NIELS BOHR LIBRARY & ARCHIVES / TRANSCRIPT

ONE PHYSICS ELLIPSE, COLLEGE PARK, MD 20740 • (301)-209-3177 • NBL@AIP.ORG

Adriaan Fokker

April 1, 1963

Interviewed by: John L. Heilbron
Location: Fokker's home, Beekbergen, Holland

Transcript version date: December 18, 2024
DOI: https://doi.org/10.1063/nbla.poln.efgx

Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: Niels Henrik David Bohr, Paul Ehrenfest, Albert Einstein, H. von Euler, Wander de Haas, Gilles Holst, Heike Kammerlingh Onnes, Hendrik Anthony Kramers, Hendrik Antoon Lorentz, Max Planck, John Joseph Thomson; Conseil National de la Recherche Scientifique, Rijksuniversiteit te Leiden, and Teyler Foundation.

Usage Information and Disclaimer:

AIP oral history transcripts are based on audio recordings, and the interviewer and interviewee have generally had the opportunity to review and edit a transcript, such that it may depart in places from the original conversation. If you have questions about the interview, or you wish to consult additional materials we may have that are relevant to it, please contact us at nbl@aip.org for further information.

Please bear in mind that this oral history was not intended as a literary product and therefore may not read as clearly as a more fully edited source. Also, memories are not fully reliable guides to past events. Statements made within the interview represent a subjective appreciation of events as perceived at the time of the interview, and factual statements should be corroborated by other sources whenever possible or be clearly cited as a recollection.

Heilbron:

The questions on the outline were meant only as suggestions.

Fokker:

Now, 'How was your interest in science first awakened?' Well, that is difficult to say, because it wasn't awakened. I just was a good pupil at school. My mother a1ways wanted me to become an engineer, and I never objected. And when we returned from the Indies and came to Paris we went up into the Eiffel Tower in the lift, you see, and I remember my mother saying to me, "Don't you admire that? Isn't it very fine, and don't you want to make such things?" So it was never discussed, and it was agreed that I should become an engineer. Therefore, I had to go to Delft, you see. And I had a school without Latin or Greek; we call it (Hochburgerschule). It's a higher school for citizens. There I was good in the physics and the chemistry and in Natural History — biology; so there was no special line indicated to be just the one. When I came to Delft I was a little bit conceited, and I just chose the department that was said to be the most difficult. That was mining engineering, and after I was there for a year I was disappointed in the lack of scientific mind there, you see. Only Professor Martus de Haas who taught the physics and optics attracted me; he was also a crystallographer. But then I wanted to have pure physics, you see, and so Martus de Haas introduced me to Lorentz. He said, "You must go and speak with Lorentz." Then I spoke with Lorentz and he agreed, and I persuaded my father and he gave leave to it. My father was a commercial man, and had his career in our Indies. Of course he asked me, "What will your career be?" And I didn't know; I hadn't the slightest idea. Well, I said that I should be a teacher in the secondary school, and that was that. But in order to take examinations in the University, at that time it was required that you have the diploma of the gymnasium — that means Latin and Greek. So I had to make up for that, and I spent a year, studying with a teacher, Greek and Latin. During that study of Greek and Latin a great interest was awakened in me for these languages, you see. In Delft I had (Headbone), the philosopher. (???) philosopher was (Boland); he was a great follower of Hegel and he introduced the (???). He made a great school of admiring youths who followed his lectures, and I did too. And this man scolded us because we didn't know the Bible, you see. And so by his influence I took to reading the Bible and the New Testament and sometimes I thought I might make studies for being a parson and going in that direction, but I didn't carry it further. Later, also, I had the impression that I was not specifically gifted for physics, you see, but then after a while I said, "Well, there is no use in changing; Just stay where you are, and everywhere you can do good work." So you see it was not a very special instinct for physics. So I was in Leiden, and the lectures of Lorentz did only come after the exam of candidate, and so I first passed this examination, and then I came to Lorentz. In the meantime, I was a member of the students' corporation with its sub-societies; I did the leading of music in the group for music, and I did a great deal of rowing in the Rowing Club. So there was a great field of interest outside the curriculum.

Heilbron:

This musical interest of yours was quite an old one?

Fokker:

Oh, yes, yes, yes. Well, at that time there was a small club in the corporation which was especially for the people studying sciences, but it was not very good. The first time I was there as a guest, I was quite shocked by this lack of spirit. They were noisy and they were drinking very much, and so I thought, "Ja, Ja, this is nothing for me." But then an older man said, "Well, don't be impatient (???)." So I entered this club, and I did a great deal in conducting the work and especially in raising the level. But there was not a center where things were discussed at a high level. Later on, when I was working with Lorentz, Lorentz gathered the students once a month and some of us were invited to give a lecture there, you see, and we had what you call a colloquium. But it was very quiet.

Heilbron:

About how many students were there?

Fokker:

Oh, I think a dozen or so. There were a lot of students with Onnes; Onnes was a remarkable man, and he was a very dominating man, and everything was just directed to this thermodynamics, you see. It was quite an exception, I suppose, that Zeeman managed to become a spectroscopist then. I am not only a theoretical man; I have a great love for practical things — to use my hands, you see. Then sometime I wanted to enter the team of the laboratory of Onnes, to work also in the laboratory. But Onnes wouldn't agree with that.

Heilbron:

He wouldn't let you?

Fokker:

No, he wouldn't let me, because when somebody started with him he got every kind of nasty job to do — calibrating capillaries and so on. And he wouldn't ask me to do that because he had too high esteem for me. And he said, "Well, just finish your exam, and then afterwards —." And then I said, "Well, that's not very good because after I have done the exam, I will start my thesis with Lorentz, and after that I want to go to the world outside. This is the time for me to work in the lab." But, no, there was nothing else to do, you see, and so I was only a theoretical man.

Heilbron:

Did the students participate in this once-a-month colloquium?

Fokker:

Yes, it was for the students. [Break] I was complaining of the sleepiness of Leiden. Lorentz was a very kind man, you see, and he never wanted to intrude too much upon others. Perhaps he might have told me that I shouldn't give so much time to this rowing and the music and more to the books, but he never would interfere with one's own desires. Therefore, it was very quiet around him, you see. He himself was so above all troubles; I mean he was so keen and everything was so clear to him. It was quite different with Ehrenfest. You know that Lorentz was asked to go to Haarlem to Teyler Foundation, and then he looked for a successor. This has been described very well by Martin Klein; you saw it perhaps? So Ehrenfest was asked by him to come, and there was all the difference. Ehrenfest was so dynamic; Lorentz was very quiet, but Ehrenfest was a man to grab you by the collar, and, when he had two youngsters, he knocked their heads together, and it was quite different. When Ehrenfest came in 1912, then you could speak of a center of contact in his colloquia — they were every week, not once a month, but every week. That was a very good thing, and Ehrenfest succeeded in making pupils. There are very few pupils of Lorentz.

Heilbron:

Who are they, besides yourself?

Fokker:

The most known one has been Ornstein. Later when Ornstein was in Utrecht as Professor of Theoretical Physics, — he was the successor of Julius who was the experimental man there. Then he reorganized the laboratory there, and just like an experimental physicist he had all this work done in Utrecht.

Heilbron:

Did Lorentz' detachment make it difficult to speak with him?

Fokker:

No, he was very, very kind, and his conversation was never dull although he was quiet.

But he didn't make an appeal on your soul, so to speak, you see; he just left you to your own intentions and to your own desires. And that's not the way to make a school; if you are making a school, you must just shake people, you see. The same thing I noticed later when I talked with Rutherford — I was some weeks with Rutherford. There's a great difference; it's a pity that Lorentz did not have a school around him just like Ehrenfest. The first pupils of Ehrenfest are very well known; there was Kramers, and Dirk Coster and there's Burgers, and there was Tinbergen who is now an economical man. Later the level was lower; perhaps Ehrenfest himself was not so sure of himself. In later years Ehrenfest liked to send his pupils away to other people, so he had sent away Kramers to Bohr and Coster he had sent away to Lund to Siegbahn, and in that manner.

Heilbron:

Do you know if there were other alternatives for Lorentz' chair? How did he happen to choose a man so unlike himself to succeed him?

Fokker:

I think that Martin Klein speaks about that. Lorentz had the impression that there were very new things to do, and that it must be a man who was very open for new ideas. It was a stroke of genius for Lorentz to find this man, certainly. That was very important.

Heilbron:

In your own education, I wonder if you could tell us the sort of things which you were required to learn in physics when you came to Leiden?

Fokker:

Well, I already told you that it was just a question of accident that I came into physics.

Heilbron:

Yes. I meant the sorts of things you had to learn, the sorts of material you had to know, the sorts of courses, and so forth.

Fokker:

Well, you had to have a foundation of physics, of chemistry and of mathematics, too. The mathematics did not go too far, because differential analysis was not in the curriculum. It should be in my opinion, but it was not. And as I said for work in the university you needed Latin and Greek. When I was some years in Leiden, this condition was done away with, and now everybody can go to the university in this direction.

Heilbron:

And at Leiden did you, for instance, hear courses on —

Fokker:

In Leiden I took just the courses which were required for the study and I followed the lectures, and so there were no special things I learned there.

Heilbron:

For instance, when did you hear about the quantum and its difficulties?

Fokker:

That was after my candidate's exam when I followed the lectures of Lorentz.

Heilbron:

He discussed it in connection with the radiation theory?

Fokker:

Yes, yes. Essentially the radiation theory. And those were very fine lectures, a very fine course. I'm responsible for being editor of this course in the collected lectures of Lorentz. There he told of the work of Planck and of his probability reasonings. Lorentz tried, to find an explanation in classical theory; on all matters he tried to find how to incorporate it there. And gradually he got the impression that it was not possible; that it required something quite new. What was the question you asked about the Solvay Congress? I think the progress of the Solvay Congress was just to make the conclusion that you ought to have something quite new — that it was impossible for the classical theory to find something. And in order to collect examples of this failure of the classical theory, Lorentz did all of these several works. And he proposed to me to investigate the mean energy of an electron in the field of radiation. And that was the way I came to it; I didn't choose the subject myself, I got it through Lorentz, you see. First I was very slow in making calculations, but during the summer vacation I got onto them, and then it went further. But everything was inspired by Lorentz. He indicated the equation of Einstein and Hopf to me and I started with that. He had used it in his equation too, at least had spoken of it at the Solvay Congress. I was too young at that time to know much of this Congress and to follow it, so I only listened to Lorentz and what he taught in the lectures and in private conversation.

Heilbron:

When did you actually begin work on that?

Fokker:

I think that would be in 1911; no, it must have been in 1912; it was after my doctoral exam and that was six years after I entered Leiden. I entered Leiden in 1906, so it was 1912, I think in the spring of 1912. My promotion was in 1913.

Heilbron:

That was quickly done.

Fokker:

In 1912 Ehrenfest came to Leiden, you see, and he considered me a little bit too old for his guidance. I had grown up with quite another environment and he wasn't accustomed to it. And I was a member of this corporation of students and saw a little bit of people like that. I always attended Ehrenfest's colloquia but I did not attend his lectures; I don't know why.

Heilbron:

Do you remember the sorts of issues that were discussed in those colloquia and what was said about them?

Fokker:

Oh, yes, the current things. For instance, when the experiments of von Laue came, of course they were discussed. And then one of our people, this Professor Gilles Holst, had the idea of reflections in crystal planes, you see. And that was the fundamental idea of Bragg. Instead of these three-dimensional calculations of Laue, he put the simple idea of reflections on the crystal plates, and that was also the idea of Hoist. He put it there in the colloquium.

Heilbron:

And did anybody ever think of doing such an experiment at Leiden before the —

Fokker:

No, because at Leiden they had all the temperature things, and they had no X-ray reflections. I think Hoist would have liked to do it. Then he was also at the discussions of conductivity and super conductivity. Indeed when Onnes was in Brussels Hoist found this superconductivity. It was really Holst who found it.

Heilbron:

He went to Philips, didn't he?

Fokker:

He was the great organizer of Philips Laboratory, a very able man, but he never came to the foreground. He didn't like to put his name on papers which he thought were done by other people. Of course people are always putting their names together on the paper, but he left it all to the other men. So he has much more to his credit than is known. That is a digression and really I can't remember all these things. I've been discussing the colloquium; it was just lovely. Often Ehrenfest invited his friends to come, you see. Now I must boast of myself also a little more. In this corporation every faculty had its own organization, and when I came on the board of this faculty, we arranged to have a folder with periodicals which passed from member to member, you see. That was just to read what was done in the world and to have a broadening of our knowledge. That was one thing; then it happened that my sister, who was a pianist — she studied in Berlin — was to have a debut in Berlin, and the whole family went to Berlin. Then I asked Lorentz if it would not be possible to catch a great fish — to invite a physicist to come to Leiden to make an address. Then Lorentz said, "Oh yes, I shall help you with an introduction to Planck," because of course Lorentz admired Planck very much. And the idea that Planck might come to Leiden was very sympathetic to him. So I went with high hopes to Planck; I said, "We want you to give a lecture to us in Leiden." Planck was very kind, and he agreed and he was going to come. Indeed he was very much pleased by the idea, also pleased by the idea of living with Lorentz in his home. Then he gave this lecture: "Uber die Einheit des physikalischen Weltbildes," which was very well-known afterwards, and he gave it to us in Leiden, you see. I'm boasting again, and that was my idea. Then later we asked Lorentz if he would also introduce Einstein to us; we invited Einstein to be here, and Einstein gave a very beautiful lecture on Brownian movement. Einstein too was living in the home of Lorentz and was very much impressed by Lorentz. And that was our connection with Einstein, you see. I believe that was before Ehrenfest came.

Heilbron:

The question of specific heats was of interest before Ehrenfest came? Was it of considerable interest in Leiden before Ehrenfest came?

Fokker:

Of course the measuring of specific heats was very much done by Onnes, but the connection of quantum theory to specific heats was not invented in Leiden. That was (???) work and Debye's, I believe.

Heilbron:

Well, Einstein's was the first.

Fokker:

Einstein's was the first, yes. And the idea of Einstein was very simply that when you have the theory of vibrations and put an electric charge on a vibrating thing you have a resonator and you get —

Heilbron:

Well, in the course of preparing your thesis you ran across some of the questions which you then discussed in that article in the Physikalische Zeitschrift, I think. You calculate the average energy —

Fokker:

Well, when I had finished it I made a kind of extract for making the result known, in the Physikalische Zeitschrift, you see. But it was intended to have a more extended abstract in the Archive Néerlandaise and here it is; it appeared in 1918 and my dissertation is from 1913, you see. And there a question is raised which is for me rather important, because I had made this thing and it was to be corrected by a man who knew better French than I know. Meanwhile I had gone to Zurich, and I had taken up this problem of rotating molecules — rotating dipoles — in a radiation field, and then I found the extension of the formula which was used here, you see in the article. And so I thought, "It belongs to that subject," so I put it in the French abstract from that. So it went to Lorentz in the beginning of 1914, and then came the war. Lorentz as the President of the Academy of Sciences in Amsterdam was confronted with a great deal of difficult problems —organizing problems. People wanted the Academy to take the initiative and to have a regular consultation on the practical needs of the military people and the scientific information which could be acquired. And that was a great burden to Lorentz, and Lorentz just left this paper in his drawer, you see. I naturally had asked him to make the information known; until at last Planck was annoyed by this lack of proof. Then Planck produced three proofs of it, you see. Of course that was very good, and then I went to Lorentz and said, "Well, Planck is getting the thing; we must have it published." Then in 1918 it was published. Lorentz was too much employed in other ways to have attention for this thing which was in his drawer. Now they call it the Fokker-Planck equation, and I'm very grateful for that, but I'm ashamed that I never did any more myself with this equation; after that I left it. Sometime much later Kramers gave some discussion of it. Then this paper of Kramers attracted the attention of people and now it is called the Fokker-Planck equation. And what they have done with it now I hardly can follow; that is that.

Heilbron:

When did you first meet Kramers? He began at Leiden just when you were leaving?

Fokker:

Yes, I was at the end of my stay in Leiden when Kramers came. He came as a young man, just as a freshman, or I think even before he began studying, and we spoke together and so on. He asked my advice and what I thought, but he fell into the hands of Ehrenfest — (and then I wasn't any good). But we were very good friends afterwards. When I was in Leiden there came a vacancy in Utrecht because Debye left. I got a phone call from Kramers and he wanted to know whether I had some ambition in the direction of the chair. But I had my chair in Delft just then and I said, "Oh, no I'm not interested in it so much." He didn't want to accept when he had the idea that I should be there, because I was his elder, you see; and that was a very great thought and a very magnanimous idea of his. We were always very great friends. One time Kramers published a paper in the Academy on this geodetical precession, perhaps you know what it is. The first idea was given by (Schouten), but he made a mistake in his calculation. The idea was very good, however; he said when you have a curvature of space and you go around and you have it parallel to itself, and you make a circuit; then you will have a change of direction, you see. And that struck me very much, and I spoke of it with Lorentz. Lorentz didn't like such intuitive things as this you see and he wanted to see the calculations; then he challenged me — "Well, if it is a good idea, just work it out and show it to me." That's why I worked it out, and I found the thought that was mistaken, because in making the circumference he only had looked at the curvature of the surface and he had forgotten what we call now the precession of Thomas. And if you add the precession of Thomas to the curvature precession then you get the right value, you see, and I had the right value. And then Kramers also made the late start and had the calculation made, but he also got the wrong result. He also mistook the dt for ds or something like that. Then I wrote to him, and then he acknowledged, and then he corrected his mistake. So we were always very great friends, Kramers and I.

Heilbron:

How did he happen to leave Ehrenfest for Bohr in the middle of his —?

Fokker:

Ehrenfest just sent him.

Heilbron:

Did he dismiss him or send him?

Fokker:

He just said, "You go to Bohr." Ehrenfest had such a great admiration for Bohr, and I think perhaps he thought, "Bohr is a more stable man than I and will do better with Kramers." You will have heard of Kramers from Bohr himself, of course. He did a great deal there. All the other people — Heisenberg and Pauli — had little cards on the door "Do not Disturb" "Keep Out" but Kramers never did. And everybody came in to Kramers, and Kramers lost a lot of time just in helping other people.

Heilbron:

And talking to them in whatever language they happened to speak.

Fokker:

Yes, yes. Oh, I'm sure a great deal of the work of Heisenberg is due to Kramers. But of course that was inside the laboratory.

Heilbron:

Then there was your year's excursion. You chose Zurich and then England.

Fokker:

Yes, after my thesis I wanted to see something else. First of all Einstein, because we knew Einstein. Einstein had come to Leiden. And that was done; Lorentz asked permission for me to come to him and Einstein was very glad to have me and we worked very good together. That was in the winter, starting in 1913. Then came the call for Berlin. And so in the spring at Easter time Einstein would go to Berlin. Ad he went to Berlin with reluctance; he didn't like the spirit of the Germans. So he didn't encourage me to follow him to Berlin, and I didn't like the idea of going to Berlin either, so that was the end of our —

Heilbron:

The question came up and you did discuss whether to go on to Berlin with Einstein?

Fokker:

Yes, we discussed it, but not so very seriously, and I had some inclination to the other side too. Then I asked Lorentz to introduce me to the English people, and in fact to Rutherford in the first place. When I came to Rutherford, Marsden was still there, and I just went through the practicum under the guidance of Marsden and I spoke with all these people there, you see. But it didn't last very long at all because after a while Rutherford was leaving for Australia for a meeting of the Society for the Promoting of Science — the British Association. Then I had some weeks still to spend in England and then Bragg suggested to me to go to Leeds and to work in his laboratory. At Manchester with Rutherford I didn't do my own work I just followed the practicum and spoke with people, and when I came to Leeds, Bragg proposed that I make a special research into the structure of some iodide. And I was so stupid to say, "I'm afraid three weeks is not long enough, and perhaps I shall study literature." It was stupid of me; I should have taken that chance, you see. In vain I had asked others to let me work experimentally and Bragg offered it to me and I —. It was stupid. But when the holidays came I went to Edinburgh for a trip through Scotland, and during this trip the war broke out and I was recalled to the army, you see.

Heilbron:

Did you meet Bohr at —?

Fokker:

I am just starting to tell you that. Well, I am proud that I was the man in Zurich who told Einstein about this hydrogen paper of Bohr. Of course he was very much interested, but he didn't give any sort of a judgment; he just thought it over, you see. It was a very strange idea, an electron which did not radiate and so on; and that was that.

Heilbron:

He made no comment?

Fokker:

He made no comment, no, no, no, but he was very much struck. And afterwards he has used that idea of course. And then in Manchester one day Rutherford invited me to lunch in the refectory with him, and there were two other young people, British people, theoretical physicists. One was a Scottish man with red hair, and I thought, "Well, they are just guests," and we made acquaintance. Then after lunch the Scottish man walked up a little way with me, and he said to me, "Are you too applying for this post?" "What post?" I said. Well, I think that Darwin was leaving and there should be a successor, and he had been a little bit suspicious against me, this Scotch man, and when he knew that I was not applying for the post, he at once changed, you see; he was such a jolly fellow. But Rutherford didn't like either of these two, he wrote to Bohr. Bohr came to

Manchester, and that was my first acquaintance with Bohr, when he came there to speak with Rutherford about these things. Then we spoke together rather much, and Bohr had a question. He disagreed with Thomson on the theory of the diamagnetism. Thomson said in the magnetic field, electrons go like that, you see, and that gives the diamagnetism. And Bohr had seen that it wasn't possible; he learned of the statistical mechanics. He had told Thomson so, and Bohr said to me, "I must not have done very well in English, I didn't put it in just the right words." So Thomson was just quite through with him after that, you see. And then Bohr wanted to — There was nothing to do with Thomson there, and Bohr wanted to go to Rutherford. And he spoke with Rutherford, and Rutherford said, "Well, you can't just leave Thomson at once; stay your term out at Cambridge and then come to Manchester." And so he did. Now when Bohr told me that, I was glad to tell him that just the same problem had been mentioned by Lorentz in his lecture. Lorentz had said then that indeed Thomson was wrong, and so that was a comfort to Bohr, and I was glad, to tell Bohr that. You see, indeed the electrons are going around like that, but if you have a circular piece of metal, then there is a boundary. And in the boundary the electrons can't go around like that —- they can't complete the circuit and a boundary electron just goes the other way around; that compensates the (???) effect. And of course then also I told Bohr that I had told Einstein about his paper, and then he was anxious, "Did he believe it; did he believe it; did he believe it?" he said. "Do you believe it?" I said, "Yes, I do!"

Heilbron:

Had there been other people enthusiastic about it quite that early? Did Bohr feel uneasy about the theory at that time?

Fokker:

Just because he was so, so eager to ask, "Did he believe it; did he believe it?" I think that he was not quite sure yet. And of course the real thing, the important thing is to believe in it, you see. In this respect I recall a remark of Rutherford on the commemoration of Faraday. Rutherford told about the Faraday as a figure, and as a very characteristic feature he said, "Well, Faraday believed the thing; he believed in the lines of force, and that was a great strength." And I thought, this is the same thing with Rutherford; Rutherford believed in atoms, you see. He 'saw' them! And that is a great strength enabling one to proceed. Well, that is the question: in Leiden, did they believe in the atoms? Well, certainly yes. This remark of ours that they did not want to accept atoms as entities was — Therefore Lorentz had – all his career, his whole life — to calculate with discrete particles; so he believed it. And when Jean Perrin had these experiments with the distribution, that was very much due to (the days of) Lorentz, and he was very glad of it. And I know that Perrin had given him some photographs of it, and it was a treasure for Lorentz to have these photographs of Perrin's showing the different distributions. But to believe it, and to see them, and just to catch them as Rutherford did is a quite different thing. When I went there in 1913 [1923?] I met a Russian man —

what was his name? He worked in —.

Heilbron:

Kapitza.

Fokker:

Kapitza, yes that was him. Now I had a photograph of Lorentz taken by Ehrenfest during the lecture you see, and you see the blackboard: you see Lorentz writing a formula on the blackboard, sort of half turned. [Stands up to demonstrate.] He was not so, writing at the blackboard, but just so, you see. And then I said to Kapitza, "Look here, that is characteristic of Lorentz; he won't turn the back on you." And then the reaction of Kapitza was quite unexpected to me. "Well," he said, "it is no use for him. Look at Rutherford; look here at the photograph where Rutherford is sitting near the Geiger counter. Look at that hand, that has just the power of a tiger." Alive, you know; that was Rutherford.

Heilbron:

Ready to grab the alpha particle.

Fokker:

Isn't it amusing to hear this remark? As a leader Lorentz was just a little bit holding back his own personality. After the war when there was a struggle to reunite the French and German physicists, he never enforced his own will, you see. And it is a pity that he did not do it, because the French were intolerable.

Heilbron:

And it could have been done much sooner, you think, had he enforced his will?

Fokker:

Well, had he taken a stand more openly — for the people had so much respect for him — they would have done more than they did. There was a meeting in Brussels which I also attended for making the Conseils de Recherche. There was a great organization built you see, of scientific —

Heilbron:

You mean the Solvay Congress?

Fokker:

Not Solvay, no; you have the Physical Union, you have the Chemical Union, you have the Union of Astronomy and they are all under the Conseils. And there different countries could become members, and the Germans also should become members. For admission you needed, I think, two-thirds of the votes; and then Picard, the French mathematician was in the chair. It was put to the vote, and we thought that Germany had been accepted. But Picard said, "No, we need two-thirds of all votes, including the countries which are absent." And so it was not two-thirds, and so it was put aside. Lorentz was very, very shocked by that, you see; he was so disappointed. He couldn't be furious; but it was right next to it, you see. And so I say Lorentz was not imposing his own will on inside matters. And therefore he was so much appreciated as a chairman everywhere. He knew very well his languages, and he had this impartiality, too much impartiality. Bohr sometimes told me that he thought that if Lorentz had used his authority a little more frankly, then the thing would have come out all right. We would have admitted the Germans somewhat earlier I think.

Heilbron:

How did it happen that you saw the Bohr articles? Did you read the Phil. Mag.?

Fokker:

On, yes, I read the Phil. Mag.; that was English. Others did not read the Phil. Mag. there, or less than I.

Heilbron:

Yes, but Einstein just didn't look at it in general?

Fokker:

No, no; I don't think so. With Einstein I have done a little thing on the gravitation theory there. I am more on the line of gravitation theory than the relativity, which I do not call relativity anymore because it is not relativity; it is differential geometry. It is absolute science, and this name relativity is very unhappy, very unfortunate indeed. Now when I was in doubt Ornstein proposed to me to write a book on relativity ... and I accepted. Then when this great revolution came from the new quantum mechanics, I was quite absorbed by this book I had to write, you see. That is the reason why I have not taken so much part in it. [Goes to look for the book.] It's this one you see, and it's the only book on relativity in Dutch. It bears a year mark of 1929; so it is just the last few years that I have been quite absorbed by this work, and I couldn't do better at that time. I remember at one time Kramers drew my attention to the work of Dirac; that was not yet the electron — the relativity theory of the electron — but the other things. And I was very much impressed by it, you see. And I spoke with Lorentz also, but it was such a new way of thinking and of working that Lorentz tried to follow the conclusions of Dirac by his own methods, you see. He had a great difficulty in assimilating the ideas of Dirac. Of course, it was very difficult. But I was very much attracted by it. I was also very much struck by the papers of Robb, Alfred A. Robb, who wrote about the theory of Einstein. In fact, he wrote an axiomatic theory for Einstein, avoiding the word relativity. That was on the absolute relations of space and time, you see. Dirac was in Cambridge and this Robb was in Cambridge and I wanted to pay a visit again to Rutherford. Then I combined it: I went to Cambridge, and I went to see Rutherford, who had moved from Manchester to Cambridge. There it was very agreeable; I made the acquaintance of these people, the youngsters of Rutherford who were working up there. And then I wanted to see Dirac, and was very much pleased. Dirac is not — he doesn't use many words. He came later to Leiden; he was invited by Ehrenfest, and so on, and there we should have a meeting and Dirac would answer questions. Ehrenfest had told him he should be willing to answer questions, and he had agreed; he said, "I'll do it." And first of all — I don't know who it was that asked the question — but Dirac replied, "That is not a question; that's a statement!" But anyhow I went to Cambridge, and there I made the acquaintance of Dirac and was very much pleased to see him. Dirac later has also stayed a week or ten days here. And there was Robb, and Robb was so much astonished that someone would come to Cambridge to see him. I did not make a great impression on Robb, because of course he has this axiomatic thinking, and he was much more learned than I in that. But still I am thankful that I once met him, you see.

Heilbron:

You began your work in relativity just after the war?

Fokker:

Yes. Well, when, I went to Zurich. Of course I (???). I had also edited the lectures of Lorentz on relativity, and then Lorentz also gave the theory of gravitation in his lectures on the basis of the restrained, restricted relativity theory. But we did agree not to publish it because when I had written the chapter on that, well, then the theory of Einstein had come out. It had not been out previously. Then Lorentz said, "Well, there's no use in publishing this; it's disrupted." But there it began.

Heilbron:

I wanted to ask you about these two articles that were published in 1914 that grow out of your thesis; the one in which you calculate the average energy of the dipole and the other where you calculate the specific heat, and find that it goes entirely different from the experiment.

Fokker:

Why, I didn't find this paper on the dipole yesterday when I went through things, and I wonder whether there is something special to be said about that.

Heilbron:

Well, the question I wanted to ask was really one of reception. I wondered how people had regarded that.

Fokker:

Oh, I don't know anything about that; I never got letters about that, no. It was not long before the war broke out, you see, and then daring the war the connections were broken so much. No, I don't remember that. It was another proof that the classical theory wouldn't do. I had quite forgotten that I ever read something about Kroo — "La Theorie des electrons à l'intérieur des Atomes." I don't like this paper anymore, no.

Heilbron:

But at the time —

Fokker:

At the time you see I caught tuberculosis during the war time; I had to do my duty in the army, and then I got tuberculosis. It was not discovered at that time, but when they looked after it, it was not open, you see, so they couldn't find any bacteria, but still it was there. Therefore, after the armistice, that was in the winter 1918, I went to Switzerland, to the Erholungsheim and made my cure there. And I was there two or three winters I think; I think three winters. And there I had plenty of time to calculate the things. And I attacked things like this: working out theories for the field strengths and the potentials in the various orbits, and when I see something like that it's terrible, it's terrible.

Heilbron:

How did you happen to select that problem, do you remember?

Fokker:

No, no, no. There was something there — a discrepancy and I thought perhaps by these retarded potentials we can find a correction, but when I see what I have written here — As a matter of fact Ehrenfest didn't like these papers at all. He was quite right, and I don't like them anymore. And I had quite forgotten the name of Kroo now.

Heilbron:

Yes, I thought it was not at all in the spirit of your usual publications.

Fokker:

No, this is a little bit too great detail, you see. This is perhaps a little far behind, but a little bit the position of Lorentz too, to look after the details. Several times Pauli said to me, "Well, I read your dissertation and it was a good thing." And I know Pauli; by praising this he said, "In the other work you have clone nothing. You are good for a factor of two or a plus or minus sign and some such special detail, but that's all." But I had revenge later on; in '55 we had this great meeting in Berne for celebrating the theory of Einstein and then in closing a speech Pauli said, "Things are happening in space and in time." "No, no," I said, "not events in time and space, but the space and time in the events," you see. And there I had him, you see; he never answered me on that. In the later years I have been impressed just by this failure to —. Everybody states that things happen in space and in time. There are events happening, and in all these events we make the use of relations of space and the relations of time. We call them so, but there's no space and time we find which are present around the world, and which are being filled. No. We distinguish space and time in the events, you see. And that is one of the things also which perhaps I have learned from (Rayleigh); who can deny that? And in that respect the word relativity has made a great deal of people just say stupid things; they're horrible, horrible, and they're like — And in the beginning the people have discussed about this relativity of simultaneity. And Robb objects to this expression relativity of the simultaneity; it's quite right. They have spent so much ink on that question, but nobody has realized the very revolutionary conception of interval zero. As for that zero — for a mathematician it is quite trivial, it's a formula just like any other, but no, it is a real, important thing. It means not only action in the distance, but also action at a distance of time. It is just a kind of contact, if you calculate the retarded potentials. … Now, we say the interval is zero, and I say, "No, it's just a kind of direct contact." It's mystical. In my student time I was very much impressed by Professor (Speiser); that was the man of Sanskrit. I followed his general course on these mythological figures there and the mystical things, and I read the book of (Muller) and that too was a department where I would have liked to explore. Now I'm very much impressed by this idea of the interval zero, and I call it the presence of the past and the not present. You catch me? And the light cone is just my presence. I have a friend who told me that this idea was also in the Confessions of St. Augustine.

Heilbron:

I noticed that here.

Fokker:

Yes, I borrowed the book from him, and that is a real fundamental thing. You can say that it is one of God's secrets that we are interpreting. And if we live and we are thinking here, just wondering, we are living interval zero. If we recollect what has happened yesterday, or a year ago, and at the present here and now, things and events which have taken place there and then — in our common way of speaking. And that is a much more important concept than the relativity of simultaneity. But now we are not speaking anymore of quantum theory.

Heilbron:

But getting back to quantum theory for a minute, I was wondering whether you recall anything of the genesis of this work of Ehrenfest and Burgers on the adiabatic invariance.

Fokker:

Now, the adiabatic invariance I think one should look up in the papers of Ehrenfest. They have been collected and edited now. Well, the adiabatic invariance. I think Ehrenfest took it over from Boltzmann. And Burgers was working in Haarlem with Lorentz when this competition was put by Lorentz, and then he answered him very beautifully. You see Teyler's Foundation in Haarlem was founded some 170 years ago by last will and testament of this Teyler who was a very rich merchant in (Sirk) and had no relatives. He was very keen for the advance of science and of theology, and he made this foundation. And so this foundation has just had the capital to rely on; this capital has been very much reduced because of the two wars. Of course the first war abolished all investments in Russian shares, and the second war was not very much better, so now they are rather poor. I think that the municipa1ty of the state has to take over the responsibilities because they can't manage anymore. Things have become so expensive, and incomes have not increased in the same rate. But then before the first war they had some money, and when Lorentz came in 1910 and '11, they wanted to relieve his burden of university teaching, and they created a chair. They had a laboratory there for physics; of course they had a great collection of historical instruments and a lot of people. But the man who was in the laboratory was not so very good; I don't think he was very good. They were rather isolated from contact with others. But one time they said, "Well, we must make a great thing of that, and we shall make an organization. We shall give Lorentz a position where he will be able to do what he likes; he will have all of his time to himself to work and next to him we'll make a conservator. This man will be relied upon to make experiments in the laboratory, will also be on the staff." So that was started in 1910 or '12 — in 1912 it was — and that was the reason Ehrenfest came as the successor of Lorentz, and they created a special chair for Lorentz in Leiden. Now, I believe the first one to work under Lorentz was I think it was (???), or Elias, but Elias had been working in Berlin with (Dubois). But he went as a professor to Delft, and then (Birch) came. Then there was Burgers, who was in charge of the laboratory and made experiments there; and when he was there he just wrote that brilliant book on the Bohr atom. But soon he left for the chair in hydrodynamics in Delft; he was 23 years old when he accepted this chair in Delft. And there he started a laboratory and he did some very good work. Then after Burgers — meanwhile the war had broken out — Wander de Haas, the son-in-law of Lorentz, came. He had been working in Berlin where he had worked with Einstein, and he didn't like the situation there, and wanted to return home. He had returned and he had taken a post of teacher in a gymnasium in (Deekendorf) quite near here, and when Burgers went away, Lorentz called de Haas to his laboratory. De Haas worked there for a couple of years, and then de Haas too was appointed professor of physics in Delft. After de Haas there came van der Pol. Van der Pol was a man from Utrecht, who had worked in England on these electron valves; he continued this work in Haarlem until he was called away by Philips to have the direction there of the electron valves and what you can do with it — the signals and so on. And he established the first communication by radio with our Indies; that was van der Pol. After van der Pol there came Coster to Haarlem, and Coster, too, got that chair in Gronigen; then for a long time this post in Haarlem was not filled. Meanwhile I had worked in Delft, and then Lorentz said he wanted to ask me, because he wanted also someone who could succeed him at the top in Teyler when he wouldn't be there any longer. And that was how I came there to Haarlem; there is not a college and there was nobody which made appointments. It was just Lorentz who asked and advised, and of course his recommendation was approved by the board of directors, or what they called curators of Teyler's, you see.

Heilbron:

Was that the same way under you?

Fokker:

Yes. And when Lorentz died I followed, and I followed also in the special chair in Leiden.

Heilbron:

What did that involve; just a lecture occasionally?

Fokker:

No, it was not a special lectureship; it was just what we call an extraordinary; no, a special professor, you see.

Heilbron:

Had you any duties with respect to that position?

Fokker:

I could choose what I would do myself, you see. I could also have a man there as a man for the laboratory, but I hesitated a long time because it was such a close union, you see. There was a very able physicist, a woman in Utrecht but I did not dare to ask her for fear of complications. And then there was Prins in Gronigen, but I didn't dare to ask him because he was a very aggressive man you see. But then I got Gorter from Leiden. Gorter left Teyler to become a lecturer in Gronigen; afterwards he was professor in Amsterdam. After Gorter I had the help of (Rathenau), who now is in Amsterdam.

Heilbron:

Was Casimir never there?

Fokker:

No, he was never there, no. That was that. Now you ask about the Delft time. In 1921 I came there to Delft as a teacher at the gymnasium; that was after my return from Arosa, from the sanatorium for my lungs. At the gymnasium it was just the time that they reorganized the physics in the gymnasium; there was more time given for it, and there should be a new curriculum. I said, they must not make the same thing as with the Hochburgerschule, the HBS; we must have a proper program for the gymnasium. So in these times, for two years, I was very much engaged in propagating this distribution of the matter over the year, and so I was engaged in things of didactic importance. That lasted two years; it was then de Haas who was professor in Delft. He was called to Gronigen and then de Haas wanted to have me as his successor in Delft.

Heilbron:

Was it recognized that you were in line for the next chair around? It was clear that you weren't going to spend the rest of your time in the gymnasium?

Fokker:

Well, I never promoted the idea that it was self-evident that I should become a professor, but de Haas wanted me to be starting there. Now, in Delft, when I was a student there it was very much worse than later; everybody admired practice. Practice was magical. And a figure like (Stodola) in Zurich making researches and new developments was impossible in Delft. They were in that respect a little pig-headed. It was a great fight for Wander de Haas — I mean, this de Haas is another man than the de Haas who was there who sent me to Lorentz; this was the son-in-law of Lorentz. He wanted them to do a good deal with research work, you see and he also had a great struggle for doing research work himself. And he thought that I would also push in that direction, to make it more scientific, and indeed I tried to do so. But there were some problems there; we wanted to have a special training course for physicist-engineers, you see, and there was a great trouble to get that through to the colleagues in Delft. And also to get the support of the physicists at universities. In Utrecht it was Ornstein who was much against it, because he wanted the career of physicists in industries for his pupils, you see, and he did not want the engineers for that. So that was a great trouble, and a difficulty. Then we needed a new building for the laboratory, and that was also very difficult, very difficult. There was a strife between the government in The Hague, and the Curatorium in Delft, and when I had the support of the Curatorium in Delft then they were against me in The Hague; and when I had the support of The Hague the Curatorium was against me. So all of it was a little bit like that you see, and then came Lorentz with his offer to come to Haarlem. He was very generous, didn't make any haste, and was there to arrange things. Then I said yes because I was a little bit disgusted by all these troubles in Delft.

Heilbron:

There is a very large building program now which —

Fokker:

Yes, we have got the building; we have got the building which was intended for another man, but we got the greater part of it. But again they have outgrown it, and they have a great new Iui1ding now. I haven't seen it yet. But I'm telling all sorts and kinds of anecdotes. On Lorentz' Jubilee, they instituted a Lorentz Fund which can grant money for scientific purposes. And also there was installed a medal, the Lorentz Medal, and the first man to receive the Lorentz Medal from the hands of Lorentz himself was Planck. So we had a great dinner in honor of Planck, and at that dinner I was placed next to a great engineer and he was also a minister of education. No, of (???). He was what you call a civil engineer; he was the man who made a plan for reclaiming the Zuider Zee. He was, you see, a very important man. He sat next to me, and then he said to me, "Why are you leaving Delft?" And then I told him this background motive which I did not announce publicly, because I didn't want to discourage my successor. And I said, "Well, we've had troubles with this institution of engineering and with the building, and I do not want to go any further." And the next week it was known that this man next to me was nominated president of the Curatorium of Delft, so I had put the idea just in the right place! And then within a year the things were settled. On the opening of the new laboratory the man in The Hague said to me, "Well, Professor, you are the man who has made this!" — just by going away, you see. Now that is a private story; that is not quantum theory. But you ask about the foundation of the Physica. This cost a lot of time and also this work in Delft. This work in Delft where I had to stand up for new things and make things go, also came at a time, 1924, '25, when Bohr in Copenhagen and other people were making new quantum mechanics. So that was a great distraction of course:

the Physica was a distraction, and for five years I was president of the Dutch Physical Society also.

Heilbron:

And you intended to do more work in quantum theory after you finished this —?

Fokker:

Most things had been done, you see.

Heilbron:

No, after you had completed this paper [on Kroo's theory] had you intended to go on working in quantum theory before you got sidetracked by these other things?

Fokker:

Well, it was very interesting to me, and I wanted to come along, you see. Therefore I went to see Dirac, also and these people. But I wanted to say, this year when I was in Arosa; that was a very important year. By correspondence with some friends we had made some good institutions. Holst was propagating the idea of a Physical Society; we did not have that. We had no Physical Society in the Netherlands; we had a group in Utrecht, we had a group in Leiden, we had a group in Amsterdam, but it was all separated. And Hoist said, "We must have unity," and I was one hundred per cent for it, you see. And the second idea we had was to start a periodical; that was done in the year 1921. Then they founded — I was in Arosa —- the Physical Society, and we started the first journal. We were three editors: one was Dr. Oosterhuis, a physicist in Eindhoven; the other was van der Pol, a physicist in Haarlem with Lorentz and myself. Then we started this Dutch journal Physica, and got on very well; I tried to persuade the colleagues to publish their things there, you see. But of course we had a restricted audience, and especially Ornstein said, "No." He didn't want it. He was in the laboratory and he was very prolific and all went to the Zeitschrift fur Physik. But then came the Nazis, and then came 1933. Then Ornstein saw that there was no use for his papers to be sent to Germany because they wouldn't take them — he was a Jew. And that convinced him to collaborate with the others, and in 1933 we founded the International Journal. At first we had the title Physica Hef Nederlands Tijdschnft voor Natuurkonde and then we could write Physica in English or French or German. Then I was editor-in-chief of Physica, and I was the only one. I had a staff of local editors from different universities, and what they sent to me I took, unseen, because they were responsible. But when other people sent in their papers to me, I would judge them and perhaps I would ask some advice from a colleague, but there was not a regular board of editors.

Heilbron:

In the earlier times, how were the papers refereed? Before the split?

Fokker:

Well, we did it; we were the three editors you see.

Heilbron:

That was all? You didn't refer the papers to specialists?

Fokker:

Oh, no there were no specialists for that. And before this came, we only had this Arch. Néerl. which was very slow, and of course it is much more for great treatises and long things; we always tried to have short things. So that is about Physica.

Heilbron:

Occasionally articles would appear not in Dutch?

Fokker:

Oh, in Physica? Oh, no, never in Dutch.

Heilbron:

In the earlier one.

Fokker:

We started a Dutch journal which occasionally had things in French; we didn't object to these things. At the Jubilee of Lorentz we gave a special issue of all international papers.

Heilbron:

I also wanted to ask you a bit about the interpretation of the new quantum mechanics, the probability interpretation, and so forth. How did you feel about that?

Fokker:

Well, you spoke of the pilot waves of de Broglie, and I didn't like them. No, I didn't believe in them. And the interpretation of the probability of course was quite acceptable to me. Of course you must not stick to causality you see, so Einstein could never accept this idea of probability you cite him: "Lieber Herr Gott —." It is curious to see how reluctant Einstein was to these new ideas. In his time his ideas were quite new and everybody objected to them, but now he objected to new things of Bohr. So you see there is a great difference in these two generations, and the generations lie apart, let us say, ten years.

Heilbron:

Well, was, there much sympathy at all for Einstein's views in Holland?

Fokker:

No, no. We were of the opinion that Bohr was right, Bohr and his people. I believe that Bohr on the occasion of Einstein's Jubilee had written this very important paper on the dp and dx and the impossibility of making exact measurements.

Heilbron:

Yes, the uncertainty principle.

Fokker:

And I think that there also Bohr refers to a certain discussion with Einstein and Ehrenfest, and that Ehrenfest once said to Einstein, "But Einstein, you are a terrible man." He scolds, and says, "Don't you remember what happened to you when you came with a new theory? Don't say the same stupid things now to Bohr."

Heilbron:

Ehrenfest was pretty clear when he spoke.

Fokker:

Oh, yes, certainly. Oh, yes, and he was great, great friends with Einstein.

Heilbron:

You mentioned earlier that the school at Leiden ran down a bit towards the end of Ehrenfest's life; was it clear that Ehrenfest's personal difficulties were making problems?

Fokker:

Well, I think that Ehrenfest lost a little bit of his self-confidence, and he was more and more inclined to send people away. Ehrenfest was at his best when he had new people; he was always very excited and very happy when he made the tour through America, because then he could speak to ignorant people who had no past. And then he had enthusiasm and could make things go, you see. And that was his great force — to set things going. But when he had the task of finishing them he shrunk back a little. Then he was inclined to say ("Just go to him there and just do it there.") In the last part of his life he also had some moral difficulties, so to say, and he lost a little of his self-confidence. And he always complained of growing old; he said, "I have the feeling that I just rush after a tramway car to catch it!" And I think that was the reason that from the inside he did not have the force just to push as he did in his earlier years.

Heilbron:

It was clear that his personal difficulties were getting more and more intense towards the end there?

Fokker:

I think, yes, I think, I think.

Heilbron:

But his death was no surprise, particularly, to anyone?

Fokker:

Well, a half year, or a year before, he told his friends about this purpose. He said you can do nothing about it, because you can't say leave it alone or don't do it. You see they told him not to do it, but it was no use. You know he had an unhappy child, a deficient child, and he took the life of the child at the same time. Now if we can stop that, I can tell you some other things. [Machine switched off] If people ask me what is my subject, I have two poles. And one pole is technical — engineering and the other pole is abstract — physics, philosophy. And now these acoustic things begin from my activity in Delft. When I came to Delft, I asked my colleague in architecture, "What do you want that I shall give to you people," because they had provided a special course for those becoming architects. Then they said, "Acoustics." I said, "I'm sorry I don't know anything of that." But just a month later I saw the collected works of (Sabine) in America, which had appeared. (Sabine) had begun acoustics of rooms in some previous years, not very long ago, and I found just the stuff I wanted in that book. So I began to study the thing, and then from that grew the idea of making reflectors of sound. In churches you have conflicting requirements; you have the requirement of understandability — you must hear what is said from the pulpit. And on the other hand you must have the reverberation for making the music acceptable. In the church you can solve the conflicting problem just by making a reflector, you see: directing the sound from the pulpit just to the audience.

So the audience is absorbing this speech, so the speech does not suffer from the reverberation, and the reverberation is still there for the music, which comes from another source. And that was my work; I have been called to several churches and there are some very good —. The other thing came about in the war time, because then what we call (???), the Dutch Society of Sciences, published the twentieth volume of the collected work of Huygens. There I read about the division of the octave and the remarks about the consonants of the sevenths. It struck me and I went to the musicians which I knew asking, "What do you do with the sevenths?" And one said, "I don't know." Another said, "We do nothing with them." And in the Encyclopedia Britannica there was a great article on the music and on harmony, and it said, "It is really very disappointing that still now we don't know what to do with the sevenths and how to arrange them." Then they gave some examples of music of the bag pipe, with enigmatic intervals. So I got into that line and I could explain the bag pipe phrase. Then I thought it should be necessary just to arrange to have these sevenths in a few forms, a composition — I can't find the word.

Then we constructed a small organ where we could make combinations of intervals which make a closed whole. That is to say I got into touch with van der Pol who told me that Euler had made a theory of music, and Euler gave just the way to combine notes with certain intervals so that you get a good whole, a consonance, you see. And he called it "Genus Musicum." You may hear a note and the fifth, and the fifth again; and then you have three notes; and then you come back for the third like that [demonstrates] and the third like that; then you have nine notes. And that is called ("Genus Chromaticum") by Euler. So if you have a new interval; that is the way to make it into the building of the whole, you see. Then on the organ I had made ten of these very simple "Genera Musica" just to experiment. Then the next step was to make a real organ with these ten "Genera." I had only twelve keys, and one at a time I could replace them, but of course you need an instrument to be able to play all of them; all these kinds of transpositions. Now I got into the trouble that if you want to have a tempered scale — you must get there — how do you arrange the number of steps so that you can represent fairly well thirds and the fifths and the sevenths? That is a mathematical problem, and I went to mathematical friends and asked, "Can you give me a straightforward way to find the best number possible?" And then they said, "For two intervals we can do it; it is a matter of the continued fractions, but for three, no. (Nacoomy) has tried it, and he did not find it," and so on. So they said to me that they would try this. And I was so happy. Now Huygens has given the number 31 and I was so happy to refind this number by just very simple reasonings, by approximation. And so it is clear that by 31, you can produce the third practically pure and the sevenths practically pure, perfect. The fifths have a little bit to suffer from that; the fifth is one quarter of a comma short. You know perhaps what a comma is; that is the relation of 81 to 80, you see. And on the pianos now and then organs now in common temperament the third is 2/3's of a comma out of tune. It's stretched too much, you see. There is a great difference —.