology evolved fast with the use of radioactive tracers. Oppenheimer said that the application of nuclear physics to biology was more important than the bomb.

**Fukugita:** How was the Institute in Princeton at that time?

**Dyson:** When I came to Princeton, Einstein was there. I think it was a bad mistake for him to come to Princeton. When he came he had two choices, either Caltech or Princeton. He chose the latter since in Caltech he had to teach. In Princeton he did not need to teach, so he lost contact with students. As a professor in the old German style, he had an assistant who was his personal slave. In my time, Einstein's last assistant was Bruria Kaufman, who herself is a good physicist. Kaufman said to me that being Einstein's assistant looks very good on a resumé but in fact was a boring job. All she had to do was routine calculations.

There were many young fellows around Einstein, doing new science in the adjoining building. Einstein was never interested in what we were doing and

never spoke to us. He never came to seminars, never to lunch. So there was zero communication between him and the young members of the Institute. I think it was mainly his fault, although partly ours. We were young and arrogant, and we didn't think we had much to learn from him.

**Fukugita:** Have you ever talked to Einstein?

**Dyson:** No, never!

He loved children, and certainly got along very well with them.

Einstein's long-term secretary Helene Dukas was a good, able woman, and a fierce protector of Einstein's privacy and private affairs. We were friendly with her. After Einstein's death she often came to our house. My children adopted her as their substitute grandmother. One rainy night, a truck came to the Institute, and took away all the Einstein papers that she had collected and cared for. The papers were on their way to the Hebrew University in Jerusalem, as Einstein had directed in his will. A few days after this, Helene passed away.

Bohr often came to the Institute. He was completely different from Einstein. He was very much interested in new experiments. He came to our seminars, had lunch



with us, and talked to young people. He was then working as a diplomat, representing Denmark at the United Nations in New York, campaigning for international control of nuclear power and nuclear weapons. But he kept up his interest in new physics.

Tomonaga also came to Princeton. I liked him very much. Unfortunately, he arrived in the week I left Princeton. So I had only a short time to talk to him. He was an extremely unselfish person. He had done much of what Schwinger and Feynman did, five years earlier, but he never said so. Tomonaga felt guilty as he was not sharing the great hardships his students were suffering in Tokyo. He felt bad to be in the comfortable environment of Princeton, so he took an

early chance to go back to Japan.

**Fukugita:** How was Wolfgang Pauli?

**Dyson:** Pauli left the Institute before I came to Princeton. He was at the Institute all the time during the war. He was close to Einstein and enjoyed talking to him. He was, however, isolated and quite unhappy at the Institute. He settled in Switzerland after the war, and finally got citizenship, which had been refused when he lived in Switzerland before the war. He didn't come back to Princeton.

I got to know him in Switzerland in 1951. Pauli had a big psychological problem. Fortunately, it was one of the rare times when he was very happy, disobeying the order of doctors and his wife. We had a very good friendship.

He was in a relaxed state. We went out to walk every day and talked a lot about physics. We called at all the ice-cream places along the way, because his doctor had forbidden him to eat ice-cream. He understood all the results from on-going high energy physics, where cosmic ray experiments were the cutting edge at that time. I was working on the convergence of the perturbation expansion in quantum electrodynamics. Pauli maintained that it is divergent. I tried to convince him that the series converges, but he did not agree. After all, we know now that he was correct. I was not unhappy, as I could write a paper on the divergence of the series, thanking Pauli for his help.

**Fukugita:** And, Dirac?

**Dyson:** Dirac quite often came to Princeton. He changed his character, from a silent young man difficult to approach, to a talkative, friendly old man with a big sense of humor. Like Einstein, he stuck to theories of his own preference, the large number hypothesis which turned out not to work, and the negative metric field theory which I never understood. When he was old, he seemed to have lost his intuitive ability to guess what is right or wrong. He became more and more

a normal person. Much nonsense has been written about him. He was not an outstandingly strange man. He was an outstandingly intelligent man.

**Fukugita:** I think Heisenberg did not come to Princeton?

**Dyson:** Heisenberg did not visit the Institute, but he visited Einstein once. It is recorded in the diary of Johanna Fantova. She was an Austrian Jew, whose family had been acquainted with Einstein in 1912 while he was working at the German University in Prague. She became a friend of Einstein 20 years later in Berlin, organising his personal collection of books, sailing a boat with him on a lake, and 20 years later again in Princeton. In Princeton she took a course to qualify for a job as a librarian, following the advice of Einstein, and worked at the University Library. They sailed on Carnegie Lake. She kept a diary which was recently published. From this diary we can glimpse Einstein's life in Princeton. In it Heisenberg's and Bohr's visits to Einstein appear. Neither of them made a good impression. Heisenberg was still a big Nazi, dreaming of a greater Germany. Bohr would not stop talking until Einstein was totally exhausted.

Heisenberg, too, was devoted to his personal theory, spinor field theory,

in his late years. He required his assistant to work on that. The assistant that I knew was Hans-Peter Dürr. His career was ruined by this work. Heisenberg did not give up spinor field theory until his death.

**Fukugita:** Have you got any impressions of Japanese scientists, other than Tomonaga, who came to Princeton?

**Dyson:** Yukawa came to Princeton, but he was not doing physics. He came to Princeton as a politician, perhaps as a symbol of Japanese science. He worked for public relations or as a diplomat and made a public appearance at the United Nations. I saw him a few times but never talked to him about physics.

Taro Asano was a young mathematical physicist from Tokyo. I had invited him to the Institute. He had done a beautiful piece of work on quantum ferromagnets. He was isolated, he was shy, he had no friend to talk to, and did not speak much to colleagues. Possibly the language was a problem. Towards summer he got depressed and disappeared. After the term ended, he drove his car at a crazy speed up Springdale Road, crashed head-on with a heavy car and killed himself. I was very sorry for this. I escorted his wife, Sachiko, back to Japan, carrying his

ashes. This was incidentally my first landing in Japan, although I didn't go out from the airport.

**Fukugita:** This reminds me of the episode of Yutaka Taniyama, who was invited by André Weil to the Institute, but committed suicide before he came to the Institute, and his fiancée followed him.

**Dyson:** André Weil was the most friendly person. He would have cared for him if he had come to Princeton. When I visited Chicago as a young student, Weil took me out for a long walk of three hours, and talked about many things. He had a reputation for being fierce, but I never saw him fierce. Maybe he had two sides.

**Fukugita:** Was there any impact from the work of Yang and Mills?

**Dyson:** Yang did this work with Mills at Brookhaven in summer. At that time I was not working on particle physics, but on random matrices. I think that Yang's motivation was to make a gauge theory of isospin, rather than to make gauge theories a central feature of the universe. Pauli was at Princeton at the time. He was very negative because Yang's gauge field was massless. Pauli thought that it had nothing to do with nature and was not interesting. His opinion had a big effect on people including Yang himself.



**Fukugita:** How about contacts with legendary mathematicians, such as Hermann Weyl, Johann von Neumann, Carl Siegel?

**Dyson:** Shortly after I came to Princeton, Hermann Weyl and Carl Siegel left to Zürich and Germany, respectively. Siegel knew me, because I had strengthened his theorem about the approximation of algebraic numbers by rationals. Hermann Weyl somehow knew me too, and helped to get me hired as an Institute professor. Soon after I came to the Institute as a professor, they had already gone. So I had not much contact with these people. The only one I knew well was Johann von Neumann who stayed longer. He was enthusiastically working on the electronic computer project. I was

much interested in it, but the majority of people at the Institute were not. Oppenheimer was not interested in it. I was one of the few who tried to keep the project alive after von Neumann left.

The meteorologist Jule Charney came to Princeton and ran climate models with von Neumann's computer. When the computer project ended, Charney left. This was a great opportunity missed. The Institute could have been the centre of two new sciences, climate studies and computer science, in the same way that Cambridge became the centre of molecular biology and radio astronomy under the leadership of Bragg. I proposed that Charney be employed as a professor at the Institute, saving the

computer programme, but my efforts failed miserably. Oppenheimer did not show any interest.

The computer project went briefly to Princeton University before it was dropped a year or so after von Neumann left. Twenty years later, computer science was centred at MIT and IBM, and climate science was centred in UCLA and in Norway. I feel sorry for this. The dropping of the Institute computer project probably delayed the growth of the two new sciences by twenty years.

**Fukugita:** How about Oppenheimer?

**Dyson:** Robert Oppenheimer was a complicated character. With Hartland Snyder, he did his most important scientific work understanding black holes. They showed that every massive object, with zero internal pressure, will enter a state of permanent free fall, as a consequence of Einstein's equations. They predicted the existence of black holes in any universe obeying Einstein's equations. You don't need any new physics. Just Einstein's theory is good enough. It is a wonderful paper. This work appeared in the 1 September 1939 issue of *Physical Review*. The paper appeared at a bad time, on the day that Hitler invaded Poland and started World War II.

Nobody paid attention to it.

Einstein never believed in black holes. He not only didn't believe in them, but also wrote a paper claiming that black holes could not exist. Oppenheimer, too, never revisited this subject. He refused to talk about black holes, even after candidate black holes were discovered in the sky. I tried to talk about it with him and to explain to him why it is interesting, but he always changed the subject. I did not know why. The father and the grandfather of the black hole did not like their son.

I would mention two important people of genius that Oppenheimer failed to recognise. One is Fritz Zwicky, who had many wonderful new ideas. He was an astronomer who discovered dark matter and the two main types of supernova, and predicted neutron stars. He is now recognised, but had never been recognised while he was alive. Oppenheimer never spoke to him while both were at Caltech. Einstein visited Caltech, but he wasn't interested in him, and never spoke to him. I have never met Zwicky. The other is John Wheeler. He is also poorly recognised. Oppenheimer disliked him strongly. Unlike Zwicky, he was not a difficult person. He had a large number of

students and was very generous to them. He gave Feynman full credit for work on problems that he had suggested. He was an extreme patriot, very right wing, a 150% American, politically opposite to Oppenheimer. Wheeler was a big protagonist of black holes. This might possibly be the reason why Oppenheimer did not like to talk about black holes. Oppenheimer disliked talking to him. Both Zwicky and Wheeler deserve a better recognition.

**Fukugita:** What is the biggest surprise you experienced in your research life?

**Dyson:** The discovery of weak vector bosons. I had not taken the theory of Weinberg and Salam seriously, and I had a personal theory of weak interactions. So the discovery of the vector bosons was a surprise to me. The discovery was so beautiful that it made me happy to be proved wrong.

**Fukugita:** What do you think of the relation between mathematics and physics as a mathematical physicist?

**Dyson:** The real gap was between pure mathematics and applied mathematics. Pure mathematicians were talking a different language. Bourbaki was fashionable pure mathematics, and I was not very interested in it.

I remember a talk about "flabby sheaves". Somebody asked what are flabby sheaves. The chairman, André Weil, said "that is already a classical terminology and we do not need to explain it". I never understood what it was. I got to understand fibre bundles, but I have not gone beyond that. Pure mathematics had somehow become extremely abstract. It did not seem to me to be a fruitful way to go. I preferred to stay with applied mathematics.

There seems to be no barrier between string theory and mathematics. They understand each other. There is a big barrier between string theory and the rest of physics. A new trend is good contact between computer scientists and mathematicians. We now have two professors in the School of Mathematics working on computer science. One of them is Voevodsky. He is working on the mathematical logic of computability.

**Fukugita:** How do you think about the present status of physics? What do you want to encourage?

**Dyson:** Physics has slowed down. 60 years ago, experiments were done in 6 months and they were explained in 6 weeks. Now experiments take 20 years. Except for high energy

physics, there is still a lot to do. Small scale science is still flourishing. Particle physics is a special case. Even in particle physics, a small experiment, such as the measurement of the electric dipole moment of the electron by Gabrielse at Harvard, is an example on the frontier and has a chance to find something new.

Progress in astronomy is great. Twenty years ago, we knew only a tiny part of the Universe, only out to redshift 0.2. Now this horizon has been enormously extended. We can see and explore the Universe all the way back to the Big Bang. Astronomy can do a lot of important science with small instruments, both on the ground and in space. Kepler is a small satellite, but enormously successful. The BICEP2 microwave anisotropy experiment at the South Pole is not expensive and is equally successful. Politics focuses attention on big science, and big science also gets too much attention from the media. I want to encourage studies in as wide a field as possible. Especially in astronomy, there are so many things to explore. Zwicky emphasized the advantage of single-purpose instruments. Kepler is a beautiful example. We can still find others.

**Fukugita:** How about particle physics?

**Dyson:** I have a critical view of LHC. Every event is complicated with an enormous background of uninteresting particles. Software must be written specifically to pick out what is looked for. You can discover only what you expected. Every discovery is predictable. The LHC cannot make unexpected discoveries. The accelerator still uses radio-frequency acceleration, with the electric voltage per unit length the same as it was half a century earlier. Ordinary lasers have electric fields 1000 times higher. The use of laser fields for acceleration must be studied. Bigger accelerators using the same acceleration principle are not a good way to go.

There are several ways we can go for particle physics. Underground detectors have a good future. They can detect everything that happens. Two countries, Japan and Canada, with good scientists and not too much money, lead the world in the use of underground detectors. Their projects are more cost-effective than the projects in America and Europe, which invested a large amount of money in accelerators. Kamiokande is a very good experiment that has already brought

several important results. It is always ready to detect something unexpected. I strongly support this experiment and its upgrade. They may wait for a long time, but they have a good chance to discover something unexpected.

In general, experimentalists ought to be prepared for something unexpected to happen. This is also true for theorists. They should not stick to one thing, but should think about a variety of subjects. If they get bored with one subject, they should break out into other subjects. I have been doing it this way throughout my long research life.

String theory is beautiful. One day it may turn out to have something to do with nature. String theory is now close to mathematics, but the gap between string theory and the rest of physics is very wide. String theory does not have enough good problems for ten thousand people to work on. I am not worried about string theory in itself, but I am concerned with their narrow training of students. This makes them unemployable for jobs other than string theory. Changing people's careers every five years would help to solve this problem. Particle physics is not easy to escape from. Escape from other branches of science is easier.

I know some people who switched from string theory to genomics. 95% of genomes are not genes. We do not understand what these non-coding parts of genomes are doing. The discovery of Human Accelerated Regions of genomes, where significant changes are seen between chimps and humans, could motivate the start of a new science. We must be flexible and ready to move into new sciences as they emerge.

When I was young, I carried out hard calculations competitively. I was not concerned with philosophical questions. For me, philosophy was a branch of literature, not a branch of science. Science was a collection of tools that I used for solving problems. When I got older, I wrote books and articles for magazines, rather than doing competitive science. So my work has changed with age.





Climate? It takes much too long to explain. In short, I am skeptical about prevailing anxieties about climate. Oak Ridge National Laboratory had a very good climate programme, started 50 years ago, before it became fashionable. The director was Alvin Weinberg. He insisted that we must understand the real world. I worked with him for some years. There were a lot of biologists, who were concerned with soils, trees, vegetation, microbes and all kinds of ecology. In addition there were physicists who wanted to understand climate. So, it was a well balanced programme.

For carbon dioxide in the atmosphere, half of the effects are concerned with climate and half with ecology. Carbon dioxide is a powerful fertiliser. It can be a substitute for water. The limitation is that it is only a small fraction of the air, while the fraction of water vapour is usually larger. Suppose we open a pore in a leaf and get one carbon dioxide molecule to go in through the pore. While the pore is open, about 100 water molecules come out. So the leaf is evaporating water much faster than

it is absorbing carbon dioxide. If more carbon dioxide is in the air, the process is more efficient and evaporates less water, encouraging plant growth. The effect of carbon dioxide is substantial for biology. There is a 30% increase of CO<sub>2</sub> in the atmosphere compared with the amount a hundred years ago, and it caused an increase in vegetation, wild forest and also food crops roughly half as great, namely 15% increase of biomass due to 30% increase of CO<sub>2</sub>. This is not well known to the public.

For climate, carbon dioxide is the easy part of the problem. There is a huge complicated feedback and recycling of carbon dioxide, but it can be measured and is fairly well understood. Water vapour is much more difficult. It is more abundant and the greenhouse effect is larger. Evaporation of carbon dioxide from the ocean is a minor effect. Evaporation of water from the ocean is much larger and dominates the effect of CO<sub>2</sub>. Water vapour is strongly absorbing, but also has a tendency to condense into clouds. This is why the problem is difficult. Vegetation is also a huge source of water vapour, and so is coupled to the circulation of water. Evaporation of water is crucial to the final effect. That is why it is so hard to calculate. If you only have to deal with carbon dioxide, the problem is

simple.

Carbon dioxide causes warming of the climate. This is a serious concern. Unfortunately, the United Nations set up the committee IPCC for studying climate. They have written into the definition of the programme that they should look at effects of human activity on the climate, but are not supposed to look at non-human effects. The programme is highly biased from the beginning, and this is what I am fighting against: I am not simply opposing what they are doing. The scientists working for the United Nations are allowed only to look at human causes of climate change.

Climate is a difficult problem. Particularly, clouds are crucial but poorly treated. The resolution is poor in simulations. One takes averages over 100 km in global climate models, while actual clouds require a resolution of at most 1 km vertically and 10 km horizontally. What the models do is not to model clouds but to represent average cloudiness in a region by fudge factors. The present state of understanding of climate is poor. I don't trust the models.

I also mention that there was the little ice age in the 17th century, when the sun went to sleep for 70 years. The world suffered from a cold climate. There is a strong correlation between the

sun and the climate. This is totally absent in the climate models. The sun was active for the last 50 years, and less active in the last 5 years. Strong global warming lasted for 50 years, but seems to have stopped for the last ten years. This is a clear observational hint, but it is not included in the climate models.

The world average temperature is a misleading concept. It is unclear what it means. The most intense warming happens in very cold places, mostly at high latitudes. There are a lot of unknowns in the model.

It is also not clear if local warming is bad. In some arctic areas, local people are happy with the warming. They had a very hard life in the past, relying on fishing in a horrible climate. Warming has made their life easier. There are always winners and losers when the climate changes.

I am skeptical whether models give good pictures of reality. The real world is much more complicated. My conclusion is that I do not believe the official predictions of climate for the next 100 years. The models are good tools for understanding climate but bad tools for predicting climate. They are good for understanding because they allow us to see the effect of changing one factor at a time. They are bad for predicting because they leave out a large number of factors that might be important.