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# J. M. Burgers

## June 9, 1962

Interviewed by: Thomas S. Kuhn and Martin Klein
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# Abstract:

Part of the Archives for the History of Quantum Physics oral history collection, 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: Cornelis Benjamin Biezeno, Niels Henrik David Bohr, Tatiana Ehrenfest, Paul Ehrenfest, Albert Einstein, Abram Feodorovich Joffe, Heike Kammerlingh Onnes, Willem Hendrik Keesom, Hendrik Anthony Kramers, Hendrik Antoon Lorentz, Wiersma; and Teyler's Tweede Genootschap.

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## Burgers:

[in mid sentence] As far as I can remember, I have never heard that Ehrenfest made a coarse joke, not ever what you would call a dirty joke. And no joke bordering on ambiguity, which Einstein could do sometimes, and Pauli certainly could do, but never such a thing came from Ehrenfest. And he had a fine sense of humor, which you know. He could use fantastic images or expressions to impress upon us the strangeness of new ideas in physics, or also the hollowness of assertions sometimes made by people. And you know he used expressions as ("Abteilung weiterer stetiger Beschwindelung,") i.e., "somebody tried to lead you along a road without precisely indicating what he was doing." But his humor in regard to human matters and personal things was always mingled, I would believe, with a sense of our limitations. He, perhaps, through this also missed the relaxation and relief of tension which coarse jokes sometimes can mean for people. He never reacted this upon us. Einstein and Pauli could relieve themselves sometimes from tensions in this way. And I would add that in my opinion, it is a matter of course feeling, and of being personally attracted to him. I would think that from the human point of view Ehrenfest was better, or finer, even that Einstein and Pauli. Much of course as I appreciate them, and much as I know that Einstein especially had expressed thoughts which are considered of such great importance and are known by many people, I still think that there was in Ehrenfest something more true, more honest, more aware of his limitations. This is a factor that has also led to his sad end. He just couldn't figure out anything and he just took the one mathematical consequence: I should no longer live. And he could not find any way to cover this up, or to make something else out of it by some hypothesis — let me say it in that way.

## Kuhn:

To what extent do you think that among the many problems which led him to this conclusion was perhaps also some feeling that now physics itself was moving away from him?

## Burgers:

Ja. That certainly has been the case, I believe. I remember that he had been talking with Wiersma, who was then still in Leiden as a student. [See letter of June 1962, Burgers to Kuhn, attached.] He was one of the (few students) of Ehrenfest. I believe Wiersma lived also in the (???) (Statt) opposite Ehrenfest's house. And Ehrenfest told us of the difficulty he had and the desperation he felt in regard to (spinor) analysis and all these things which mathematically didn't seem to lead to anything and which exasperated him. And I believe that certain features of this kind may have combined with others. And then, he expressed it always, "Well I am not a man who finds out anything. I am not worthy to sit on the place of Lorentz and I should make this place free and available again." And the Dutch law just had no position for him where he could still earn sufficient money for a living in view of the heavy obligations which he had. Of the people whom I know, but I know him very little, I would say that Bohr might have the same purity of mind in this sense.

### Kuhn:

Both Dr. Klein and I have been looking at the thesis. I may say with some difficulty because I don't read any Dutch.

### Burgers:

Ja. If you look at it in Dutch, then I can understand. For me, I have the feeling I did not bring much in it. What I did at the first point was of course these adiabatic invariables. There I did something which brought matters a little step forward, but as you already expressed it, it was something of working out a puzzle where you were convinced that it should be there. I mean Ehrenfest was convinced, I was convinced. It was only the question how to turn the equation backwards and forwards so that the various transformations fitted into each other and you got the proof out of it… I was convinced that these integrals should be invariants. As I mentioned, I have not the deeper insight what this really means in explanation of the models of the universe. But I mean I was convinced that mathematically they should be invariant. And so the question was only how to show it.

### Kuhn:

Now these are the phase integrals that you're speaking of.

### Burgers:

The phase integrals… Ehrenfest had just written a long paper which was in the Amsterdam Proceedings about adiabatic invariance. That of course referred to the quantum condition for angular momentum and the quantum condition for the oscillator. And just about that time — oh, it may have been a year later; it was 1916 I believe — Sommerfeld and Epstein's work came out. There was all this correspondence. And I'm fairly certain that in our conversations about it the idea would have come up: 'Would this not be an instance of an invariant.' Of course you know that this work was based on the idea that the variables in the Hamilton-Jacobi equation can be separated. Now, if the system is so simple that you separate into separate oscillations, then you are sure that every oscillation is an independent entity which has its own energy and its own frequency and its own invariant. Here it was one step more complicated, but essentially it was so close, it was so near. This was an idea that just hung in the air and who has expressed it first I do not know. Whether Ehrenfest said, "Well, you could try," or whether I said, "Well, I could try," that is not to be found and is not important.

## Kuhn:

And not of consequence, no. Let's go back to the thesis as the prize essay for Teyler's Tweede Genootschap. Was that prize always in physics, or was the subject set every year?

## Burgers:

I believe that the subject was set every year but it certainly was not always in physics. It may have been for instance also in paleontology or in medical science or something like that. There were two societies. You know the history that Teyler — it is taken from the British 'Taylor' — was a rich man in the textile industry in the 18th century. He left a big estate and capital which is still being contended by his descendants (in Dutch society). They were always protesting and bringing up money to — at least it was 40 years ago, and I don't know how it is now. But this matter was always in the courts, but it always ended that the foundation had no money to pay legal council. There was a foundation that was governed by five directors. There were two societies. One was a theological society, and that sets a prize essay every year. And the other was the second society. The second society combined physics and paleontology, and I believe also medical things. Ja, and also art. Because you see the foundation which founded the museum has a wonderful collection of paintings and of drawings and has the geological museum and physical instruments and various things. You know that some of the world famous fossils were still in this museum?

## Kuhn:

On the essay itself, who do you suppose set the topic? Do you suppose Lorentz?

## Burgers:

I suppose Lorentz. It was of course not public but it was very evident, Lorentz was (asked for advice). Kramers had not known about it, and I have not taken the trouble to mention it to Kramers. Kramers was at that time already away. He was working with Bohr.

## Kuhn:

How many essays do you suppose were submitted for that?

## Burgers:

I think I was the only one…It was open for everybody. It was not widely publicized. There was just a printed notice hanging in the library.

### Kuhn:

It was intended then that it be a thoroughly technical essay. It was not supposed to be addressed to a wide audience?

### Burgers:

No. It was intended to be a scientific essay.

### Kuhn:

The terms of the essay refer to the Rutherford atom. You make it the Bohr-Rutherford atom. Why shouldn't it have been the Bohr-Rutherford atom in the first place?

### Burgers:

I should have to look up that… Ja, apparently you are right.

### Kuhn:

I wondered, for example, whether that meant that in Lorentz' mind, there was a topic that was about the Rutherford atom but on a classical basis, and without going in very deeply to the special Bohr quantum conditions?

### Burgers:

That I really cannot remember, but in my mind the two things were closely connected.

### Kuhn:

As you worked on this and worked through the literature on this, did you do this mostly by yourself or did you work constantly with Ehrenfest on it?

### Burgers:

No, I had been working much with Ehrenfest. Let me see, I had some other publications before the thesis was written, and especially one was on the rotating atom. The question was whether you could add the rotation energy to the energy of the electronic states and that this could explain the displacement of the lines which you have in the spectrum of a rotating atom or a rotating molecule. What I did not find out, what I had no idea of, a point which Bohr explained later to me. If you just assume only the simple form of a rotation energy, then the rotation energy is the square of the quantum number. Now if you write all the differences of two squares you can get a lot of numbers, but there are some which you never can get. I think six is not the difference of two squares. So I mentioned that somewhere there. And later Bohr told me what was the matter with this. You can have only the single jump from n to n-1. So you will always have an odd number for the difference between the two squares. And that dives regular spacing, not with single units but with double steps. You see what I had found then was just not sufficient to make such a point, and that was Bohr who found these things. Another point on which I had been working and this was with Ehrenfest, was the question of forced oscillations of systems which were subjected to quantum conditions. Before I started to do anything myself, in 1916 Miss (van Leeuwen), who was the last one who made her thesis on magnetic systems with Lorentz, had worked out the free oscillations of the Debye model of the hydrogen molecule. She just assumed that the orbits were given there, and then calculated the free oscillation in order to see whether you could explain the absorption spectrum of hydrogen, and that she did not succeed in doing. Maybe it was not the absorption spectrum but the dependence of the index of refraction on wave lengths. And then since I was working rather much in Whittaker's Analytical Dynamics I found that he considered also systems which were subjected to non-holonomic conditions, and you could use the quantum condition as a non-holonomic condition and then work out the free oscillations. So that was the thing which I did which was my first paper. I did not get much result either. But I was still interested. And then later on I came to the idea that if you have a system subjected to quantum conditions, and you wish to investigate its behavior under exterior forces, then you can take the following picture. You extend the system with an oscillating system with very heavy masses and couple the oscillation to the motions in which you are interested. Then work out quantum conditions for the complete system, in which the oscillation has its quantum condition, and the normal motion of the system, somewhat influenced by the others, has its quantum condition. And then you can make the mass of what you have added larger and larger so that you have almost an impressed exterior force. That I did I think in the later part of 1917. And so there were these things on which I had worked, and there had also been discussions with Ehrenfest on the application of these ideas to the theory of dispersion by a gas, but not much has come out of it. So I had constant contact with Ehrenfest in all these problems. I believe that when I worked out this I did it myself, but of course the Leeskamer where we were sitting had all the literature, and Ehrenfest had treated it so much, and so many points were discussed at his colloquia, that you could just as well work by yourself and still be imprinted by all the things which were in the air.

## Kuhn:

You speak of studying very carefully in this period Whittaker's Analytical Dynamics. Was it brand new then?

**Burgers:**

[looking at a book] [But see appended letter of June 14, 1962, Burgers to Kuhn] Cambridge, 1917. I think there was only one edition of it. The other book, Modern Analysis, had appeared in 1915, I think in a standard edition, which we had. And then I came to Analytical Dynamics I see only in 1917.

**Kuhn:**

That's presumably the first edition. Was that an important book to this line of development for you and Ehrenfest?

**Burgers:**

For me it was. Afterwards I think that perhaps it was a bit too formal. Sommerfeld and Epstein had referred to Charlier, Die Mechanik des Himmels, the astronomical things. And that gave us all the information about the solution of the Hamilton-Jacobi equation and the separation of variables.

**Kuhn:**

You used Charlier too?

**Burgers:**

Ja ja, of course. It was also in the Leeskamer, so I looked up the things. And then further I had been looking partly in Poincare's Mécanique, but I did not get much use out of it. And I was interested in Whittaker's presentation. Whittaker gave much attention to the neat treatment of contact transformations, and so I felt somewhat in favor with them and used them. But at the moment I still have the feeling of more formalism than real thing. And I know that Epstein was not quite happy about Whittaker. I mean, the later chapters of Whittaker give some indication of the development of analytical systems as you use in astronomy for series development and for complicated Fourier series. I believe that Epstein in a letter to Ehrenfest expressed the idea that Whittaker made it somewhat too simplistic and left out important things and that one should rather study Delaunay, Mécanique Céleste, but I have never done that. But I was attracted by his formalism and I used it.

**Kuhn:**

Was Ehrenfest at home in this formalism as well? Did he work himself in to the contact transformation point of view?

**Burgers:**

Certainly he was well-acquainted with contact transformation. I do not think that he worked so much in Whittaker directly, but this was of course a general principle. I do not quite remember how I came myself to it. I think it took me some time before I got the proper insight in what Lagrangian equations were and Hamilton transformations. Then Lorentz once gave a series of lectures on mechanics, in which he quite nicely started with these point systems, and then introduced first the kinetic energy and the Lagrangian function of the point system and then indicated how this can be expressed if you have generalized coordinates. So gradually I got these ideas in more precise shape. And then we read very much also in Hertz's Prizipien der Mechanik which also has a formalism which is very well cared for. And the idea of contact transformations was in the atmosphere. I do not quite know whether it came from Ehrenfest or from someone else. You see part of all attention to these things went through the discussion between all of us. There was Kramers, who was very much acquainted with it. Costar was more experimental, but still he talked about it. Then there was Struik, who was a very good mathematician, so Struik's contributions have also been in these matters.

**Kuhn:**

Did Einstein come to Leiden in those days, and did you have any talks with him about these quantum problems at that time?

**Burgers:**

Not myself very much. But Einstein came in 1917. That was the first time I think that it was possible for him to travel to Holland, and that was after he had finished his work on the theory of gravitation. And then there were constant visits from Lorentz and from Van der Waals, and everybody came to Ehrenfest's house to discuss with Einstein. And there I was out of it. I mean, there were so many other people of great importance who had to discuss with him. I have seen Einstein a few times and been on a walk with him, and Ehrenfest will have told somewhat about him, but that I do not rightly remember. Einstein's interest in that time was connected with the other phase of quantum mechanics. I think already in 1917 he was working out his version of the deduction of Planck's equation, the spontaneous and induced radiation. And then there were these problems which I have never quite understood, at least not in that time, of the difference between Nadelstrahlung and the way in which the permutations have to be counted. You know that was one of the big problems in that time.

**Kuhn:**

How did Ehrenfest feel about the needle radiation problem? The whole notion of treating light as photons?

**Burgers:**

That I cannot remember sufficiently precisely. I know that he was very much interested. I can quite remember there were intense discussions between him and I think Zernike and (Isemann) about that, but I have not the proper feeling for the problem, and I have no proper recollection of how the points of view were in this case…

**Kuhn:**

Did you have contact with Kramers after he went to Copenhagen, I mean in the years when you wore working on this problem, did you?

**Burgers:**

No. When Kramers was in Copenhagen I had no correspondence with him. I mean from time to time — once or twice perhaps — I had a letter, so that I knew he was engaged there.

**Kuhn:**

But there was no contact say about what was going on in Bohr's group?

**Kuhn:**

At this time how do different people — yourself, Ehrenfest, others — feel about the atom model itself? Is it clear that there is fundamental trouble, and if so which problems seem to look particularly fundamental? What is secure at this point, and what is it that one is hoping to work out, or to change?

**Burgers:**

I find it very difficult to reply to that, and really, as I have mentioned somewhere in the biographical note, I have not quite the (fantasy) to feel the weight of these problems so strongly. I have not quite at the moment anymore the recollection of how Ehrenfest reacted to them. Some of these things of course intensified gradually. But in 1918 I turned off from the whole subject. You see it happened so that I felt a little bit my lack of inventiveness and my lack of making a program in these things for myself. I was these nine months in Haarlem with Lorentz. This then came even somewhat more to the foreground, and so I felt rather attracted at once to start on a new subject, to break off the old things. This was a pity in many respects, but it has happened. Because of this change, I got so many other things on my mind that I was no longer quite attentive to Ehrenfest's observations. Then there is another point, and that is not for the record, so to say, but I have indicated here and there that everyone of us after a certain time turned somewhat away from Ehrenfest. Ehrenfest was so intense in his questions and so penetrating that at the moment you had the feeling, 'I want to keep certain things for myself.' Especially because of the difference between Ehrenfest's analytical mind and the nature of several of us, the desire not to probe into everything came up sometimes. I have often thought about whether here there is a difference between the Jewish mind — especially many European Jewish minds, educated in Germany and Austria — with its desire to probe into everything, and the non-Jewish mind — I would not say Christian mind, but perhaps (???) mind or something like that — which does not like to analyze everything and has no other defense than to go away. So that was also in this same time. There were so many political things that then happened in the world and about which people have different views and young people sometimes want to think separately from older people. And then finally I became engaged. I penetrated into the world of relations with other people in quite a different way, so that I turned off somewhat from scientific thought, on the one hand through personal and social relations, on the other hand perhaps because I was now turning to an engineering school. I have had some (preference) for engineering work; I was there of course to do scientific work, but at least to do it in this atmosphere. So I turned to a different thing and was somewhat impatient sometimes in listening to Ehrenfest. I was not quite inclined and also had not the possibility of sensing what he was driving at. So I lost things. And so to my regret I cannot give you any idea of what happened in those years until l925 when De Broglie and then Schrödinger cane forward. You see, it was necessary for me to turn away from physics. I would never have come somewhat into hydrodynamics and got to like the subject, to love it, if I had not cut myself from physics. You cannot serve two sciences at the same time. And over that, I felt myself sufficiently connected with hydrodynamics that I could come back later on. But then it was more in the nature of an amateur. And here I have lost something. And I hope that you still can replace something with other people.

## Kuhn:

I would add, just from this particular point of view, the very fact that you did turn away in 1918 makes your testimony about the years before 1918 rather more valuable than that of some of the people who did not. When you speak of feeling forced to move away from Ehrenfest's constant analysis in depth, you do mean this also with respect to physical problems?

## Burgers:

No, the other problems were the main mover here. The consequence was that it broke off, a little bit, the contact also in other things, since I was at the same time studying quite a different subject, and since Ehrenfest did not desire to give much attention to non-linear systems. Hydrodynamics is a typical case where you have at least to face non-linear systems of equations. So it was a little bit of a divergence in both those -–

**Kuhn:**

Did you have at all the feeling on occasions, as Ehrenfest would keep probing deeper into physical problems, that some of this just doesn't need to be done. 'Let's solve the equations instead.'

**Burgers:**

Well, if you ask whether I had that feeling, ja, I may say yes. But that doesn't mean very much. I mean that was simply because I was a little bit impatient, and I was a little bit more directed toward practical results.

**Kuhn:**

You mention a great many problems that one confronts in this period. Did some of them seem more obviously severe than others, more likely to wreck things? Others there seems to be nothing to be done about? Can you separate problems of that period in that way?

**Burgers:**

I know of course that there was no feeling about attacking the Zeeman problem, the irregular Zeeman splitting of spectral lines. The theory gave only these regular spacings, the normal spacing. And Larmor's theory will get the same thing. There was no idea of what could be the reason why it did not work, why atoms were different. One of the problems which always was before Ehrenfest — I do not know whether it was reported or not — is the question of the transition from the oscillator to the routing system — the pendulum which turns over — and the chancing quantum rule you get in such a case. It made the impression there was some deep mystery in it which perhaps might explain things, but I did not get any hold on it. Ehrenfest was turning around with it, but it remained as far as I know at that time an open problem.

**Kuhn:**

Do you think of other problems of that sort? You mention that one, and also the problem of the n factorial. I wondered whether there were more problems that you remember of that sort, that vexed him deeply?

**Burgers:**

I do not think so. I was involved in this problem of the n factorial in a strange way. There had appeared a paper by Scherrer, I think — ja. He had given a calculation of the entropy constant and had introduced some (deviation) by n factorial. And Keesom had made the observation that if you took Scherrer's model in the literal sense, then you could deduce from it that a gas would remain ideal down to temperatures so close to the absolute zero that there would be no sense to assume that. Ehrenfest told me about Keesom's observation. For some reason Keesom did not wish, I think, to publish anything about it. And Ehrenfest proposed that I should give a calculation. But of course it did not bring any solution to the problem which was there, and I was only vaguely aware that there was this problem. Then later on Ehrenfest in a series of lectures told us about Tetrode's calculation of the entropy constant. I do not remember how Tetrode has introduced it. If I am right, now the real solution has come through the recognition that particles are interchangeable, and that the whole idea of identical separate particles is not really applicable in these cases…

## Kuhn:

On this general subject, what is the state of Nernst's, heat theorem in Leiden in this period?

## Burgers:

I cannot reconstruct that. I mean that is too much mixed up with what I learned later about it. I really cannot distinguish at the moment what I have read later on these processes by which you calculate the gamma constant by cyclic processes.

## Kuhn:

I was fascinated to notice, in looking at the thesis, there are a couple of places where you use the phrase "quantenmechanika," put in quotation marks. Where did that come from? The pages are 237 and 238.

## Burgers:

Ja ja, I see it, I see it. But I still would have believed that the word was in the air, somewhat in the atmosphere. It is too typical a word which Ehrenfest would have used so that would have come up from conversations that along with the classical mechanics there should be some other mechanics. I believe that that will have cone up in that time simply in the atmosphere and probably from Ehrenfest.

## Kuhn:

You think it's quite clear that by this time Ehrenfest was 1ookin for a now sort of mechanics?

## Burgers:

Ja ja. Well, everybody was. Nobody of course in the sense of Schrödinger mechanics, but since Bohr had shown that quantization of these orbits was of importance and since this fitted in with Planck's idea that there should be a new set (of masses) applicable for atomic systems, it was a new kind of mechanics that everybody was groping for. I think that was in the air. But we did not know anything more than that you had to introduce these quantum conditions and that you had classical adiabatic invariance along with them.

## Kuhn:

Would you say that what was being sought for was simply some way of fixing up the old mechanics so that it would be a quantized mechanics rather than a quantum mechanics.

## Burgers:

I think that you are right in that. At any rate in my mind it was not much more than this. And if I jump over to the later idea that variables have non-commutative rules of multiplication, it is something so different that I cannot imagine that in all the things which were in Ehrenfest's mind in 1918 there was anything like that… Maybe as a vague idea sometime, but certainly not with the feeling, "I could do something with it."

## Kuhn:

But you think he may have been prepared for something in which the basic statements would have been very different from Newton's laws or Hamilton's principle?

## Burgers:

Ja, ja. Well was there not also in that time already the problem of the energy and the mass of the electron where you had to assume that some other force than electricity would keep the things together. And if you worked this into an equation, you got results which you just could not use. So the feeling was that as soon as you come down to dimensions of atomic nuclei and electrons, nature may no longer fall into rules which you know from ordinary systems. That was certainly in the air, and that was certainly also connected with Einstein's work. I mean that Ehrenfest must have felt it and have told us more than once that at least the dimensions may be quite different. I think Ehrenfest once also told us that it might perhaps be that when you are looking at the atom you see these systems which look like planetary systems, but actually you are looking at a universe, say through a whole — with ideas and experience which usually (refer) to the large-scale universe. I mean, he played around with such ideas… Ehrenfest said, "Might we see really stellar systems." They are not quite in the atom, but when you still probe further —. As if the interior would suddenly be the exterior of the universe. He had certainly such ideas and told us about them, or I mean mentioned them in conversations. That certainly, but I have no idea that he at any time came up to a point which would give a thing to handle. I've got no idea how Born or how Heisenberg has come upon the system… Ehrenfest might have sometime had the idea. I do not know that a new type of multiplication or a new type of mathematics might be able to prove it, but as far as I can retrace, not in such a way that he could set it down on paper, "Now let me try, today I write two equations, tomorrow I add some to it, and something may come out of it."

## Kuhn:

He did feel though, I take it, that adiabatic invariance was likely to be a fundamental clue? Had that been true from the beginning?

## Burgers:

Ja, ja. That is my impression… I would have the impression that it was constantly growing in him. I mean that in 1911 He may have had sonic idea. In 1913 he certainly was closer to it. In 1915 he got closer. That I would think is so, but I cannot say how far developed it was at an earlier stage…

## Kuhn:

Was the feeling about degenerate systems, that this was also fundamental, or was this a mathematical problem to be solved, to simply find, some way of getting out of the trap that one was apparently in with degenerate systems?

## Burgers:

Again I do not know. In my mind not much more was in it than the mathematical point, but it is quite well possible that Ehrenfest had a feeling that there was something deeper in it. And at any rate; we were of course struck by the fact that when you have oscillations in two degrees of freedom, then as soon as it becomes degenerate and the two periods are equal, you can also describe it by polar coordinates and put the same computation in a different form. That there was something more behind it than just the mathematical transformation from one system to another, I think was in Ehrenfest's mind and somewhat in the air.

## Kuhn:

You spoke a little while ago of the abnormal Zeeman effect. Now the normal Zeeman effect papers came out in 1916, the ones that predicted simply the Larmor displacement but on the quantum theory. And they do this in terms of space quantization. But the notion of space quantization in this form does seem to violate the whole idea of adiabatic invariance. Do you remember discussions of this?

**Burgers:**

Again I do not know.

**Kuhn:**

I wanted to ask whether you might have been involved in discussions of these and similar matters with Bohr when he came to Leiden in 1919? I know you were already in Delft then, but…

**Burgers:**

No, I have not been involved in discussions with Bohr. I have seen Bohr only once or twice. But let me see. I have certainly been at the occasion of Kramers' promotion, when he got a doctor's degree, and then Bohr was there. And I believe that Bohr also came to the Dutch Scientific Congress, the (Dutch name), the physical and mathematical congress in April of that year. It must have been in 1919. I believe that Bohr had been invited for that. And probably gave a paper there. And I have been introduced to him. I have been at the dinner which Kamerlingh-Onnes gave in honor of Bohr, but I have not been talking much with him then.

**Kuhn:**

I wanted to ask you about Kamerlingh-Onnes. He gave a dinner for Bohr. Was he himself a great admirer of Bohr's, or was this simply the formal, nice thing to do?

**Burgers:**

Both, I think. Onnes had been extremely interested always in quantum theory and in the meaning it could have for his experimental work. I have never talked much with Kamerlingh-Onnes about it, but I think he would talk about a molecule or atom which 'took its coats off,' in order to explain something about quantum mechanics or the quantum theory.

**Kuhn:**

Was he a deep conceptual thinker with respect to his own area?

**Burgers:**

That is very difficult for me to reconstruct. You know, all the work which he did to reach these cold temperatures was based on a deep belief in the law of corresponding states — from the Van der Waals law of corresponding states. He started by having the cycle with (ethylene) or some other substance and making liquid air. Then many measurements were made about the isotherms of the noble gases and of helium. All his attention was directed to putting in such a pattern all the isotherms of all the known cases so that you could predict what approximately the critical state would be, and what (conditions) you had to give to the cryostat to make them liquid. All his practical work was governed by the fundamental conviction that the law of corresponding states would help to determine your work. And so that he got finally liquid helium was for him a triumph of this idea, and so from this thinking I would believe that he had strong conceptual feelings about applying certain ideas in physics also to practical ideas. He was at any rate deeply impressed by the idea of quantum states and by the importance of this for molecular interactions and atomic interactions.

## Kuhn:

What sort of person was he? I realize I know almost nothing about him

## Burgers:

Well, he came from one of the northern provinces of Holland and had a certain roundness and also a certain abruptness. He was somewhat of a dictator and arranged everything as he wished to have it. That gave him his perseverance to see things through. It was not easy. He came into Leiden as professor for experimental physics about the same time as Lorentz came for theoretical physics. His predecessor, Rijke, had not had such higher ambitions than to keep the collection of instruments in good order. He had done some work on the capacity of Leiden jars comparing capacities and so on. Not doing very deep work. And then Onnes came in with this idea that "Well, low temperatures that must be the clue for new physics. If I can get there, then I shall have opened the domain where now everybody can try out things which are known in high temperatures and see whether they are still working the same or whether something different happens" That is the driving force of Kamerlingh-Onnes — "I must reach that, in view of all the new things which can come out of it." And so he started to work, and he had of course to build up all the machinery for that. And he was happy enough that he found a young man, almost a boy, (???) (Flynn) who became his chief mechanic and whom he educated and who was a very good instrument maker. He helped Onnes in the design of everything and was really in his heart an engineer. (???) (Flynn) has once told me, "Well, if the government would inspect all our books, then, they would put both of us in prison." So many things had to be cheap — to get the money for the instruments. This meant of course that the laboratory looked just like a factory, with all these bunches of pipes and various geologic generations of things not quite broken down and new things added to it. It was very impressive for a young student as I was to come in. And there were all sorts of stories about Kamerlingh-Onnes. Of course it was absolutely forbidden to smoke in there. And I think that in 1913, R. W. Wood, from Johns Hopkins, came visiting. He lit his pipe in the hydrogen room. Onnes almost pulled it out of his mouth and threw it away. And Kamerlingh-Onnes was also rather forgetful. He worked in the afternoon, and then this (Flynn) sat until late in the evening. And Mrs.

Onnes from time to time sent — not a taxi-cab — a horse drawn cab to wait for him. And the cab was waiting before the laboratory. Onnes quite forgot and walked home without observing it. He had a (???) in his hand and when he got hone he had only still the top of the (???). He had struck it so often against the garden walls so that it had broken down. And also Lorentz said he pushed all his courses upon Lorentz. He was not quite in good health and exploited this idea, and Lorentz was goodness in person. And Lorentz understood everything. Lorentz gave an introductory course into physics for medical people and since he observed that the younger boys were not quite acquainted with mathematics, he wrote even a little handbook on differential and integral calculus. But actually, I think it was according to the law, the work of the professor in experimental physics to give this course, and it is only much later that Kuenen was appointed to relieve Lorentz from this work and to take over these courses. But Onnes was such an autocrat. He never would permit Kuenen to do his own experimental work, on work of his own. It should always be in the program which Kamerlingh-Onnes had made for himself. His rule for his assistants was that you should work all during the day in the laboratory, and in the evening you should write up your data. You had a little pocketbook, and you should write them neatly into a report in the evening. On the weekends you should make your weekly reports. And you could get free — I know it from when (Bouta) came — you could get a day off for love; I mean when you were engaged. But you could not get a day off really to listen to a theoretical lecture because experimental physics requires the whole man. But he had a nobility in his heart, and also I have disappointed him by not staying with him and by going back to Ehrenfest. He still had been very kind to me.

## Kuhn:

I just wanted to ask how Kamerlingh-Onnes got along with Ehrenfest. They must have been such extremely different people.

## Burgers:

Ja. Well Ehrenfest was of course extremely respectful in every point, and this is the main point. Well, Ehrenfest felt of course some of the strangeness of Kamerlingh-Onnes and found a way of dealing with it. And Onnes certainly had a great respect for Ehrenfest — that is quite true — but they were not intimate. Lorentz was much more reserved, much more reserved than Kamerlingh-Onnes. Kamerlingh-Onnes pounced upon you. That gave a certain attraction in the long run. Lorentz was so modest in his style that you felt always the distance. I have done — many bad things, and one of them was in Kamerlingh-Onnes' laboratory. The main work which I did was just reading the galvanometers for temperature measurements. It was often that one of the older assistants or research associates had to do a series of work, and he determined when one point was fixed and (when one had to move to the next). During the coffee hour on one occasion he had gone away, and I had finished the measurement for that point, and so I thought it would be good to proceed to another point. So I told one of the (new boys they should exhaust somewhat further and go to a higher vacuum). Suddenly it occurred to me that I didn't quite know what would happen, so I told him to stop. But what has happened is that (ethylene had got into a lot of dirt that should not be and has been) blown into the air and lost. And this was cleaned up by (Flynn) and his people without ever telling Onnes. And I was so much afraid, I only told Ehrenfest. And I have also told Dr. (Kooming) who was the chief physicist, but it has been kept away from Onnes. So apparently people stepped in to protect me at that time.

## Kuhn:

What about Kramers?

## Burgers:

Ja. Let me see. I believe that Kramers and Struik had come in 1912. And they did Candidate exam, an exam which you do after two or three years, in 1914 or the beginning of '14. And Coster had come in 1913, and I came in 1914. But I had then some experience, some mathematics and physics already from home and brought it with me. And Ehrenfest pushed me through so that I was not much behind them. I know that Kramers already had quite a set of ideas of his own, but I still have the feeling that his work in physics came really to fruition during his stay with Bohr; not so much during his Leiden period. And afterwards of course he was much further than I, and he has a mind which is much more imaginative. Of course he was the great physicist. What I have in mind is that we got most of our ideas first from Ehrenfest, but that Kramers was an independent reader in many respects. He was perhaps even somewhat less attached to Ehrenfest than I was, and by reading other things formed more of his own ideas. For some reason Kramers had had Hamilton's Quaternions. I mean that was not a thing which Ehrenfest would introduce. And Kramers had other interests… I do not remember anything, but I would believe that Kramers had read perhaps also old work by (Franz Neumann) and had of course had Maxwell's Electricity and Magnetism and had ideas about these matters. So not quite all came from Ehrenfest but had grown up in his own mind. That is as far as I can reconstruct it.

## Kuhn:

When Kramers left Leiden to go to Copenhagen, … was that sort of a deliberate breaking away from Ehrenfest?

## Burgers:

Maybe. I do not know. It surprised me somewhat… Perhaps he had just put in his mind, "Well, it would be nice to see somebody else." That would be my most probably construction, that it was his own idea — going there. It was of course a difficult (???), since it was in war time, and traveling in war was not quite a simple matter.

**Kuhn:**

What was Kramers himself like?

**Burgers:**

A very pleasant friend and comrade, very kind and observant, also having a sense of humor and I would say a little bit — very slight, but also no consciousness of his own capabilities. A good musician — extremely good. He matched Ehrenfest in that respect. I would not even say who was — perhaps Kramers. He came from a family of people who were very musical.

**Kuhn:**

Did they play together?

**Burgers:**

Oh ja, ja ja. Kramers played especially (viola) cello, but also piano very well. He could play with Ehrenfest, he on the (viola) cello and Ehrenfest on the piano. They had discussions about it. Kramers was very acquainted with music, and Ehrenfest was also acquainted, with music. I said I have such a feeling that there were often disputes between them. Kramers was more independent and also had more support in himself in these matters and liked to be controversial, to be in opposition.

**Kuhn:**

Were their personal relations nevertheless fairly close, or did these disputes drive them apart?

**Burgers:**

Oh no, no no, not as far as I can think. These were amiable.

**Kuhn:**

Do you have any recollections from your own discussions with Kramers, as to the problems which in this period were most on his mind?

**Burgers:**

No I don't… I cannot quite reconstruct what we talked about. We have seen each other a lot of times and walked together and were very often together, visited each other, but here my recollection is blank, to my regret.

### Kuhn:

I meant to ask you one other question about the thesis. How many copies of the thesis do you suppose were printed, and how widely was it circulated?

### Burgers:

It was published as an issue of Uit Archive du Musée Teyler, and so I suppose that 400 copies have been printed in that way and have been distributed to libraries as (???). And I think I had perhaps also a few hundred copies and sent them out to many people.

### Kuhn:

When it was printed in the Teyler archives, was it printed in this final form? Because there are a lot of things in square brackets in this, which I take it were added after you submitted it as the essay.

### Burgers:

Ja ja. These were added when I made it ready for printing… I'm pretty sure that what appeared in printing was only this one thing, and that was then published as part of Uit Archive du Musée Teyler.

### Kuhn:

I would like to ask you about two of Ehrenfest's students of a couple of years later. One is this Trkal, who worked on the n factorial problem…

### Burgers:

Trkal. You must put the accent on the first syllable; that I know is right. I have seen him often. Did he have to go back to Prague afterwards?

### Kuhn:

He was with Ehrenfest just for the time of his thesis then, a few years or so?

### Burgers:

Something of that order, ja, ja.

**Kuhn:**

The other one perhaps you would know more of, and that's a Russian, (Krukoff). He also worked on the question of adiabatic invariance.

**Burgers:**

Ja, I have seen him a few times but not very much. I have not seen him often.

**Kuhn:**

He was in Leiden after the war was over I presume?

**Burgers:**

Ja, ja. Certainly not during the war. I cannot either remember in which year Joffe came first to Leiden. Joffe was certainly in Holland in 1924, but I believe, he must have been already in 1922 or so, visiting Ehrenfest. And Joffe was in this country giving lectures on the physics of crystals or something like that, which has been published in this country. And then in 1924 he came for the first Congress for Applied Mechanics we had organized in Delft. He, with Galerkin and Kryloff. And then in l925 Joffe was again there. Friedman had died then, and then Joffe asked me whether I would visit him (in ???). But I know that Ehrenfest was a great friend of (Krukoff) and there were very close connections with them and that they were working for a 1on time, but I do not know the details.

**Kuhn:**

I believe it was in l924 or '25 that Ehrenfest was offered a position in Moscow. Am I correct on that?

**Burgers:**

I do not know the details, but it may be. And Ehrenfest in that time has certainly been in Russia. And Mrs. Ehrenfest had been there several times. How far did it go, I do not know. Ehrenfest was much befriended with Joffe and liked Russian students very much. But he was hesitant as to the Bolshevik revolution. It is difficult to talk about these things, but in 1917, when the first revolution came, when the Tzar was thrown off, then he told me once that he had moved Lorentz and Kamerlingh-Onnes to send a telegram to Joffe to congratulate him, that this despotism had now fallen down. But then afterwards things moved further and further to the left. Then he became rather afraid. And of course students at that time were more apt to be leftist, you will know it from (Africa). I hope that you will not take it against me in any way. [Jokingly] But this also contributed a little bit to a shift of opinion. Students then were apt to move over on the left. And then Ehrenfest became somewhat scared about developments. So he has never been in admiration for the state of things in the government of Russia, but he was keenly aware of the difference of nature. I mean when he came in Holland it has been such a difficult thing for him that Dutch people speak so little, that they are so close in themselves. The Russian students always talk; they talk you down and talk you until midnight and over midnight. In Holland people do not say anything. In his colloquium when he started it was quite a new thing to people. It has happened, I believe, on the first evening — I mean that was before I came to Leiden — but it seems that Mrs. Ehrenfest and Ehrenfest had gotten into a dispute. Mrs. Ehrenfest had told her husband that, "When you start anything it's completely spoiled" — oh, it may have been the reverse. (???) Then other people had taken part in it, and so gradually he got it going. But he was still bothered about the attitude of Dutch people. So to talk with students, that was really something in his nature, and with Russian students, who attracted him extremely deeply. But he had rather a difficult — the younger son was found not to be mentally complete, he was mentally deficient. Extra money was necessary for this. And they had lost their money already earlier. I think they had money partly in Russian funds and certainly in Austrian funds, and when the war ended, these were valueless. The big house which they had for themselves in Leiden was a big charge on them, and it was difficult to keep up with it. And all this has made life difficult for Ehrenfest. He was keenly aware of this. I have met Mrs. Ehrenfest in Leningrad when she was there. I do not remember whether she was there simultaneously with Ehrenfest. That may not have been the case.

## Kuhn:

Ehrenfest's older daughter, Tatiana, was a student in Moscow at one time… I know she was Max Born's assistant in the late 20's. I think it was either just before or just after that, I'm not sure which.

## Burgers:

These things must have escaped me. This came of course you see because I had to give my attention to quite different things in Delft. In 1921 I got an assistant in the laboratory, and so I could do some experimental work. I was interested in (turbulence), and Ehrenfest was not interested at all in (turbulence). I could never got through to Ehrenfest to talk about the statistical mechanics of (turbulence, and this was somewhat of a difficulty. Then in 1921 my first wife became seriously ill, this also has kept me somewhat out of contact. I wanted to, I had to, give so much attention to my own subject. And then in 1921 I became acquainted with von Kármán, and that, was of course a new inducement to me. So I dropped out of the Leiden atmosphere, and I did not quite follow what Ehrenfest and his wife and his children were doing at that time.

**Kuhn:**

Did you stay in contact with him though, until the end?

**Burgers:**

From time to time, yes, of course. Yes. Yes. But not quite regularly. But I saw him at meetings, visited him at home; we had still discussions about various things. Then in 1931 I was elected a member of the Royal Academy of Sciences, so we saw each other there usually every month. But there were also a few times, matters about people, with whom we had different contacts. Ehrenfest's and my opinions sometimes clashed. So I thought well — I go my own way. It is difficult to speak about these things. For instance: When I came in Delft, the man who had asked me to come there — one of the two people was Biezeno — who was professor, is now Emeritus, of applied mechanics, theory of elasticity, strength of construction, and a very good mathematician. I came into close cooperation with him. Very important form of cooperation. That is both scientifically and also because we were both members of the department of mechanical engineering. The rest of our colleagues were engineers, so we represented the scientific point and were driven more closer together in a kind of opposition to many of our colleagues. There were also internal difficulties… You can well imagine that. So I had much to do with Biezeno. I had been much befriended with him. But Biezeno and Ehrenfest could not quite go together, and in certain points — questions of music sometimes — they had (violent) discussions. I had the feeling that Biezeno was somewhat more upright, I would say in some matters, where Ehrenfest first made some point and deviated out of it. At any rate, all the natural things, my work, my position, my reputation, as a member of the faculty there, brought me to Biezeno, and we had many things in common. So this was another reason that I could not always accept the points of view of Ehrenfest. Also there were questions about people where we sometimes had to make a judgment, and I really believed, and still believe, that Biezeno was right and Ehrenfest was not quite right. All these things made that we drifted apart somewhat more. These are all personal matters, but they have taken part in —… You see, there were sometimes questions about appointments and recommendation of people where I was not involved in it directly. Gradually I found out there were some people making a certain line and pushing forward somebody and that Ehrenfest was also involved. Biezeno and I had scientific criticism about that person. And especially… we were somewhat self-conscious that we knew this. Well, we stood farther out of it, but somebody asked our opinion about it, and we wanted to be independent about it and to be able to say, "Well I think this and this about that and that piece of work." And such things happened. And then you suddenly feel there you must keep to your own side in this.

**Kuhn:**

Would Ehrenfest take these things personally?

## Burgers:

Well, there has been one case where he was really much moved and somewhat disappointed by it. There was one question with a person who is still alive, and that is why I am also reluctant to speak about it. I had written to that man and criticized his work rather severely, and perhaps under the influence of Biezeno I may have used somewhat strong words. But the man himself, when I met him a few weeks after, said, "I thank you for your criticism." Maybe that is a move on his side, so I thought that matter is so far closed. I mean, I remained on friendly terms with him, and I certainly had no opinion of him otherwise. And then Ehrenfest was hurt through my moves in these matters, and I could not quite follow what happened there. So I was often deeply sorry for him. But there were also points — you see I was not yet formed, I was still growing, isn't it? And one had to form one's own points of view, and that means that you cannot quite accept everything else or listen even, listen to everything. Because you are still uncertain in yourself, and you say, "But there is somebody whom I rather trust in this point of view, and I have to do much as he does, I keep with him." So you are sometimes torn between things. But when 1933 came, Ehrenfest once told me I do not know which month it was — he said, "Well, I believe the only thing which now should occur is that important Jewish people everywhere should take their own lives as a protest to what Hitler is doing, (but the world does not react to it.)" But in his own act this was one item. There were more items. You have of course read his letters. And you have talked with Mrs. Ehrenfest about it.

## Kuhn:

Mrs. Ehrenfest I must say has never discussed this subject with me at all. Even with (Galligher) Ehrenfest, with whom I became fairly friendly, it is very difficult for her to say anything about it, which I can understand of course. [The concluding section of the conversation has been erased from the tape at the request of Martin Klein. It dealt with his knowledge of certain of the circumstances of Ehrenfest's suicide. He will prepare a memorandum on his knowledge of this subject and will deposit it for later use. He feels, and I agree, that it should not be a subject for general scrutiny at this time.]

## Kuhn:

Notes after end of recorded discussion After the recorded part of the discussion T. S. Kuhn joined Dr. and Mrs. Burgers at dinner. The following comments are based on notes taken at that time. Asked about the circulation of his thesis, Burgers indicated that it had appeared initially in Teyler's Archives which circulated perhaps 400 copies and that he had had perhaps another 400 copies printed and. bound for himself. He thought that it had been quite widely circulated but that few people outside Holland had paid very much attention since they rarely read Dutch. On the other band, he indicated that a copy had apparently been sent to (Litvinoff) who was in Copenhagen at the end of the war, that Litvinoff had taken this back to Russia with him, and that Joffe had later reported that the thesis was the first document to reach Russia which dealt with post-Bohr developments of the Bohr atom. Burgers also indicated that Ehrenfest had felt that Planck's first version of the quantum theory was better than his second. On the other hand, Ehrenfest did feel that there was some good experimental evidence for a zero-point energy and was therefore far from ready to dismiss Planck II out of hand. Burgers indicates that Kamerlingh-Onnes was also very much concerned with the problem of the zero-point energy. Further illumination about Kamerlingh-Onnes' personality is gained from the following episode reported by Burgers. Onnes was sufficiently a dictator in his laboratory that he did not want his main collaborator-assistant Keesom to do experiments in the laboratory on crystals. As a result, Keesom had to wait for this important piece of his work until he had his own professorship. [The following is taken from Professor Burgers' letter to Thomas S. Kuhn, June 14, 1962: (1) In the first place I saw that E. T. Whittaker's book. "Analytical Dynamics" came out in 1917 as a second edition. There has been an earlier edition of 1904. I also found that in my third paper on "Adiabatic Invariants of Mechanical Systems" (which appeared in 1916 or in the beginning of 1917) I referred to the first edition of Whittaker. Thus the book must have been known to us already in 1916. I do not remember what directed attention to it; Ehrenfest at that time was in correspondence with Epstein, who then still was in München; it is possible that references to Whittaker nay have occurred, in that correspondence. Also in that year Dr. Gunnar Nordström was living in Leiden, and I remember that when the new edition of "Analytical Dynamics" had come out, I ordered copies for myself, for Nordström and for the "Leeskamer"] In my first paper "Note on the Model of the Hydrogen Mo1ecule of Bohr and Debye," which was written in 1916, I did not quote Whittaker, but I made use of a principle concerning the treatment of non-holonomic variables, which I believe I had found in Whittaker's book. (2) In the second place I believe I made a mistake in a name. You asked me whether Ehrenfest may have had the feeling that it became impossible for him to find a way into various new developments in quantum theory. In reply I mentioned a conversation of Ehrenfest with Wiersma and me, in Wiersma's room, who then lived opposite to Ehrenfest's house. I believe I gave the name as that of Rutgers, but on thinking it over I am pretty sure that it was Wiersma (who died about 1944), and not Rutgers. It must have been about 1928, I think."]