

JAN BURGERS



1895 - 1981

Edited by Jan V Sengers and Gijs Ooms

Published by J.M. Burgerscentrum Research School for Fluid Mechanics on the occasion of its 25th anniversary

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Research School for Fluid Mechanics

FOREWORD I

by GertJan van Heijst, Scientific Director J.M. Burgerscentrum, Tthe Netherlands, Mekelweg 2, 2628 CD Delft

This little book about the life and work of J.M. Burgers is issued at the occasion of the 25th anniversary of the J.M. Burgerscentrum (JMBC). To be more precise: at the occasion of the 25th anniversary of the first KNAW recognition that the JMBC received (in 1992).

The Dutch research school for fluid mechanics is named after Prof. Johannes (Jan) Martinus Burgers, who was the first professor in fluid mechanics appointed in the Netherlands. He was appointed professor in Delft at the age of 23, actually two months before he received his PhD degree in Physical and Mathematical Sciences from the University of Leiden, where he worked under supervision of Prof. Paul Ehrenfest.

During his career, Jan Burgers played an active role, both as an outstanding scientist and as a stimulating driving force behind the international organization of science. Just to illustrate the latter, he was one of the founders of the International Union of Theoretical and Applied Mechanics. In fluid dynamics, Jan Burgers was interested in fundamental issues, but also in applied problems. He was internationally highly recognized for his scientific accomplishments, and his name is now directly connected with the Burgers equation, the Burgers vortex, and the Burgers vector, to mention a few. In collaboration with his brother Willy, he worked on rheological problems, and they discovered the so-called screw dislocation in crystalline substances.

Jan Burgers kept well-structured and very precise records of all his activities, and his archive of handwritten and typed notes is still present in Delft. Going through these notes one obtains a very clear picture of his well-structured way of working, including his lecturing.

In 1955, at the age of 60, Burgers moved to the University of Maryland (USA), where he got appointed as research professor in fluid dynamics, and where he started his second career. In Maryland he remained active for many years.

In the early 1990s, the fluid dynamics community in The Netherlands became organized nationally, first in the form of a Delft-based collaboration network, soon followed by a nation-wide organization in the form of a 'research school' on fluid dynamics, in which most academic groups working in this area were participating. Apart from providing a network for the various academic groups involved, the research school also estab-lished close links with many industries and technological institutes.

It seemed appropriate to name the school after Prof. Jan Burgers, the scientific pioneer in the field, the first professor in fluid dynamics appointed in the Netherlands: the research school was named "J.M. Burgerscentrum".

In the early years, the JMBC had a modest size, of about 70 PhD students and approximately 14 academic research groups. The first scientific director was one of the founders of the school, Prof. Charles Hoogendoorn. In 1996 he was succeeded by Prof. Gijs Ooms, and under his stimulating leadership the JMBC continued to flourish. After many years of very successful leadership, I took over the position of Gijs Ooms in 2014.

In the meantime, the activities and the size of the JMBC had increased steadily: in 2017 the number of PhD students and postdocs participating in the JMBC had grown to a fivefold of the initial number, based in one of the 50 university research groups that constitute the research school.

In parallel to the J.M. Burgerscentrum in The Netherlands, the Burgers Program Maryland was founded in 2004 at the University of Maryland (USA), where Jan Burgers worked during his second career. The mission of the Program is to enhance the quality and international visibility of fluid dynamics research and educational programs at the University of Maryland, in partnership with the Burgerscentrum. The board of the Program has been chaired originally by Prof. Jan Sengers, and since 2006 by Prof. Jim Wallace, while Prof. Sengers has continued to serve on the Board as liaison with the JMBC. There have been numerous faculty and student exchanges between the JMBC and the Burgers Program.

This book is edited by Profs Jan Sengers (emeritus University of Maryland) and Gijs Ooms (emeritus Delft University of Technology). The book contains a number of papers about Burgers' work in Delft and in Maryland, some being autobiographical notes by Jan Burgers himself, and some being written by the editors. Apart from describing the scientific aspects of Burgers' work, this book also provides important information about the person Jan Burgers himself, and about his political vision. This all adds to our admiration of this remarkable fluid dynamicist.



FOREWORD II

by Jim Wallace, Director Burgers Program for Fluid Dynamics University of Maryland (USA), College Park, MD, 20742

Fluid dynamics has a long history at the University of Maryland. Much of the most notable earlier research occurred in the Institute for Fluid Dynamics and Applied Mathematics (IFDAM), which was merged with the Institute for Molecular Physics to form the Institute for Physical Science and Technology (IPST) in 1976, but it was also dispersed throughout the College of Engineering. During the early Nineteen-fifties internationally prominent fluid dynamicists, such as Sydney Goldstein, J. Kampé de Féret, George Batchelor and Jan Burgers lectured at IFDAM. In 1955 Burgers began a second career at IFDAM, after retiring as a professor from the Technical University of Delft, and he continued to be very productive until his death in 1981.

In January of 1995 I represented the University of Maryland at the centenary celebration of Burgers' birth held in Delft by the J. M. Burgerscentrum. In May of that year I also organized a half-day Burgers centenary celebration symposium on the Maryland campus that culminated with a lecture by Frans Nieuwstadt on "The Legacy of J.M. Burgers."

When Katepalli Sreenivasan became director of IPST in 2002, he raised funds to endow an annual Burgers Lectureship and, together with Jan Sengers of IPST, he put together a Burgers Board to organize this Lectureship. Frans Nieuwstadt spent several weeks at the University of Maryland in the Fall of 2003, and he gave the first Burgers Lecture entitled "Resolving Reynolds' Riddle."

When Sreenivasan left, Sengers became Chair of the Burgers Board and brought great vigor to the task. He brought me onto the Board, and together we and the Board conceptualized a broad range of activities and participants constituting the Burgers Program for Fluid Dynamics (http://www.burgers.umd.edu). The Program describes itself as: "Inspired by the intellectual heritage of J.M. Burgers, the mission of the Burgers Program is to enhance the quality and international visibility of fluid dynamics research and educational programs at the University of Maryland with the help of an endowed Burgers Fund. Fluid dynamics in this context is viewed to include a broad range of dynamics, from nanoscales to geophysical scales, in simple and complex fluids." Almost 80 faculty members, spread over 22 different academic and research units in the College of Computer, Mathematical and Natural Sciences and the A. James Clark School of Engineering, participate in the Burgers Program.

The Burgers Program was inaugurated in November 2004 with the first of its annual Burgers Symposia. Highlights of the day-long event were the remarks by Gijs Ooms, the Scientific Director of JMBC: "The Legacy of J.M. Burgers in the Netherlands," remarks by Jan Sengers on "The Legacy of J.M. Burgers at Maryland" and the inaugural Burgers Lecture.

The November 2014 Symposium celebrated the 10th anniversary of the Program with the Burgers Lecture given by Katepalli Sreenivasan, who had initiated this Lectureship, and additional lectures given by Gijs Ooms, Charles Meneveau, and Bruno Eckhardt, each of whom were past Burgers Lecturers, as well as by Rachel Lee, a graduate student who had been supported by the program to attend a short course at the JMBC, and Ken Kiger who had taken a sabbatical at the JMBC.

In addition to our annual symposium, the activities of the Burgers Program include:

- a visiting professor program
- a faculty and student exchange relationship with the J. M. Burgerscentrum
- publications partially resulting from this exchange program
- short-courses put on by the Burgers Program in collaboration with the J. M. Burgerscentrum
- an annual spring showcase Ph.D. Student/Postdoc symposium in collaboration with Johns Hopkins and George Washington Universities
- summer research schools on fluid dynamics
- the Fluid Dynamics Reviews seminar series.

I have no doubt that Jan Burgers would be enormously pleased to know that his legacy at the University of Maryland is flourishing and that we have such close ties to the JMBC. I hope this book about his life and work will inspire new generations of students to be attracted to the fascinating subject of fluid dynamics.

JAN BURGERS (1895 - 1981)

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INTRODUCTION Jan V Sengers, Gijs Ooms

Johannes (Jan) M. Burgers was born in Arnhem in The Netherlands on January 13, 1895. His potential as an outstanding scholar was recognized early and he started to work as a Professor of "Aerodynamics, hydrodynamics and their applications" in the Department of Mechanical Engineering, Shipbuilding Engineering, and Electrical Engineering of the Technical University in Delft in 1918, two months before he received his PhD in Physical and Mathematical Sciences from the University of Leiden under the supervision of Paul Ehrenfest. After an impressive career in fluid mechanics in The Netherlands, Jan Burgers moved in 1955 to the University of Maryland in College Park, MD, where he remained professionally active, first as a Research Professor and subsequently for another decade as a Research Professor Emeritus. A considerable amount of information about Jan Burgers and his work can be found in the well-known volume edited by F.T.M. Nieuwstadt and J.A. Steketee [1]. A curriculum vitae of Burgers is presented in Appendix I. Additional information about the activities of Burgers in Delft (in Dutch) can be found in [2, 3].

At the Delft University of Technology Burgers established a research program with an international reputation. Among the multiple accomplishments we mention the development, together with his assistant van der Hegge Zijnen, of the hot-wire technique for measurements in turbulent flows. In particular they measured the velocity profile in the boundary layer and obtained data for the boundary layer thickness and

the velocity gradient close to the wall. Burgers also studied the resistance of bodies moving in a fluid, taking turbulence into account. He applied Oseen's theory to boundary flow. He developed the theoretical model of the "Burgers vortex". This is an exact solution of the Navier-Stokes equation. The Burgers vortex describes a stationary self-similar flow. An inward radial flow tends to concentrate vorticity around the symmetry axis. At the same time, viscous diffusion tends to spread the vorticity. The stationary Burgers vortex appears when the two effects balance.

Burgers realized that the study of turbulence based on the Navier-Stokes equation was very difficult. Therefore, he decided to restrict his attention to model problems with which the essential aspects of turbulence could be treated. As an example, he used (the now famous) one-dimensional Burgers equation and made extensive investigations of it.

The activity of Burgers in rheology led to a collaboration with his brother Willy. They wrote a report on the plasticity of crystalline substances and studied the theory of dislocations in crystal lattices. Burgers discovered the so-called screw dislocation.

Burgers paid much attention to suspensions, but he was not satisfied with his results. The essential problem was the lack of convergence of the sum of the separate effects of an indefinitely large number of falling spheres on a given sphere. He also became interested in compressible flows. He paid, for instance, attention to the interaction of two shock waves. He also became interested in gas dynamics and studied turbulence in rotating interstellar gas masses.

Burgers enjoyed studying technical problems. He received a medal from the City of Rotterdam for his work on the ventilation system of the Maas tunnel. His work on windmills is another good example of his interest in technical applications (Figure 2).



Figure 1 - Jan Burgers at work at the Delft University of Technology.



Figure 2 - Burgers inspecting a model windmill near Delft around 1940 [51].

Burgers was also a dedicated lecturer (Figure 3) and carefully prepared the slides for his presentations (Figure 4).



Figure 3 - Burgers lecturing at the Delft University of Technology.





Figure 4 - Examples of slides used by Burgers. Top: hotwire anemometer; bottom: boundary layer suction.

When we went through the Archives at the University of Maryland and at the Delft University of Technology, we found 35 typewritten pages of biographical notes written by J.M. Burgers himself in 1962. These autobiographical notes cover two topics: a first chapter discussing his environment at home and a second chapter dealing with school and university education. In the manner that these autobiographical notes were written, it would seem that Burgers planned to continue with some additional chapters. However, checking the archives at the University of Maryland, at the Delft University of Technology, and at the Niels Bohr Library of the American Institute of Physics we have not found any additional chapters. Hence, we have concluded that Burgers did not continue to compose a more complete autobiography. However, he did leave accounts of his memories of his activities at the Technical University of Delft [4, 5].

This booklet is organized as follows: Part I is devoted to the activities of Jan Burgers in The Netherlands. In Section I.1 we present an English translation of an assessment of the importance of Jan Burgers for science and technology, originally written by F. Alkemade and G. Ooms on the occasion of the 90th anniversary of the Dutch Physical Society (Nederlandse Natuurkundige Vereniging) [6]. The remainder of Part I describes the personal reflections of Jan Burgers about his life in The Netherlands. Specifically, Section I.2 discusses extensively the influence of the parents on the young Jan Burgers. Section I.3 deals with his primary and secondary school education and with his experience as a student and young scholar at the University of Leiden, including the somewhat complex interactions of Burgers with his PhD supervisor, Paul Ehrenfest. Information about interactions between Burgers and Ehrenfest can also be found in [7, 8]. The personal recollections of Burgers concerning his tenure at the Technical University of Delft are presented in Section I.4. In Part II we give an account of the contacts of Burgers with the USA in general and of the activities of Burgers as a Research Professor at the University of Maryland in particular. Some supplementary documentation is provided in a number of Appendices.

PART I - JAN BURGERS IN THE NETHERLANDS

I.1 Assessment of Jan Burgers in The Netherlands

by F. Alkemade and G. Ooms (from [6])

The origin of Johannes Martinus Burgers' (JMB) serious and broad interest of science can be traced back to his youth in Arnhem. His father was in his free time occupied with many kinds of scientific endeavors: he acquired old microscopes and organised lectures. His sons Jan and Willie (later the Delft Professor in the Department Metallurgy, W.G. Burgers) assisted him in these endeavors. At the HBS (secondary school) JMB discovered to his surprise that he overtook his father in knowledge. After finishing his school education he stayed home for two more years, to follow a short course in Latin and Greek. In those years knowledge of these languages was required for admission to a Dutch university.

In October 1914 JMB started his study in physics at the University of Leiden. He was lucky that he could follow lectures of some well-known scientists (Hendrik Lorentz, Paul Ehrenfest and Heike Kamerling Onnes). In the laboratory of Kamerlingh Onnes the young physicist got a thorough education in experimental work. After his doctoral examination Lorentz asked JMB to become conservator at the Physics Laboratory of the Teyler's Museum in Haarlem. In January 1918 he left Leiden. In Haarlem JMB was most of his time occupied

with his doctoral thesis dedicated to the atomic model of Rutherford-Bohr. His research concerned adiabatic invariances, a subject that typically belonged to the old quantum mechanics. During this time JMB laid a solid basis for his skills in the application of mathematical techniques. That would be of much importance for his later work. In September 1918 JMB left Haarlem. Two months later he defended his doctor's thesis with success in Leiden with Ehrenfest as promotor. He had then already been appointed as Professor at the Technological University Delft.

His tutors Lorentz and Ehrenfest had recommended JMB for the new function of Professor in "Aerodynamics, Hydrodynamics and their Applications". After his appointment in Delft, JMB literally had to start from nothing. First of all he needed to acquaint himself with this new field of research, at which not much attention was paid in Leiden. Moreover, in Delft this subject was also virtually new. In his oration JMB formulated a research program, which he planned to carry out during the first ten years. He wanted to pursue a profound investigation of the flow of gases and liquids with the aim of solving practical problems. In addition to theoretical work, also many experiments would be initiated.

Besides fluid mechanics itself, JMB had to learn to lecture on the subject. The opinions about his lectures differ. Some students remember his clear (although somewhat fast) explanations of difficult topics. Others found that he discussed topics at which he was clearly still working himself and that he was speaking somewhat muttering in front of the blackboard. JMB himself was not proud of his lectures. He saw too many difficulties that were present in the material to be a successful teacher. The number of students who received their doctor's degree with him was relatively large. Before the war it was rather unusual to write a doctoral thesis in Delft.

Turbulent flows are still not well understood, but in the twenties of the last century there was really virtually nothing known about it. Experimental data were scarce and it is not surprising that JMB rushed to carry out experiments with so-called hot-wire anemometers, with which velocities in turbulent flows in his brand-new wind tunnel could really be measured. Measurements with hot-wire anemometers would remain a speciality of the Laboratory for Aero- and Hydrodynamics for a long time after JMB's departure.

During the thirties, statistics started to dominate turbulence: there occurred a shift from the "eddy" models, developed by Taylor and Prandtl in the twenties, to a statistical description. JMB, however, proceeded along a different route in his turbulence research. He decided to pay his attention to models with which the essential aspects of turbulent flows could be investigated. For that purpose he replaced the Navier-Stokes equation by an elementary and easy to handle equation: the Burgers equation. This equation has found many applications also outside the field of turbulence.

Next to turbulence, JMB was active in other fields of fluid mechanics. He became interested in rheology. A good occasion to become involved in that field occurred when in 1932 the KNAW (Royal Netherlands Academy of Sciences) installed a so-called "Viscosity Commission" consisting of researchers from different disciplines: physics, mechanics, chemistry and biology. JMB became secretary of that commission. This brought him in scientific contact with his brother Willie. Together they wrote an article about the plasticity of crystalline material. JMB discovered the screw dislocation. This work brought him many international contacts and the term 'Burgers vector' became well known as part of the new type of dislocation.

JMB's involvement with applied research started almost immediately after his appointment in Delft. Colleagues, who had important contacts with industry, asked him regularly to collaborate with them on

problems that were encountered in industry. Among others, he worked on the following problems: cooling water supply of power plants, mixing of clay with water, flow in the bend of a cavitation tank, flow resistance in the ventilation circuits of drying ovens, vibration of concrete, and the influence of wind on a kindergarten. JMB had also many contacts with the national research institutes. He advised on all kinds of problems: construction of a wind tunnel, scaling effects, flow of underground water, etc. He made extensive calculations of the flow in a ventilator of a centrifugal pump. He developed ventilation systems for the Maas tunnel and for the mines in Limburg. He developed also an improved version of the blades of windmills.

During World War II a time of reflection dawned for JMB. He thought about the fate of society, the influence of science and technology on it and its reconstruction. He also reflected on philosophical themes such as "causality" and "purpose" in relation to life, in which he was strongly influenced by the philosopher Whitehead. After the war he felt that he had a mission: society had to be warned for the dangers that scientists had created. JMB established together with others a "Study Centre for Societal Problems" with the purpose to study those problems.

A few years later he left this period again behind himself and became strongly involved in new very promising topics in fluid mechanics. The Netherlands started to oppress him. JMB was determined to emigrate to the United States. When he visited the US for the first time in 1930, he felt directly attracted to that country. He was very much impressed by the nature and character of the people. During a short visit to Caltech it was proposed that he would stay for a period of six months to lecture and to pursue research. He accepted the offer gladly. In 1955 JMB left his fatherland definitively to start a new life at the University of Maryland and built a new scientific career. Also in the choice of his topics he broke away mostly with Delft: he dedicated himself in particular to the behaviour of gases at high temperature, plasma's, shock waves and magneto hydrodynamics. He died in 1981.

I.2 Environment at home

by JM Burgers, Autobiographical notes

I have lived with my parents from my birth until my 19th year, when I left for the University of Leiden. Since the education that I have received at home has had a decisive influence on my development, I want to describe some features of the environment that have acted on my brother and me. To expand this into an actual biography of my parents, with observations on the circle of relatives and friends amidst whom they lived, would require an amount of work much larger than I can afford at this moment, although it would form an interesting story. Moreover, to my regret, there is no record of many valuable details, and often my memory gives only vague indications.

My father was born on October 23, 1862, in Arnhem, from relatively humble parents. My grandfather, whom I have not known, was a carpenter. He seems to have had a good sense of humor and the gift of acting, both matters which my father inherited from him (and my brother afterwards). Father had no more than a somewhat scanty primary school education; later he learned some French and German, and he certainly developed a good style for official letter writing, concise and to the point. From 1876 till 1886 father worked in the Arnhem office of a firm for the dispatch of parcels and goods, where an uncle of his was local director. For some years he lived with his mother in the village of Oosterbeek near Arnhem (walking to and from every day, a distance of about 4 km), and from 1884 to 1886 he also fulfilled the evening service at the post office in Oosterbeek, at the same time acting as telegraph operator. In 1886 he became "assistant" at the railway post office in Arnhem, where the parcel post from Germany is received and subjected to customs

inspection. His work was heavy: from 8-12 a.m. and from 1-5 p.m. in the daytime, plus every other evening from 8-10.30 p.m., and every other Sunday morning and Sunday evening, There has even been a period of about I.5 years, when he had to work every Sunday morning, and evening. Free Saturday afternoons were utterly unknown at that time. Often there was work in (unpaid) overtime, when trains were late.

Father has developed himself extensively by reading. I do not know when this self-education began. It seems that in 1888 a friend once sold to him a small induction apparatus (operated with a galvanic pile, such as used in the old days to produce nice electric shocks), and added to it some volumes of the "Ideeën" by the author E. Douwes Dekker (1820-1887), who wrote under the pseudonym "Multatuli". Multatuli, in deeply felt, earnest, and at the same time brilliant statements of his ideas, attacked many conventions that stifled society, and penetrated into everything, with an outlook far surpassing that of almost everybody at that time in our country. These books have opened the eyes of many men and women in The Netherlands; they also did this to father and stirred him profoundly. They must have activated in him an already existing urge to understand. Father's genuine enthusiasm grew to such an extent that he began to collect more books and also to make instruments for himself, or to buy some parts; he wanted to see what he had read with his own eyes and with his own apparatus. In this he found sympathy and support from some friends, while also, from their marriage (September 8, 1893), my mother has fully supported all my father's inclinations for study. I do not know whether she came first in this respect, or whether these friends had come earlier, but my mother's wedding present to my father was a good microscope. Although not being a scientist in the strict sense, my father had an excellent way of explaining to other people that what he had understood himself. He was very versatile in performing demonstration experiments with his instruments, and he started to give series of popular lectures on various subjects, a course in electricity being foremost.

Again I regret not to have data about how it began.

Apparently, father originally had been reading sections from Multatuli's works to members of a society of freethinkers "De Dageraad" (The Dawn); he had a good voice for reading aloud and for speaking in public; he always took his hearers in. I think that this was connected with his sense for acting, and also with a sense of fairness and of balanced criticism (he never became a hero worshipper).

I cannot reconstruct how father's lectures started as a more or less regular feature in the family life. They were of various kind: sometimes public lectures upon the invitation from an educational society, or a school, or from just a group of interested people, relatives, friends or other acquaintances, people from intellectual circles, or some old ladies of the aristocracy who took an interest in science. Lectures were also given in neighboring cities, requiring a lot of preparation for the transportation of the instruments. From about 1908-1938 each winter in Arnhem father gave about half of the series of "Popular Lectures" arranged by the local Physical Society, lecturing on seven or eight consecutive Sundays from 7 - 8 p.m., before an audience of simple people, who were very devoted to father and felt the spell of his words and his experiments.

The main subjects of father's studies and lectures were elementary electricity, "static" and "galvanic", as it was then called; the microscope as an instrument, with its history; the microscopic living world; astronomy; geology. Every lecture was carefully prepared beforehand. For many lectures father also made extensive collections of slides (mainly reproductions from illustrations in books or periodicals; also from microphotographs which he had started to make, and photographs from specimens of his collections).





This was done in the spare hours and the free Sundays which were left to him by his work at the post office, and my father often has said that doing this, notwithstanding the heavy work at the office, was the one thing which has kept his mind open and has protected him from becoming depressed.

It is true that the subjects mentioned looked simpler in those times than they do now. But it should also be kept in mind that many qualitative demonstration experiments can be extremely interesting to all kinds of people, if well explained. We enjoyed very much the whole "physique amusante", which developed from experiments on static electricity. Father first had one Töpler-Voss electrical machine (acting on the induction principle), with glass disks, for producing sparks. This machine was transported in a wooden box to every place where lectures were given. It was operated by hand - how often was it my task to turn the handle for the demonstrations. Later on, a second one was acquired, and then two Wimshurst machines that were more powerful. One of these had four glass disks, and could be operated only during frosty winter days, when the room was heated and the air was dry. It gave sparks of 19 cm length, which we sent through large pieces of quartz or through crystals, in order to see the reflections illuminating the whole piece. With this machine we also could work an X-ray tube which father had received from a doctor, as it had become too hard for operation by the then existing induction coils (at that time we had no idea of the danger which may arise from radiation!).

We had numerous Leyden jars, and also so-called "Franklin plates", capacitors made from a piece of sheet glass, with tin foil on both sides. Often one side was cut up into numerous little pieces, so that one could see the small sparks between them during the period of charging, and then a very bright spark, beautifully ramified, at the moment of discharge.

There were also many Geissler tubes, for the discharge through rarefied air, with colored glass bent in various shapes. We had galvanic batteries (father mostly used those which have a mixture of diluted sulfuric acid with potassium bichromate as electrolyte). Father made himself galvanometers with so-called "a-static" needle systems, which were suspended from a thread of unspun silk, obtained from silk worms which mother reared during one summer. Electroscopes were a specialty as well. The point of glory of the course was a demonstration of wireless telegraphy, with the Branly coherer as detector.

As far as I can remember, there have been many books at home, though often bought second hand. There was also a collection of minerals and later on there were shells; mother sometimes found an odd collection on the market place, left over from a sale of furniture. My father even gradually brought together a collection of historical microscopes, (again mother often was a great help in acquiring valuable items); after his death this collection has been presented by my brother and by me to the Government Museum for the History of Science in Leiden.

As I mentioned, my father's interests in these subjects were supported not only by my mother, but also through the presence in Arnhem of several friends with similar interests. They often worked together. Also father's acquaintance with a very good instrument maker helped him greatly during many years. I still remember the building of a large Ruhmkorff inductor, on which one of father's friends (still alive at this moment (February 1962, age over 80 years) cooperated for many months, but it did not develop the power father had hoped it would give.

Usually there were many discussions between father and his friends, on a wide variety of subjects. Since these friends at the same time were good friends to my brother and me, they have performed a significant

part in our education. For instance, they helped us with our playthings, with work in cardboard, wood, or other forms of handicraft, with drawing and sketching; and further with intelligent information and advice on many subjects.

Nearly always there was a microscope on my father's table. From our youth we heard about the strange world of life that is found in water. Once we had an aquarium and saw the making of a floating nest by the spinning water beetle, and the metamorphosis of a dragonfly. Father had various books on natural history, and amongst them there was Ernst Haeckel's "Kunstformen der Natur", with 100 large plates of beautiful representations of animals and plants, in particular the microscopic ones: Radiolaria, Foraminifera, Diatoms; various types of jellyfish and polyps; and many others.

During the summer we made many walks with father and mother in the surroundings of Arnhem, which have always been very beautiful, and which at that time were still much more free and unspoiled than they are now. We looked for interesting stones, and often found pebbles with fossils.

Father's mind was directed to clearness in thinking. At the same time it was, what I would call, "integrating", seeing things together and trying to understand their relations. Although attentive to details and to their meaning, his mind was not analytic in the sense that he felt interested in precise distinctions and divisions for their own sake. His interest moved easily from the wonders of the microscopic living world to the stars and their immense distances, or to demonstration experiments in electrostatics or galvanic electricity. At the same time he was a good talker, who liked a joke and who could take jokes, and he was a good actor. He was able to entertain, often with sparkling conversations, our many relatives and acquaintances, who came to our home for friendship and conviviality.

My father implanted in me reverence for the wonders of nature. I have inherited his desire to see things together, as well as his broadness of interest (I went even further than he did). I have always been an absorber of knowledge and one who likes to reproduce thoughts in a re-arranged form trying to bring out some general point of view, more than one who continually probes for new relations and tries to make discoveries. I have gone more "into the breadth" than "into the depth". But I can witness of the joy which results from understanding and from a wide area of interests. What knowledge I have, has always been for me something to live with - it is a part of myself.

On the whole, father's inclination was "descriptive". I may also characterize it as "constructive" or "educative", as opposed to breaking down a lot of ideas by continual criticism. His was a mind sustained by reverence for what could be known. In his work, both at his office, and at home, for instance when he was engaged in making some pieces of apparatus, or cleaning some instruments, he was methodic and precise, with the need to finish things well and never to leave loose ends. He did not like things lying around in disorder.

In political matters his attitude was liberal, and he felt attracted to socialistic ideas. Being a simple civil servant in a subordinate position, he did not like to go far. Also he was aware that usually there are many sides to each problem. He was not a member of a party, or of a church. He did not feel the need for close association with others, or the need for faith in a personal God. He stood on his own, like my brother and I learned to do. Father never used the notion of sin. The Bible was put away from us, as father and mother judged that it contained matters that are not fit for children's ears. We had no religious education in the customary sense, but we were imbibed with responsibility and mutual faith, by the atmosphere reigning at home. It was only much later, when I was a student in Leiden, that a chance remark by Ehrenfest, who once

read to us Tolstoy's comment on Joseph's capitalistic activities in the service of Pharao, made me aware that one could look at the Bible from a point of view which had connections with current topics. My interest in it came later from the side of ancient history, when a modern translation of the Bible had made its contents much better accessible.

To come back to my father's attitude, he knew that we have to face the consequences of the deeds of other people as well as those of our own deeds, and that we must carry the burden together. Never would my father have said "why must this strike me?" He bravely accepted his share in troubles and disaster, and did not think himself as separate from his fellow men in this respect. He was never afraid of death and saw his end coming as a rest, for which he longed after the terrible experiences of the last year of the Second World War (he was nearly 84 when he died on August 8, 1946). He did not ask for assistance from a minister, being true to his own ideas until the end. In all this my father was not dogmatic. Perhaps it was sometimes more a fancy than a reality that he believed always to be open for argument, but he certainly taught us to be open minded, and I know that the best teachers for young people must themselves have a strong belief in what they say. As to me, I am too often aware of all the complexities and alternatives that are present in any situation, to be a successful teacher of young people. I am apt to explain too much and leave the burden of the decision to others.

My father was always honest and scrupulous in his dealings with other people. That this formed a part of our education will be understood. He taught us respect for the civil service, which is needed to run a country, as well as respect for our teachers. He had an aversion for anything that was showy or pompous, or even purely ceremonial, in which he could not see himself as a performer. Perhaps there was an element of shyness in this. Nevertheless, he took care not to hurt anybody, and he was not attracted to the role of

a fighter. But he was carried on by a sincere idealism and respected all mankind as forming one great community. Our education was not directed towards patriotic feelings or towards some conventional loyalty, but towards a cosmopolitan outlook. We never were taught to consider a symbol as more important than humanity. The best of father's friends, those to whom we owed much in our education, shared these ideas. How great a shock came to all of us in August 1914, at the outbreak of the First World War, can perhaps be imagined.

I have kept from my youth an utter lack of feeling for ceremonial performances. In certain respects I am more reticent than my father was, who could be very convivial. Sometimes I can understand (on theoretical grounds) that a form of ceremony will add to the meaning, say, of a reunion of people who have come through a struggle together, as was the case in The Netherlands just after the liberation in May 1945. On such an occasion I might even be able to express in words something of the thoughts that bind us but on the whole I find it difficult to see myself as a part of it. Also, for giving a speech, I am apt to be too concise, too brief; I cannot elaborate on such matters.

We had music lessons, piano, and for my brother later the violin (father and mother used to make some music long ago, when we were small children). I have never learned to dance, and connected with this in some way is that I cannot give myself with ease to something in which emotional feelings would take the lead over thinking. I sometimes have the feeling that when I should become very angry with somebody, I might loose my self-control.

So far in these pages I have mentioned my mother only a few times, but it must be understood that there has been a deep reaching influence from our mother's side in the development of my brother and

me, which, however, is harder to describe than the influence of my father. Mother was three years older than father, being born on October 5, 1859. She came from a family with some intellectual standing. It was a tradition in her family that in every generation there was one who studied theology and became a (protestant) minister. A cousin of her, whom I have known quite well, was doctor of medicine and had the position of inspector of public health in our province; he visited us from time to time and often talked with father about micro-organisms. A brother of my mother was surveyor. And mother herself, although her school education had not been much more than that of father, had a very intelligent mind and was clear in her thinking.

Mother made the home to be the place where it was safe for us to live, and where all our relatives were glad to make frequent visits. The fact, that my father and mother lived in a way different from others, did not set them apart. And though amidst our relatives we were materially the poorer, I think most of them sensed that in our home there were things which were not so richly present elsewhere; interesting talk by my father, who was liked and even admired; things to be seen and heard; and hospitality and sympathy from my mother, who was a lady in every respect, who attracted everybody by her natural distinction and charm. She always had tea or coffee for everybody. She ensured that there was an atmosphere of comfort for those who visited us.

My mother was deeply devoted to father and to his ideals; she helped him fully and unconditionally, to such a degree that she sacrificed most of her own feminine desires and ambitions, wishing no more than to live with him and see through his eyes, so that father's program of life might be realized. She gave great care to our household; everything was attended to and all was neat and clean; she worked hard, and, as far as we could know as boys, everything ran smoothly. She was very apt at needlework and had been an assistant in

a shop for needlework and the like, during several years before her marriage. The meagerness of my father's salary made it necessary that she sometimes undertook embroidery work for others (she embroidered the gold lettering on several banners for a corporation in Arnhem for scanty payment, as none of these corporations were rich). With all this she kept a definite refinement in our style of living, and always had a keen interest for everything that reached us. It is only much later that I have understood something of the sacrifice our mother has made. In the picture that settled in my mind, the image of my father, who certainly was an exceptional man, takes such a large place that it puts my mother somewhat in the shade, but she also was exceptional.

During many years my mother, who had a good handwriting, wrote a great deal of the notes which father used for his lectures, and she took part in the things on which he was working. This had been a constant great joy to her in those years. Later on, much and gradually all of this work fell upon me, and partly also upon my brother (we often made drawings for father's lectures or helped in the making of instruments). Mother then lost her place at father's table, opposite to him; that became my place and she remained more in the other room, the living and dining room.

To give some idea of my mother's influence, I would mention that father was not deep emotionally. He even had a certain intellectual limitation (perhaps a characteristic of the later part of the 19th century), which took for granted that reason would pervade the world. He was not aware of the possibility of far reaching mental strains, for which he had no understanding. Emotionally mother was a much deeper personality, though she forced herself to suppress much of this. I believe it is from her that I have inherited a desire to look for something beyond the evident outward appearance of things, in particular when I am moved by a landscape. I cannot find back where this originated, although I have some recollection of the enthusiasm




mother could express when she saw a view which extended into a distance and suggested depths beyond human reach. Mother also loved flowers and could be very much impressed by the unexpected find of a wild flower. These strands of feelings have come to some fruition in myself only later, through influences of my first and of my second wife, but the roots certainly go back to my mother. I still am moved by a view through trees, a little violet in a forgotten corner, the call of a bird, the stars - these are the privileges which one can enjoy as a pedestrian who can look around and listen. My brother has been deeply impressed by mother's personality; I think that in his case it is her image that always comes first.

As a child my mother had received a religious education, but she abandoned all connections with a church through father's influence. She often told me that she had known the feeling of missing something, the singing in particular, which can be so helpful to give a certain outlet to one's feelings. She often told us about her youth - owing to the early death of her father, she had spent some years in an orphanage; she spoke about the boys she had known, and about the minister who gave religious instruction and for whom she always retained a great respect.

Most of our relatives were on mother's side, and there were many nice and convivial people among them. I must mention, however, that father had a sister, five years older than he, who was a widow from the times from which I have recollection. She lived in Arnhem, not far away from us, and my brother and I (and also the boys about whom I shall speak later) visited her often. She loved all of us and spoiled us sometimes a bit.

As I mentioned before, I have been educated without any church connection. It is only later in life that I developed interest in religious matters and obtained some understanding for their importance. I cannot

believe in a personal God and I never feel an inclination to pray. What I possess as religious feeling probably comes from my mother, although there is in it a part derived from my father's idealism and, if I may say so, from his mental courage, reinterpreted in a way which grew up in my own mind. Here in this country (the United States) my wife and I have become members of the Unitarian Church in College Park.

When father had to retire from the post office in 1913 as a consequence of insufficient health (he had twice suffered from pneumonia, and was overworked by the heavy work at the post office and troubles with the customs people - father, belonging to the postal service, had to act in the interest of the addressees of the parcels, whereas the customs officers acted for the government), his pension was small. Our household, however, was held up by the fact that several boys boarded with us, for a part boys whose parents lived in what were then The Netherlands East Indies, where secondary school education was scarce. The first one of these boys had come already in 1904. Father also received some money for his courses, but that was not a large sum, and would have been grossly insufficient for the household. Moreover, part of it served for buying instruments and appliances. About 1913, I, myself, and my brother were already earning some money by giving help and advice to other pupils of the secondary school, who could not keep up with school and homework. Father was considered to have a good influence on the education of children and he gave much time towards taking care of their schoolwork and their behavior. Father loved children and could keep discipline, without becoming too exacting; he could be very playful and always had attraction for them. This now formed one part of his occupation, the other part being his own studies and his lectures. In retrospect I think that father's attitude with regard to child education was somewhat naive, and directed exclusively to the intellectual side and good behavior. Father certainly had a good influence upon children but he had no deep understanding for difficulties from which children may suffer. I must observe that in that period,

now 50 years behind us, educational problems on the whole did not go as far as they reach now; life still was somewhat simpler and children were more inclined to accept discipline. What I must stress is that the burden upon mother was very great. She also gave her love to all the children, in particular to the first ones who came, and both father and mother did their utmost not to make any difference between us (my brother and me) and the other children. But we had scanty domestic help and there was a lot to do in cooking and in taking care of the house, in mending clothing, etc. Domestic appliances were much less efficient than they are now, and always the amount of money that could be spent had to be calculated. Moreover, it was mother who always first felt the impact when we boys were quarreling or fighting. It also was mother who carried on the correspondence for the parents of the children (who lived in the East Indies) and who mostly talked with teachers or the principal, when there were difficulties with the progress of the children at school.

(I may mention that also when we were away from home at the university, and later when we were married, it was mother who always wrote letters to us; father's contribution usually was a loving greeting with the words "mother already has written everything").

As I mentioned, the first boy from the East Indies, A.F.E. Jansen, came in 1904; his brother, J.V. Jansen, came about 1907. Their coming has had a very important effect for my brother and myself. Before this the highest position on the social scale which was seen for us, was to become a schoolteacher (some of our female cousins, who were rather bright, went that way), or perhaps a surveyor or something like that. However, the father of these boys desired them to go to a secondary school, and as the first one did not differ much in age from me (he was 10 months older, but we were in the same grade), it then became possible that I should go with him, and that my brother should follow in due time.

This was a first step ahead. It then happened that teachers and the director of the secondary school gave advice and help, and gradually it became understood that we might look forward to scientific studies.

The two boys Jansen whom I mentioned, have become very close friends to my brother and me. The older one has studied mechanical and electrical engineering, and for many years has been conservator for metallography at the Technical University in Delft. He took an important part in the programming and arrangement of the new laboratory for metals that was opened in 1961, where my brother now works. The younger brother is now acting director of the Museum for Ethnography in Rotterdam and has an extensive knowledge of Western American Indian peoples, and of the peoples of Polynesia. Both of them have more than once expressed their indebtedness to our parental home. I am sure that also the other boys, and the two girls who later on have lived for some years in our parental home, still think back to those years with much pleasure and gratitude; in this home everybody got something which he could take along for his life. The last boys left my parent's home about 1926. For father and mother there have then been some quiet years, which they have both enjoyed. Also my mother found much pleasure in this period of freedom. I believe she still has taken care of a collection of butterflies that somebody had presented to my father. In the fall of 1929 an illness manifested itself, which became more and more serious, and which has clouded mother's last years. She died in October 1931.

Since then my father lived alone with a lady housekeeper, amidst his collections and his friends, and we regularly visited him. In 1940 came the disaster of the Second World War and the occupation of The Netherlands. Father could manage, and we still came to him each year for many short periods - until the airborne American attack upon Arnhem in the fall of 1944. Then father and his housekeeper were evacuated. For three weeks they straggled around; finally they were picked up through the Red Cross and we could

bring them to Delft where they stayed with us until August 1945, after the liberation. Father then returned to his house in Arnhem, with his housekeeper, but much had been destroyed and parts of his collections and instruments had been stolen (the collection of historical microscopes had been rescued through Government services). Much of the city was gone and several of our friends were no longer alive. Gradually father's forces diminished, from a kind of bodily and mental exhaustion until the end came in August 1946.

I will insert here three quotations from letters that my brother and I received after our father's death.

(From Dr. B. Meylink, principal of the secondary school in 1948, and teacher of physics in the years that my brother and I were pupils there):

"I never met anybody, not even amongst the very greatest, who had such a genuine enthusiasm for the natural sciences with respect to all new things which were discovered and to all new points of view which came to the foreground. He was so full of admiration for the grandeur of nature and the infiniteness of the universe, that it gave warmth every time you heard him speak about it."

(From Professor dr. A.A. Pulle, professor of botany at the University of Utrecht, who was born in Arnhem and had known my father when he - Pulle - was a boy):

"During the last days I have thought rather much of the old times when your father and mother lived in the Gravenstraat, where the oldest of you was born on a winter evening. It was on the 13th of January; it must have been about 1895. A short time before I had made your father's acquaintance. At that time he had a primitive microscope (it must have been a good microscope, the one my mother had given to father, from the firm of Reichert in Vienna, Austria - JMB) and with great enthusiasm he looked for all kinds of micro-organisms. It was then that for the first time I saw diatoms (I am inclined to believe that this has been of influence on A.A. Pulle's later career; he also came from a modest family, were the mother, a widow, was teacher of needlecraft at one of the schools in Arnhem; she was a friend of my mother - JMB). Afterwards he has moved to a different quarter of the town, and then turned more to physics. The man was a wonder; what has he not attained with simple means and how great was his rare enthusiasm. My oldest boy, who came back from the East Indies a month ago, once stayed for a week in your father's home when he was a pupil at the secondary school, and even now he still could tell about all he has seen and experienced during that week."

(From Professor dr. J.H. Oort, professor of astronomy at the university of Leiden and a great friend of my brother and me):

"I have always felt a great admiration for your father. As I wrote already to your brother, I felt in him something of that which the apostles must have possessed, a an inner, simple, but unbreakable, strong faith, which radiated through the whole of his life so far as this was visible to me. There was no other such a man in Holland."

I will attempt to give an impression of my parents' home, as it must have looked to a visitor. To my regret my memories no longer are precise. In August 1944 (now almost 18 years ago) I saw it for the last time in the state it had been for a long period. But during that period I had no more looked at it with fully the keenness of interest it deserved. Since I had left home for the University (October 1914), there gradually developed differences in outlook between myself and father. This was a natural result of the expansion of my horizon and of interest in matters where I wanted to think independently. Also I married; my wife and I were very close in our thinking; we built our own life, and while father felt very strongly bent towards me, I had the need to be more detached from him. The same was the case in my second marriage, after the death of my first wife. When we came to Arnhem and stayed with father and mother, or, after mother's death, with father, it still was a very interesting environment, but I did no longer feel it as my environment, and even the collections of instruments, of minerals and shells, were no longer in the center of our interests. We also knew that it would be impossible to find room for all these treasures either in our house, or in that of my brother and his wife. I have no recollections of the apartment where I have been born, but I remember much about the one to which father and mother moved in 1897. It was an apartment upon a second floor (American reckoning), without a garden, but with an attic. Much of father's work was developed here, and I believe

that here also began the cooperation with his friends. As long as I have known there were already many instruments for electricity, at least two microscopes, books, and some minerals (although at that time father did not know much about them). Much of father's photographic work started here. In 1906 we moved to a larger apartment, which had a basement, living rooms on the first floor, and bedrooms on part of the third floor and the attic. There was a small garden, although without much sunshine, but still so that mother, who loved gardening, could grow some flowers.

We lived there until 1913, and it was in these years that father's activity was at its greatest. In 1913, when father had already a leave of absence from duty as a result of insufficient health, father could buy a house with the help of some friends who lent money; this house again had a basement, a first floor with two large rooms and a smaller one; a second floor with two large rooms and two smaller ones; and a third floor with bedrooms and an attic. The kitchen, both in this house and in the previous one, was in the basement; when we had dinner in the living room, everything had to be carried up, while the dishes had to go down again (there was no kitchen elevator). The 1913 house had a little garden that was better situated than the former one, and mother enjoyed very much that she could do more in it.

When a visitor would come in, in the 1906 house or in that of 1913, and was ushered into father's study (the front room on the first floor), he would see himself at once amidst cabinets with various instruments. Rooms in houses of this type in Holland had high ceilings and fairly large windows. In the middle of the room there was a large rectangular table; father had his chair on one side of it (opposite to the chimney; other chairs around the table were for us or for the visitors. Behind father's chair against the wall there was a large double set of bookshelves, with many books, and also sets of drawers with mineral specimens or shells, for which I had made the little cardboard trays and the labels.

On the bottom shelf stood many boxes with father's slides (old standard size: 3¼ x 3¼ inches; total number over 1000).

Against the other wall (to the left and the right of the chimney) there were two cabinets with instruments. I believe one had as its lower part a set of large shelves, where drawings and prints were stored. Between the two front windows (to the right from father's chair) stood a (Töpler-Voss) electrical machine, in a casing by itself. Father was accustomed to let it run from time to time with a hot-air motor, to produce little sparks "in order to have ozone in the room" which father believed to be refreshing. Somewhere around were pieces of guartz with crystals, and boxes with glass covers, containing minerals and fossils (other specimens were stored in a room in the basement. On the table there often was a jar with ditch water containing algae, the common polyp (Hydra viridis), daphnias and other microscopic living beings, and also a microscope. Sliding doors (to the left from father's chair) connected father's room with the living room, where mother usually sat. The sliding doors were closed when father gave a course, and a screen for projections could be unrolled before these doors. Sometimes the projection apparatus had a fixed place on the table ready for immediate use; later on, it usually was put aside and had to be brought out for the occasion. A large map of the Moon, in a frame behind glass, hung above the mantelpiece (opposite to father's chair), which map I had made for father. As it had been too much work to put in all the lunar craters, it contained only a selection; but it had all the maria, colored in red, and at that time I was rather well versed in the geography of the Moon.

Suspended from the lamp (above the center of the large table) was a magnetic needle which could swing in the vertical plane, indicating the inclination of the Earth's magnetic field. At times there stood on the table a little motor with clockwork for rotating cardboard disks on which we had painted all kinds

of designs; these merged together for the unaided eye when the motor was speeded up, but presented a variety of interesting patterns when viewed in the darkened room by the intermittent light of a Geissler tube, connected to an induction apparatus with hammer interrupter. Somewhere a Crookes' radiometer was rotating, when the Sun shone upon it.

In mother's room, the living room, there was also a large table (for many years we had an oval table there), for all the boys who sat together at meal times, or were busy on their schoolwork. Along one wall there was a sideboard, and against the other wall, on both sides of the stove, some cabinets with mother's books and treasures (mother had a lovely little collection of exotic teapots), and the piano. The mantelpiece had a large mirror, and there were some pictures on the walls. From this room sliding doors led into a veranda or porch, closed in with glass, where mother had many flowerpots, and a table at which she could sit in the summer. There was no room which mother had just for herself. From the veranda, steps led down into the garden.

The visitor, after having had tea from mother, then was taken upstairs (this refers only to the 1913 house), to be shown into the "museum room", which was above father's study. Here a lot of other instruments were on display in glass cabinets. There was the collection of historical microscopes, which included a hand microscope made by Musschenbroek in the 17th century, which father had rescued from a basket of rejected instruments from the old Physical Society in Arnhem (this Society had come into financial difficulties about 1902, and its collections were sold, mainly to the secondary school in Arnhem; but there were "left-overs"). There was also a large "solar microscope" and several instruments from famous 18th century and 19th century makers. In 1913, if I am not mistaken, father had bought, on a sale, a collection of acoustical instruments, with organ pipes, Cagniard Latour's siren, tuning forks of all sizes and pitches on large wooden

resonant boxes, Helmholtz resonators, an apparatus to show Lissajous' figures with the aid of oscillating bars having various cross sections, etc. But we never have become at home in acoustics as much as we did in electricity, as this collection came when father, although still very active, was somewhat over the height of his desire to absorb new domains.

There were also various "curios" and in later years father often was presented interesting objects by acquaintances. There was, for instance, a tusk of a walrus with beautiful carvings, which had come from Eastern Siberia (Nizhny Kolymsk), and a collection of pieces of amber with insects (both of these are now in possession of my brother). There were many pieces of agate, cut and polished; a beautiful cameo shell, a set of ivory chess pieces, but much of this has been stolen during the period of evacuation of Arnhem. There were also interesting old books on shells (I once bought a whole set on a book sale in Leiden), and a copy of Rumphius' "Ambonese Rariteitenkamer", a famous book on marine animals and shells of the East Indies, printed in 1705 (most of these books are with my brother now).

The room on the same floor above the living and dining room in the 1913 house, was the study room for the boys. On the third floor were the bedrooms, although father and mother had their bedroom on the second floor in one of the smaller rooms. Cold water came to the second floor (in the 1906 house it came at first only in the basement; later a tap was installed in the corridor on the first floor.

Water had to be carried up and down to the bedrooms on the higher floors. How much household work this meant, can be imagined. Mother had some help, and we, as well as the other boys took part in some chores, but this was not extensive. To come back to father's study, I do not remember where the big Wimshurst electrical machine stood with its four glass disks. It has been sold, like some other instruments,

to a school in Eindhoven, already much before 1940. Little electrical motors of various sizes, steel magnets of various dimensions, electromagnets, galvanometers, induction coils of the Ruhmkorff type with hammer interrupter, Leiden jars, Geissler tubes, all such things were to be seen. During the years before 1931 the source of electric currents was galvanic piles, Leclanche for weak currents, and potassium bichromate plus hydrosulfuric acid when stronger currents were required. The mixture had to be prepared periodically, which always needed great care in mixing the acid with water. Some months after we had moved to the 1913 house, we got electric light (there was not yet a cable through the street, but it was put in, just for this occasion); we then could use lead accumulators, which were charged by inserting them into the circuit for the light. We also used a set of old-fashioned carbon filament lamps, which admitted one Ampère each. It will be understood that father always had lots of auxiliary materials, tools, nails, screws, tin foil, copper wire of various thickness, ebonite, paraffin wax, wood, paper in various sizes, and all kinds of odds and ends. Father never could reject anything that still might turn out to be of some use.

After father's death, in 1946, my brother and I had to clear the house rather in a hurry, as it had to be sold for use by other families. There was much we had to throw away; in a sense this was a tragic end, but the more important objects which still were there, got appropriate destinations. Our own houses were not capacious enough to store all the objects (and sometimes the junk) which father had collected; moreover, both of us had already stored much in our houses. Some books and a few instruments were sold or given to father's last friends. The collections of minerals and shells went to our secondary school. The most important books were kept by us. The furniture was old fashioned; there was much which people nowadays did not like to use, or could not use, since modern houses do not have such large rooms with high ceilings. So the house came to an end - it still lives for a large part in our hearts.

There still is a point deriving from my father's influence upon me, which I could mention here. I have said that he was an excellent teacher. He had a healthy naturalness and soundness about himself, and he never sought to overawe anybody by a show of learnedness. He always genuinely tried to make others see what he had understood himself, and knowing the difficulties he had had to overcome in this he had understanding for those of others. The courses he gave, started in our home. It was his gift to explain phenomena, say of electricity or of light, to people who were not educated in physics. He did this from a desire to communicate to others the joy he had found himself in mastering any topic. For this purpose it was not necessary to go to far outlying fields: joy of understanding is possible with respect to the simple beginnings of a science. And the people around us liked to understand. They came to father for that purpose. My teacher of classical languages, for instance, liked to listen to explanations and to argue about them.

There were at that time a number of scholarly educated people in Arnhem and its neighborhood. Many of them were members of the local Physical Society, or of the local branch of The Netherlands Society for Natural History. At the meetings of these societies we could often listen to very interesting speakers, for instance on radioactivity, or on the formation of the coal fields, and I still remember that once the reversal of the yellow sodium line was shown on the screen with a projecting spectroscope. There were also certain gentlemen who had extensive collections of butterflies. Others did biological experiments. The circle of people amidst whom my parents lived, including our relatives, would deserve a special description.

There was also available a good collection of serious popular and semi-popular books and journals on scientific subjects, some of them translated from the German, others original productions by enthusiastic writers and editors in Holland.

When I came to the secondary school, I gradually began to catch up in knowledge with father, and then got beyond him, as the secondary school brought algebra, geometry, trigonometry, etc., and a fuller course of physics (and chemistry) than my father had mastered. But father still was studying himself, and we were in the habit of talking about what we had read and arguing about it. A great event for us was the appearance of a very good translation of R.K. Duncan's book "The New Knowledge" (1908), which opened our eyes to the phenomena exhibited by ions and electrons, and to those of radioactivity. It was a kind of revelation to us, bringing a lot of information and order concerning topics about which we had heard only in a vague way. We then learned how one progressed from Geissler tubes for electrical discharges to tubes exhibiting cathode rays, and father soon got a whole new series of demonstrations under way.

Later on, when I got ahead of father, I still sought to translate my further knowledge into terms which could be captured by him. This remained so, when I came to the university and learned about quantum mechanics, then in its first stage, with Bohr's electron orbits around the nucleus. It came to my mind that a layman has no difficulty to accept that there exists only a set of discrete orbits. The notion of classical mechanics, according to which every orbit should be possible depending upon initial conditions, is rather an anomaly for a logical mind, looking for order and not having been subjected to training in differential equations.

The experience gained in this way has given me the conviction that every term, every notion and every equation used in theoretical physics, belongs to the set of ideas which are common possession of all intelligent and interested people. Every physical notion is linked to other notions and these again to further concepts, and ultimately they have all arisen from the desire to give an interpretation of concrete observed facts. It is only the extreme length of the chains of reasoning, which cause the difficulty for a mind

not trained to absorb such chains and not acquainted with the mathematical way of presenting chains in shorthand form. For a physicist working in the front lines it is often too cumbersome to look for a translation of newly conceived physical notions into common terms. Nevertheless, I believe it should be the task of every scientist to give some attention to the translation of his concepts into terms which make their meaning clear to non-specialists. The ultimate object of science is not its use for technical performances. Its ultimate purpose must be to expand the world picture shared by a large group of people, and to bring to them the joy of understanding. At the same time we should take care to point out the limitations involved in the scientific picture, resulting from its abstraction from values which sets it apart from much, that is contained in life when viewed in its fullness.

Before closing this chapter and passing to school education, I would look back once more towards the atmosphere of clearness and of reasonableness, which we believed that existed before 1914. I mentioned that we made many walks around Arnhem; there was a connection between what we received at home and what we enjoyed in the open air. It laid the foundation for a deep love of the Earth and of the life its carries. It was a time when public transportation was measured to the needs of the excursionist and the pedestrian, when there was much less traffic and far less noise on the roads and when there was not yet the terrific expansion of the population, in numbers and in mobility, leading to the deterioration of the countryside by continuous building of new groups of houses, of roads for heavy traffic, or by "opening up" recreational areas.

Something of the atmosphere of this period is pictured in a novel by J.B. Priestley, "Bright Day" where also a group of old and young people is described who liked to walk. Much of this background of our life vanished with the First World War, although parts of it still could be recaptured in Holland for many years after 1918.

Both with my first wife and with my second wife I have often come back to Arnhem and again we made long walks. With my second wife, during the years "1942-1944", I even got a much better idea of this part of the country since we covered larger distances and had maps who explained the character of the landscape and its geology. Some parts of the country around Oosterbeek even now are still as beautiful as they were 50 years ago, as I saw on a trip with my brother in May 1961.

But it is a curious experience that I believe to have found in America, for instance in Maryland, some of the features and the moods which I absorbed in my boyhood. When coming to the new continent, I have kept open the "eyes of discovery", as an explorer who is elated by the landscapes and the new kinds of flowers amidst which he now can move around. This is a constant joy to me which makes me feel at home here.

I.3 School and university education

by JM Burgers, Autobiographical notes

In this section I will speak briefly of influences emanating from school during the period I lived at home; then I shall pass on to the years at the University of Leiden, and the beginning of my career in Delft.

My brother and I had our primary and secondary school education in Arnhem in the city schools. Primary education was from the age of 6 to 12, secondary from one's 12th to the 17th year. The schools were very good. It is interesting to mention that we wrote on slates in the lower grades. I still remember the series of pictures of three quarters of an apple we made when we were engaged upon the multiplication of fractions. French started in the fourth year. We had much homework. I also think of the geography we had to learn, in particular from the European countries: series of mountain chains in France, the system of canals by which one could go from, say, Strasbourg to Paris, and the cities you met along important railroad connections from one country to another. History brought all the wars in which William III had been engaged. We also learned the grammar of our own language, and quite a lot of French grammar, with irregular verbs and the rules for the passé défini and the subjunctif.

In 1907 the older of the brothers Jansen and I passed the examination for admission to the secondary school, the "Hogere Burgerschool", with a five-year curriculum. My brother came two years behind us, owing to the difference of our ages. After we had passed the final examination of the secondary school in 1912, the older Jansen went to the Technical University in Delft, while I stayed at home during two years for an abbreviated course in Latin and Greek, which at that time was still necessary for admission to the other

Universities, for which one had to pass a special examination. These languages I learned under the guidance of Dr. F. Wolf, a teacher of classical languages at the "gymnasium" or classical school. He was a scholar with a wide interest in literature and he knew Sanskrit. He has never asked any payment for the time he gave me. During the same period (1912-1914) I daily went to the village of Velp to help a pupil of the secondary school with his homework, in order to earn some money; and I studied mathematical and physical topics.

We had excellent teachers at the secondary school. While in those years not directly preparing for university study (although it opened the road to the Technical University), the school furnished a good and extensive all-round education, including mathematics (without calculus), physics, chemistry, natural history, geography, history, literature, the three foreign languages (French, German, English) with summaries of their literary history, economics, an introduction into political science, drawing. We read Molière, Racine and other French authors; learned about Shakespeare, Milton, Dryden, Goldsmith (we read the "Vicar of Wakefield"); and learned about Schiller, Goethe, Lessing and other German authors. I read Dickens's novels at the time with my father in a Dutch translation.

For many years I have remained in close friendship with several teachers and with the principal (director), Dr. H. Hulshof. It was later that I learned that Dr. Hulshof was engaged in the elaboration of a kinetic interpretation of the thermodynamic potential. His doctor's thesis had been on the theory of the capillary layer, for which he had proposed the notion that the pressure in this layer was anisotropic. This idea had found much encouragement from the side of his professor, the well-known (older) van der Waals. In some way, however, there had arisen a difficulty about it with Dr. G. Bakker, who is better known in the literature for his work on the theory of capillarity. It was Hulshof's conviction that Bakker had heard about it through van der Waals, and had worked it out rapidly and then presented it more or less as his own

idea. Knowing the probity and the intrinsic modesty of Dr. Hulshof, I am inclined to believe that he was right. He may have been thinking a long time about this notion of an anisotropic pressure and had been somewhat slower in bringing it into the open, so that Bakker got the start. A curious point is that Dr. Bakker was the teacher of physics at a similar school in The Hague, where my first wife has been a pupil. I became acquainted with Bakker at some Ehrenfest's colloquium; I believe, this was before I knew about the dispute between Hulshof and Bakker.

Our teacher for natural history in Arnhem, A.C. Oudemans, was a man with great enthusiasm and great knowledge. In his class room he had many collections, minerals, shells, butterflies, beetles, etc. and also cages with various living animals, all of which were kept in an excellent state. During 5 or 10 minutes before he started with the class, we - the pupils - were allowed to go around through the room and look at the animals, or at the collections in their drawers.

Oudemans also had the custom to bring to our attention interesting matters happening around us, for instance, astronomical occurrences, or the capture of an uncommon type of fish in the North Sea, etc.; for this purpose he had a bulletin board with clippings from newspapers or illustrated journals. He himself did scientific work on mites, and he was always busy when there was no class, and after school hours, either with re-arranging parts of the collections, or with the preparation of very fine skeletons, or with his studies on mites. He had also a taste for historical matters, and he wrote a big volume on "The Great Sea Serpent", in which he collected and reviewed all references to it throughout the ages up to modern times. His many-volume work on mites (the publication of it was continued after his death from his extensive files of notes) started with all historical references to mites, from biblical times to the present.





He permitted me to come every week, during an hour after school time, with a box of rock specimens or shells from my father's collection and helped me to find the names, which he either knew by heart or found out by comparison with the school collections. From Dr. Oudemans' textbook on natural history, it was in particular the third part, on mineralogy and geology, which gave much information to my father and me. I remember that a short time after we had bought it, father, having become interested in the characteristics of the six crystal systems, made a set of neat models, from knitting needles (furnished by mother), drinking straws, cut into sections to form the axes, and threads to represent the edges of the main forms, all fitted together with sealing wax - which, as is well known, was an important item in the physicist's outfit sixty years ago.

My teacher of mathematics in the advanced years, Dr. C. van Beek, lent me Holzmüller's book on extended elementary geometry. Dr. Hulshof gave me Sturm's "Cours d'Analyse", in which I have been working at the same time that I was busy with Latin and Greek.

Dr. B. Meilink, the teacher of physics and later the successor of Dr. Hulshof as principal, who guided his classes with almost no effort and who inspired discipline by his natural and calm way of doing, lent me Maxwell's "Theory of Heat". I can still remember how I was puzzled by the chapter on the "Representation of the Properties of a Substance by Means of a Surface", which I could not understand at that time. Later on he lent me Maxwell's "Treatise on Electricity and Magnetism". As will be understood, this contained much that gave me great difficulties, as, e.g., the chapter on spherical harmonics, the title of which made me think of Kepler's harmony of the celestial spheres. Nevertheless, I got much out of the book, for instance, on the theory of electric currents. Dr. Meilink also procured for me a set of lecture notes on mathematics and on physical chemistry from a student at the University of Amsterdam, which I worked through.

Further he permitted me to do some practical physics at the school laboratory, as a preparation for university study; this gave me some relief from the course of practical physics in Leiden later on.

We had an excellent teacher for chemistry, Dr. B. Holsboer, but although I had dabbled a little bit in chemical experiments, this subject has never attracted me greatly. I was interested somewhat in the formalism of organic chemistry, but all this was long before the modern theory of valence and of the various types of chemical bonds was developed.

At the same time there still was much material for study in father's books. There was, for instance, an almost complete collection of the volumes of a monthly journal "Album der Natur", which in its later years (it stopped about 1912) brought papers with extensive digests of new subjects in various sciences, for instance, on the phase rule and its application, on colloid chemistry, on the early stages of the mutation theory.

From one of father's friends I got two little volumes of the Sammlung Goschen on metallography, which gave more illustrations of the phase rule and from which I also learned about the texture of metals as revealed by the metallographic microscope. A volume of the then existing "Scientific American Supplement", which the father of the boys Jansen had sent to us, had articles by R.A. Millikan on his exact determinations of the charge of the electron. In the journal of The Netherlands Chemical Society the extensive text was published of a lecture given by professor H.A. Lorentz on the various determinations of Avogadro's number (if I remember correctly, it was mother who directed our attention to it, having read about it in the newspaper).

From another friend I got the money to buy (second hand) the book "Meteorologische Optik" by J.M. Pernter and F.M. Exner. In this book I found a presentation of Airy's diffraction theory for the rainbow, to which Pernter had added an extensive calculation of the color distribution, for various droplet sizes, on

the basis of a reduced set of Maxwell's color equations. Since father and I were much interested in the polarization of light and its application to the observation of minerals in thin sections under the microscope, I started to work out a calculation of the interference colors which could be observed, e.g., with sheets of mica of increasing thickness. I wrote a descriptive article on polarized light for the semi-popular journal "De Natuur", in which I gave the results of this calculation. The article was illustrated with several photographs of the structure of granite, basalt and other rocks, made by father with the polarizing microscope, and with drawings which I had made. It must have appeared, I think, in 1913. That I still liked to read on natural history, and in particular on the theory of evolution, will be understood.

We certainly made many walks in that period, but I have no specific recollections. It is curious to note that, apart from some very brief visits on bicycle to Emmerich and to Cleve, two cities in Germany close to the border of our province, I had never been outside The Netherlands. We always looked at the international trains coming through Arnhem to and from Germany (often having cars for Switzerland, and sometimes for Genoa or Ventimiglia), and once or twice father made a project with me for a day trip to Köln (Cologne). But nothing came from that. Even in The Netherlands I had seen rather little, some of the cities of our province, not far from Arnhem; we had been several times in Amsterdam and in The Hague, and in a few other places, but that was all.

Although I had read about Germany and its geology, and had been presented collections of rock specimens from the Eifel, from Thüringen, and from Switzerland, I had never seen a mountain and the highest point near Arnhem (about 110 m above sea level) had been my limit of altitude. Father and mother had made a trip along the Rhine to Frankfurt, with a side trip along the Mosel; and they had been to Paris and to Berlin. I believe all three trips were made upon the invitation of one of mother's cousins and his wife.

They told us often about these trips. In 1907 they made a trip to Düsseldorf to see an exhibition of industrial products and machinery. Mother told us about a huge wheel, which went backward and forward alternately and also about the adventures of the return trip; they had taken passage on one of the Rhine steamers (still operating) but owing to fog the boat was delayed very long and they had to sit on the quay for the whole night, and were happy when a booth went open in the morning for market people, where they could get some hot coffee. Their honeymoon trip had been to Brussels and some other cities in Belgium. I have the impression that father had some difficulties with hotels, owing to lack of experience, but they must have seen the famous "Rocher Bayard" near Dinant on the Meuse and the cave of Han on the Lesse.

In May 1914 I had to submit to the medical examination for the military service. Fortunately I was rejected on account of a too narrow measure of my chest. I am very grateful that this has saved me from passing tedious years without scientific and cultural contact in military garrisons, when the first world war broke out in August 1914 and The Netherlands army was mobilized to protect the frontiers (which remained necessary until the end of the war in November 1918). My brother, and I believe also the Jansen brothers, were rejected for their eyesight.

I would add that my education in matters of art was not extensive. We liked to go to concerts and father knew the music of many operas. Some of his friends were good musicians. But father, and even mother, had not much knowledge about paintings. We had been in the famous "Rijksmuseum" in Amsterdam, and in the "Mauritshuis" in Den Haag; but the classical paintings had made little impression upon me.

We knew a bit about classical sculpture; father had some good reproductions from the masterpieces in the Louvre Museum in Paris, and our classes in literature and in drawing at school gave attention to art; but it did not come to my heart. It is only later, under the influence of my first wife, that I began to develop more feelings in this direction.

There were some movie theaters in Arnhem to which we went rather often. The foremost of these theaters sometimes had a good scientific film as an item on the regular program. I remember one on the microscopic living world, which was presented very well both photographically and in the way it was arranged. It must have been a French film, in 1913 or 1914, which thus appeared many years earlier than the films which the German firm of Zeiss brought out, to be shown before scientific societies, some years after 1918. In this French film even the strange spherical colonies of the alga Volvox were shown. Father and I naturally were delighted to see the organisms we knew so well. I also remember a film of a valley in the French Alps, taken with a camera on the front of a locomotive with pictures of the villages and of the stations at which the train stopped.

Leiden (1914 - 1917)

After I had passed the examination in Latin and Greek in August 1914, father brought me to Leiden in the end of September to find a room (students boarded with private families, or lived in rented furnished rooms) and on October 1 I started as a student. In the home where I came to live there was in the first year also a Belgian physicist, Victor Counson, who was following courses in Leiden.

The war, which was relentlessly going on, naturally always was in the background of our thoughts. Newspaper information, as well as the mobilization of the army, and various measures which gradually became necessary to alleviate difficulties caused by scarcity of food, kept the war constantly before us. On very quiet nights (traffic had been cut down through rationing of gasoline) one could sometimes hear the gunfire in Flanders. When various revolutions started from 1917 onward, many of us were interested in their background and in the meaning they might have from the social point of view, whether they might bring improved conditions of living or not. But the study itself brought so many new things and new ideas that it was the university influence which holds the foreground. I could now absorb much more than before at home, and I did this with eagerness.

I had come to the University of Leiden with very little idea how it was operating. The only names known to us in Arnhem were those of H.A. Lorentz and of H. Kamerlingh Onnes; I may have heard the name of a professor of mathematics, and of one or two in the department of classical languages. I was befriended with a student of mathematics, A.C. Elsbach (died 1932), and I went to him for information. When I got a list of the courses and saw that Professor Lorentz would lecture at the Physical Laboratory (which I had noticed

on one of the streets), I decided at once that I should attend his course. Lorentz had left Leiden for a position at a private institution, "Teyler's Foundation", in Haarlem (I shall come back to this foundation later), in order to be relieved from the duties of a regular professor and to have full time for scientific work. He was, however, "extra-ordinary" professor in Leiden and came every Monday morning for a special lecture. Hence, on the first Monday in October I presented myself at the Physical Laboratory (the famous Cryogenic Laboratory of Kamerlingh Onnes), where I was received by a gentleman (I think it must have been Dr. C.A. Crommelin, chief physicist), who turned me over to one of the workshop boys, and the latter guided me through corridors with all kinds of complicated machinery and big bunches of pipe lines, through a small court, to a lecture room in a separate building at the back of the main laboratory. While waiting in the class room, I saw two gentlemen passing outside, and immediately realized that one of them must be professor Lorentz, since I had heard about the knobs on his forehead, which had developed as a result of his constant thinking. The other one, a dark looking man of somewhat smaller bodily stature, who accompanied Lorentz, later on appeared to be Professor Ehrenfest, his successor as ordinary professor of theoretical physics in the academic year 1914-1915. Lorentz lectured on "Interference and Diffraction of Light", a subject that I could follow without much difficulty, having read about it in an extensive Dutch textbook on optics while still being at home.

University study in Holland was, and still is, very free, much more free than it usually is in the United States. Once having paid the admission fee for the entire university one could go to lectures in any department one liked (or also stay away from anything that one did not like); it was not necessary to have an official adviser and ask for permission to follow this or that course, or to change one's plans. One could follow a course in ancient Egyptian, if one liked to do so, along with physics and mathematics. Every student at our universities is counted as a fully grownup person, who selects for himself.

I had physics from P. Ehrenfest, J.P. Kuenen, H.A. Lorentz and H. Kamerlingh Onnes; mathematics from J.C. Kluyver, later also from W. van der Woude; astronomy from E.F. van de Sande Bakhuyzen and W. de Sitter; geology from K. Martin.

The lecture room for theoretical physics at the same time served as a special library for mathematics and physics with complete sets of the Philosophical Magazine, the Annalen der Physik, the Physikalische Zeitschrift, etc., complete works of Cauchy and other mathematicians, of Stokes and of W. Thomson, and a lot more. For a small yearly sum one could become a member of the "Reading Room Bosscha", as it was called; it was not allowed to take books out (many of them were on deposit from the main library, and Ehrenfest insisted that all books should be always available in this room), but one could sit there and read and work. The number of students in physics and mathematics was small in those years. It was an extremely educative and stimulating situation; we always were there before class started and had books before us; usually we came also in the afternoon. In this way one became acquainted with the "founding fathers" of modern physics and mathematics, as well as with the current media of publication. Ehrenfest had been the originator and was the spirit of the "Reading Room" having brought over the idea from Göttingen. It has been in existence for many years, but I am sorry to have heard that a later professor of theoretical physics has stopped it. To us it has meant an opportunity, of which we have profited greatly.

The fact that I came to Lorentz' course drew the attention of Ehrenfest. Soon he invited me to a lecture which he gave before the Chemical Society on Bohr's model of the atom - the first time I heard about it ! - and then he invited me to attend his weekly colloquium, which was held at his house. At this colloquium the current literature in physics was reviewed and extensively discussed.

It is Ehrenfest who has had the greatest influence upon my development and who introduced me into the spirit of real scientific inquiry in physics. Quantum theory was then in its first stages; the theory of special relativity had obtained a definit form a few years before, but, as I gathered later, there was still much to do about the tensor which should represent matter; in the years 1915 and 1916 Einstein gave the final shape to his theory of gravitation. The work of James Frank and Gustav Hertz on the excitation of atoms by collisions with electrons accelerated through a definite potential (1916 or 1917), was one of the first striking demonstrations of the correctness of quantum theory. The mass spectrograph had been described in principle already in R.K. Duncan's "The New Knowledge", which had come out in 1908; now, at one of the first colloquia which I attended I heard about the discovery of isotopes.

Ehrenfest made us acquainted with all these subjects, and let us also share in the development of his own thinking. He often needed one or another of his students to talk about some new idea, as he found this helpful to clear up his own mind. So he told me about the development of Einstein's theory of the gravitational field in 1916. Einstein was accustomed to send copies of the proof sheets of his papers to Ehrenfest every time a further article was in the press, so that Ehrenfest received the news before it actually had come out. Ehrenfest told us also about the conceptual difficulties which he saw in many theoretical investigations. He brought me into contact with other physicists in The Netherlands, as well as with the few foreign guests who came to visit him (the war prevented normal international travel). I remember G. Breit and G. Nordström; the latter was in Leiden for a long time and married Miss C van Leeuwen, a student in physics somewhat older than I was. Einstein came in the summer of 1917; I remember that I was sitting with Gunnar Nordström in his room (opposite Ehrenfest's house), when Ehrenfest called at the window and told that Einstein had arrived, whereupon we joined in the reception. Niels Bohr came to Leiden not before 1919.

I also owe very much to a close friendship with three fellow students of that same period who had come to Leiden one or two years earlier: D. Coster (died 1950), H.A. Kramers (died 1951), both physicists: and D.J. Struik, mathematician. Much understanding for the meaning of mathematics was obtained from talking with them, in particular with D.J. Struik. It was Ehrenfest who had introduced me to them; I believe this was even the first time that he invited me to his home. How much was I impressed by his study, a large room with three windows in one of the walls, looking out on a part of the garden, and a large couch at the other wall, where I have been sitting so very often. On the shorter wall there was a large blackboard, a strange form of decoration even to me (it was in this room that also the colloquium met in those years). In the bookcases I saw many books, amongst them some Russian books, and I remember Hilton's "Mathematical Crystallography". There were pictures between the windows at the wall, Boltzmann, who had been Ehrenfest's teacher in Vienna, Maxwell, Waiter Ritz with whom Ehrenfest had had a close friendship, but who had died young: Tolstoy, Dostoyevsky (of whom I had never heard) and one or two views from the surroundings of St. Petersburg, from which Ehrenfest had pleasant recollections.

To me this room has been more impressive than the study rooms of any other scientist by whom I have been received. It had a simplicity and austerity, and for me it has a grandeur, since it speaks of the many chapters of modern physics which have been discussed in it, with Ehrenfest's colleagues as well as with the many, many visitors that have come to see him. The room has spoken to me the stronger since I was received there as a close friend, almost as a close relative while also my best friends were at home there. Soon I was also received in the dining room, which so often had full sunshine coming from the garden. Mrs. Ehrenfest had (and has) a very pleasant way of treating us, and of talking; she was almost as inquisitive as Ehrenfest was himself, and she often engaged us for work in the garden of which she was very fond.

All this deepened the impression I had from Ehrenfest's study room; I have found myself almost at home there as it had been in my father's house.

When I was in Holland in May 1961, my wife Anna and I visited Mrs. Ehrenfest, and we were again in the same room. I still am sensitive to its atmosphere and I miss Ehrenfest himself. There were many of the old photographs and of the books, which I had seen on my first visit in October 1914. If ever there would be some truth in the idea that walls do absorb something of the spirit that has reigned in a room, how much would these walls be able to give back!

I mentioned that Ehrenfest introduced me to some students in physics and mathematics, who became my closest friends. There existed in Leiden a "dispuut", a society of students interested in physics, mathematics, astronomy and chemistry, called "Christiaan Huygens", and I think that again it was Ehrenfest who mentioned it to me. Soon I was admitted as a member. The society met every fortnight, from 7 p.m. to about 11.30 p.m., sometimes followed by a nightly walk. There was a longer lecture and a shorter communication, given by members, and often also a jocular improvisation on an assigned subject. The longer lectures were always well-prepared and treated advanced topics taken from the field of study of the member who gave the talk. Often the talks went quite deep. Since questions and discussions were fully allowed and encouraged, the meetings were extremely stimulating. They usually were held in the room of one of the members, who had to arrange for tea and coffee, and for sweets, cookies, pies, etc. Coster, Kramers and Struik were members, as was also A.C. Elsbach. The first time, when I came as a guest, I listened to a talk by C. de Jong, on unsolved problems in astronomy. De Jong at that time was already working on a thesis connected with Kapteyn's theory of two-star streams. I also became great friends with Marcel Minnaert, who came from Belgium in 1915; he originally was a biologist, but now studied physics and was later to become the foremost

solar physicist in The Netherlands, as successor of W.H. Julius. There also were some female members, and it was there that somewhat later I found Jeannette Roosenschoon who came to Leiden in 1916 to study physics. She became my wife in 1919. (My second wife, whom I married two years later, was the sister of another member of Christiaan Huygens; she studied law. Thus all of us belonged to the same generation). A few times each year "Huygens" arranged walks or excursions; often Ehrenfest joined us. My father enjoyed very much becoming acquainted with the friends I had found in Leiden. Struik, Coster and Kramers also have been in the house in Arnhem. Kramers became very much befriended with my brother.

Ehrenfest taught us how to read scientific papers, to look for the assumptions made by the authors, and to hunt them out when they were not given explicitly. His powerful analytical mind opened our eyes to many subtleties in physical theory. He always strove to find interpretations of new thoughts, and had striking ways for the illustration of their peculiarities. His method of lecturing consequently was unique. He encompassed and taught theoretical physics as a whole, and in passing gave us insight into a good deal of mathematics bringing cross connections between domains which until then had looked as quite separate.

His regular courses were on Maxwell's theory and on statistical mechanics, but there were also special courses. A course on some aspects of colloid physics, developed into an exposé of the probability laws for radioactive phenomena. A course on certain parts of theoretical mechanics developed into a highly illuminating survey of the theory of integral equations, in which Ehrenfest gave us a thorough introduction into what later became known as the theory of "Hilbert space" (1915). Although Ehrenfest did not introduce the projection operator, the ideas gained from these lectures have helped me greatly when many years later I read Johann von Neumann's "Mathematische Grundlagen der Quantenmechanik". In a later year we had a seminar on linear partial differential equations of the second order, in which Ehrenfest stressed the relations

between the various types of equations and quadratic surfaces. I had to make many large scale sketches of various sets - of confocal quadric surfaces to illustrate the lectures (one of the sketches for many years has hung in the dining room of Ehrenfest's house). He often let us give parts of this course. It was still in my first year, that, on one of the rare occasions that he was not well, he asked Kramers and me to present Poynting's theorem on the flow of energy in the electromagnetic field. We were invited to his home the night before, in order to be properly briefed. I felt very glad to be asked to do this.

Ehrenfest gave constant food to my desire for understanding and he expanded the views and interests I had brought from home. No longer it was my father's maxims that took the first place in my thinking: Ehrenfest's influence became the stronger one. Ehrenfest was fond of music, and was a good piano player. Sometimes he played for us. It seems to have been Einstein who opened Ehrenfest's ears for Bach's Preludia and Fugues. Ehrenfest made me acquainted with several of them, which still are my favorites. I also remember one of Beethoven's "Bagatellen", which he often played. Once he had a guest staying for some days at his house, who played Beethoven's "Sonate für Hammerklavier" with its passionate and sombre "Adagio sostenuto". A few times Ehrenfest arranged for some students to play Bach's Fugues in such a way that each voice was performed by a separate instrument, so that one could follow the tunes more easily.

I also vividly remember that on a morning in the month of May, after a class, he took us to one of the buildings in which the Ethnographical Museum in Leiden was housed at that time: in the garden there was a set of five wonderful statues of the Buddha (still forming a famous treasure in the collections of the Museum, and now placed in a room of honor), standing under a magnolia tree in full flower, with bright sunshine over everything.
I mention these matters as examples of how much Ehrenfest contributed also to the development of our inner life. Ehrenfest, so to say, distributed all, that which was living and active in him. Sometimes it looked (I believe to see this now, from a distance), as if he gave away everything he had found or observed, without building up a reserve, a kind of stronghold, within himself.

I regret that I cannot give a picture of Ehrenfest as I could do of my father. He was, of course, much more complex than father was, and also much more complex than I am myself. His analytical mind stirred up everything, so that at times it looked as if nothing would be left as it was. On the long run this pushed his students somewhat away from him and I have also experienced this effect. There were things that we did not like to have analyzed. It may look as if this betrays a lack of intellectual interest, but in several cases it was an instinctive protective reaction from our side. I can enjoy myself with things or in situations without asking whether they have a meaning, whereas Ehrenfest would question every aspect. Ehrenfest had a great personal sensitivity, which I have not always understood. He had a hunger for friendship as if he could not find a sufficiently strong anchor within himself. There was some inner sadness in Ehrenfest, perhaps also a hidden fear, may be due to his Jewish origin.

I remember a meeting in the spring of 1918, which Ehrenfest had arranged to make physics teachers acquainted with recent discoveries. He asked some of us to give talks; I believe I spoke on the work of Franck and Hertz. This meeting naturally gave him great pleasure, but what was strange to us was that he said it had given him more pleasure than the birth of his youngest child in that same year. This surprised us greatly. I spoke about it with Lorentz and asked him whether he could talk with Ehrenfest, and help him to find a way back to feelings that looked more normal to us.

But even for Lorentz it was too difficult to penetrate into the deeper recesses of Ehrenfest's mind. None of us could reach deep enough, and each of us had already other problems before himself, which we wanted to consider and to keep for ourselves.

While we perceived that Ehrenfest's self analysis could take dangerous forms and lead to utter despair, we could not help him. At that time I had already become engaged with Jeannette Roosenschoon and, while it had been Ehrenfest who had helped me to become liberated from my father's world, the intimate exchange of ideas with her opened still another world for me. A world, it is true, not bringing the vistas of science, but bringing pictures of personal and social relations that were not less important for development. We married in the summer of 1919, and it was with my wife that I strove to build up my place in Delft (see later on), and to form a picture of the new aspects in which Europe was presenting after the war and the various revolutions. Then Ehrenfest's influence lost hold, in a similar way as it had happened with my father's influence a few years before. Moreover, the colleagues whom I found in Delft, in particular C.B. Biezeno, about whom I shall speak later, soon assumed an important part in my daily contacts.

It is probably also a result of a difference in mental attitude of more shallowness and a more formalistic attitude on my part in comparison with that of Ehrenfest, that I cannot give a good picture of what was in Ehrenfest's mind when we struggled with the mysteries of quantum theory. The problem of the adiabatic invariants was an important topic. Ehrenfest had the conviction that here was a domain where classical mechanics provided an inroad into the new theory, and he strove hard to grasp the meaning of those cases where unexpected relations presented themselves as, e.g., when the oscillations of a pendulum increase in amplitude beyond 1800 and pass into a cyclic motion. Another problem was how to count the various configurations of a mechanical system, say a gas with many molecules, so that the proper basis

is obtained for the calculation of the entropy, as the most obvious way of counting required a mysterious division by N! to give the proper result. I had to leave quantum theory behind me when I went to Delft to occupy myself with fluid mechanics. I have no direct knowledge of Ehrenfest's first reactions to the work of de Broglie, Schrödinger, Born, Heisenberg and P. Jordan. The discovery of the electron spin by Goudsmit and Uhlenbeck gave him great joy.

To come back to my story, the first important extension of Bohr's theory had come in 1916 through the work of Sommerfeld and Epstein on systems for which the Hamilton-Jacobi partial differential equation can be solved by the method of separation of variables. In view of the importance of adiabatic invariance, the question naturally turned up whether the quantities introduced by Epstein, the "phase integrals", would also be invariants. We were convinced that this should be the case, and I succeeded to prove this by the application of a set of transformations of partial derivatives. It was somewhat like solving a puzzle. A paper on this subject was accepted by Professor Lorentz for publication in the Proceedings of the Royal Netherlands Academy of Sciences (Ehrenfest became a member of the Academy in May 1919). After having given a proof for the general case without degeneration I could show that in the "degenerate case" the remaining independent phase-integrals still were invariants. Later I constructed a new proof with the aid of the transformation to action and angular variables, as used by Schwarzschild, and treated in ET Whittaker's "Analytical Dynamics" (this was in 1916-1917).

So much about Ehrenfest. I will now turn to some of my other professors. I must begin to say that with all my admiration for professor Lorentz (an admiration continually strengthened by Ehrenfest), his personal influence was much less. Lorentz had that natural modesty which is afraid to go too deeply into the mind of another person.



Figure 5 - From left to right: Father of Jan Burgers, Jan Burgers, Paul Ehrenfest, around 1917 (AIP Emilio Segrè Visual Archives, Ehrenfest Collection).

He remained much more aloof, and never subjected us to much questioning, nor pushed us to some topic as Ehrenfest could do. We revered Professor Lorentz, but the distance was too much for the development of the type of friendship we had with Ehrenfest. I came, however, into closer contact with Professor Lorentz during the period January-September 1918, when I was his assistant in Haarlem.

Professor Kamerlingh Onnes was a much more authoritative person. In the year 1914-1915 he still gave a course on the theory of the monocycle, as developed by Maxwell and by Helmholtz in view of a certain analogy with thermodynamic relations. We were with only three students, and once, when the two others had not turned up, he gave for me alone a private lesson on his investigations on superconductivity and on the fact that a magnetic field cannot penetrate into the interior of a superconductor. As I was interested in experimental physics, I also followed a course in glass blowing in the cryogenic laboratory, but I did not become an expert. Gradually I came to carry out measurements with electrical resistance thermometers, and from January 1, 1916 - December 31, 1917, I have been an assistant in this laboratory. My main work was to read the galvanometers and to help other students with electrical temperature determinations. Experiments with liquid helium could not be carried out during the war years, but in 1917 a vapor cryostat came into use, working in the domain between liquid neon and liquid hydrogen.

Kamerlingh Onnes wished to have his assistant completely for his laboratory who should do observations during daytime, work them out in the evening and write them up during the weekend and one should not flirt with theoretical studies, because experimental physics required the whole person. To this regime I could not subject myself. I was too much interested in theoretical problems and was too much attracted by Ehrenfest. As my mind was not very inventive, I could not arrive at a program for experimental research.

Gradually Kamerlingh Onnes noticed that I had strong attachments on the theoretical side; Ehrenfest talked with him about me, and finally Kamerlingh Onnes asked me about my plans. I told him that I felt too much attracted to theoretical studies, and I know that it was a great disappointment to him. He has taken it nobly and did not detract his friendship from me. I may mention in passing, that later on, when I accepted the position for fluid dynamics in Delft, Kamerlingh Onnes said that if ever I should like to do experiments on the viscosity of liquid helium he would be glad to have me come back to him. Unfortunately, at that time I looked upon viscosity only as a datum to be used in the calculation of the Reynolds number for a flow pattern, and thought that to measure the viscosity of helium would be just to add another number to a table of physical constants. From what has been found later, it may be that I have missed a great opportunity.

From my professor of mathematics, it was Kluyver from whom I received most, through his excellent lectures on the theory of functions. I remember with great admiration his course on the Riemann function, about which we read at the same time in the books we found in the "Reading Room". With van der Woude, who gave geometry and analytical mechanics, I have become much befriended, but I did not follow his lectures; he came a year or so later, and I had already studied much of these topics. It was to the astronomer W. de Sitter, who was an expert in Hamiltonian dynamics, that Ehrenfest took me when I had got the proof for the invariance of the phase integrals, to discuss the details.

I did the "candidaat examen" on May 22, 1915. It was Ehrenfest who had prompted me to do it within a year since I had already much preparation before I came to Leiden. The "doctoraal examen" followed on December 1, 1917. In October 1917 my brother had also come to Leiden to study chemistry. For some months we lived together with the same family where I had been since my arrival. My brother also became a member of the Christiaan Huygens society, which had more chemists among its members. But in January

1918 I left Leiden for Haarlem, as Professor Lorentz had asked me to accept the position of "conservator" of the Physical Laboratory of Teyler's Stichting, of which laboratory he was the director. My brother remained in Leiden until the summer of 1919, when Professor H.J. Backer asked him to become his assistant for organic chemistry at the University of Groningen. My brother's work there was interrupted by a stay of two years (1920-1921) in Rome as teacher of the sons of the Ambassador of The Netherlands for the mathematical and physical subjects. The present Ambassador of The Netherlands in the United States, his Excellency Mr. J.E. van Royen, thus has been a pupil of my brother. After having returned to Groningen, my brother in 1923 received a fellowship from the International Education Board to work on crystal structures at the Royal Institution in London under W.H. Bragg. This was followed in 1925 by a Ramsay Memorial Fellowship, and my brother continued to work at the Royal Institution until the summer of 1927, when he was offered a position at the Physical Laboratory of the Philips Factories in Eindhoven. In 1940 he was appointed professor of physical chemistry at the Technical University of Delft. Father has still seen that the two of us were professors at the same University.

To come back to Teyler's Foundation: this was an institution dating from the end of the 18th century, when there was everywhere a great interest in the physical and natural sciences. Among its early professors had been van Marum, who constructed a powerful electrical machine with which he performed many experiments. The Foundation also had a museum with rich collections of paintings and drawings; and a collection of physical instruments (among them van Marum's machine); minerals and fossils with a large slab containing the famous reptile "Andreas Scheuchzeri", which Scheuchzer had held to be a remnant of a man who had died with the Biblical Great Flood.

To work at the physical laboratory of this Foundation was a very honorable position. Two of my predecessors, Dr. G.J. Elias, and Dr. W.J. de Haas, (Lorentz' son-in-law) had carried out important experimental work at the laboratory. One after the other they had been appointed professor in Delft, Elias for electricity and Maxwell's theory; de Haas for physics. The daily contact with Lorentz was of great value, but nevertheless I felt somewhat lost in Haarlem, although I went weekly to Leiden with Lorentz on the occasion of his lectures. Now that I was on my own, I suffered from some inability to develop a full program of work for myself.

Before I really came to consider this matter seriously within myself, I received an invitation to accept a newly created chair at the Technical University in Delft for aero- and hydrodynamics, in the Department of Mechanical Engineering and Shipbuilding. The jump away from physics and from atomic problems was a large one, but there was also an attraction in the idea of starting a new line of work.

Hydrodynamics was not treated as a part of classical mechanics in Leiden. Ehrenfest had not much feeling for a domain of science which was governed by non-linear equations, although in 1917 he had directed our attention to a little book by R.. Gramm "Die Hydrodynamischen Grundlagen des Fluges" (The Hydrodynamical Foundations of Flight), in which the theory of two-dimensional circulatory flow around wing profiles was explained. This indicated an interesting field for the application of conformal transformation. In the "Reading Room" I had sometimes looked at F.W. Lanchester's "Aerodynamic Theory", but this made the impression of an incomprehensible phantasy. Hydrodynamics, as a part of theoretical physics, had a scientific ancestry of high standing, and the names of Lord Kelvin and Helmholtz are connected with many of its intriguing aspects. Even Professor Lorentz had written two important hydrodynamic papers, one on problems of viscous flow that have provided a basis for C.W. Oseen's theoretical investigations; and one on turbulence, explaining Reynolds' ideas and adding to them some very inspiring developments.

The invitation from the Technical University was made by two professors of the mentioned Department, professor C.B. Biezeno, who lectured on the theory of elasticity and lectured on strength of construction; and professor C.P. Holst, who lectured on construction of machinery, and who had a great interest and admiration, and I may say, a fine feeling for theoretical work. They explained to me that what they desired was a scientific attitude towards the subject. Although it was the time when flying and airplanes attracted more and more attention, it was not their idea that I should be an expert in flight as a technical achievement: I should have to bring the basic ideas necessary for understanding and mastering the phenomena of flow, and to work in this domain as a scientist. From the discussions it appeared that to cooperate with them, in particular with Biezeno, was very attractive and would protect me from being immersed in technical and industrial relations. In that period the mathematical methods used in hydrodynamics were still closely related to those applied in the theory of elasticity. The cooperation with Biezeno soon developed into a close friendship, and many are the discussions we have had together, not only on matters of scientific interest, but also on problems connected with the welfare of the Technical University, and on personal matters.

I had, of course, to learn hydrodynamics myself and I started to read various papers on vortex motion, among them Ahlborn's photographic work on vortex motion behind bodies which were towed through a large tank with water. I had still to finish my thesis work on "Het atoommodel van Rutherford-Bohr" (The Model of the Atom according to Rutherford and Bohr). It originally had been a prize essay for Teyler's Stichting, which I had written in 1917. H.A. Kramers had gone to Copenhagen, to work at Niels Bohr's Institute, in the second half of 1916 or the beginning of 1917, and he had not been aware that this theme had been set; otherwise he probably also would have written an essay. The point of view which I had

taken had grown out of the work on adiabatic invariants and was based upon a treatment of the equations of analytical dynamics with the aid of contact transformations, as indicated in E.T. Whittaker's "Analytical Dynamics". Professor Ehrenfest was my promotor, and the degree was awarded on December 12, 1918.

The appointment in Delft officially had started October 1, 1918, on which day I had also moved to Delft. On December 2, 1918, I gave my opening discourse with a lecture on "The Hydrodynamic Pressure".

After that I felt myself obliged to abandon the theoretical physics of atomic structure completely. Hydrodynamics needed all attention and it was not possible to serve two masters at the same time. Only after 1926 I made myself acquainted again with the new views on quantum theory, which had developed from the work by Born and Heisenberg and by Schrödinger.

As regards hydrodynamics, the theory of the circulatory flow around airfoil profiles required first attention. Soon we received information concerning Prandtl's work and that of his pupils Max Munk and Albert Betz on the vortex system behind airfoils of finite span. Also Prandtl's boundary-layer theory, first presented in 1904 and later worked out by Blasius (1908) and Hiemenz, had to be studied, as well as Osborne Reynolds' fundamental work on turbulence. The discussion of this work by Lorentz, who had also extended Reynolds' stability calculation, was extremely stimulating. A remark by one of my mathematical colleagues happened to make me acquainted with the work of Oseen - great application was needed to get into the meaning of his calculations. But in 1920 I began to see a relation between certain aspects of Oseen's work and Prandtl's boundary-layer theory, and I constructed an intermediate picture by making use of a transformation of the equations for two-dimensional flow, given by Boussinesq. It is convenient however, to stop at this point, since many new developments started with the year 1921, when I became acquainted with Dr. Th. von Kármán.



I.4 Some memories of early work in fluid mechanics at the Technical University of Delft

by J. M. Burgers, Institute for Fluid Dynamics and Applied Mathematics, University of Maryland, College Park, Maryland 20742. Reprinted with permission from Annu. Rev. Fluid Mech. 7, 1-12 (1975).

I have been asked to put down on paper some features of my early efforts in fluid dynamics. I will try to do so, but memories of days now long past have become vague in many respects; they may suffer from distortions, and my narrative will be rather spotty, with gaps, perhaps with inconsistencies. In particular, although I will mention several names to which are linked lasting ties of great friendship. I cannot render the atmosphere of personal relationship which I have experienced with colleagues, assistants, students, scientists from other countries, or with people from industry and technical enterprises; nor can I adequately describe the deep influences that contacts with men and women in all circles have on one's mind and one's "output." Scientific problems may come forward from things heard, or read in books and papers, and sometimes they seem to arise from nowhere, but the background of the entire society is always there and is effective. An important influence is the often felt need to reduce a scientific problem to its most essential and simple points, in order to make clear to others what can be done and what would be beyond reach; or to defend one's manner of thinking and one's way of approach. Looking back, I may even say that the major part of my scientific work has been directed toward interpretation, more than to finding new results, although interpretation often opens the mind for a new view. (Lorentz and Ehrenfest, to whom I owe very much, were both, each in his own way, great interpreters.) Finally, I would add that, apart from my interest in physics and mathematics, I have many interests in other matters, biology, geology and mineralogy, history, philosophy, and generally in all things beautiful. These interests continue to go round and round through my mind and often mix themselves with mathematical work. Also they are deeply influenced by personal contacts.

I studied theoretical physics at the University of Leiden (1914-1918), where my main teacher was Professor Ehrenfest, although I owe much of my development also to Lorentz, Kamerlingh Onnes, and other professors, as well as to a group of students into which I was taken up. But it was Ehrenfest's interest and friendship that has had great influence upon my attitude with respect to science, and with respect to physics in the first place. While other important influences came after my student years, that of Ehrenfest made the first great impression upon me next to that which had come from my parental home. My father had implanted in me the desire to know and to understand many aspects of the natural world around us, ranging from elementary physics and astronomy to geology and biology. My teachers at the secondary school in our hometown extended my domain of interest, and lent me books to study calculus and other subjects. At home there was a cabinet of instruments for the demonstration of physical phenomena, many made by my father himself, who was an excellent teacher and demonstrator; there was a small collection of minerals and shells; there were microscopes for becoming acquainted with unicellular organisms, and there were many books. My brother, Professor W.G. Burgers, profited from that same background. It caused both of us to come to Leiden with many interests, and in some measure already prepared for what the University could offer to a student.

Following a central interest of those years, my thesis work was on the model of the atom, proposed by Rutherford and by Bohr. I cannot dwell here upon those years of introduction into the world of science. But after having completed the thesis work, I became somewhat afraid of having insufficient phantasy for making fruitful advances in Bohr's theory. I had already a position as a physicist, working under H.A. Lorentz in Haariem, but when a development at the Technical University in Delft suddenly opened a quite unexpected perspective in another direction, I felt a great attraction to change to a new subject. The point was that

the professors of the Department of Mechanical Engineering and Shipbuilding of the Technical University had become convinced that it was necessary for their work and their teaching to have someone with them who could occupy himself with fluid mechanics, and 'who could lecture on the essentials of that subject to their students. A chair for this purpose had been created in 1918. The Committee that was searching for candidates considered it desirable to look for a person with sufficient background in mathematics and its applications who would be prepared to build up the subject from its fundamentals.

Hydrodynamics, although it had a famous history abroad, had not at that time received much attention in the Netherlands. Some outstanding work was done by J.D. Korteweg, professor of mathematics in Amsterdam, after whom the Korteweg-de Vries equation has been named, and there were two fundamental papers by H.A. Lorentz. One of the latter treated basic solutions of the Navier-Stokes equations, corresponding to impressed point forces; and the other discussed the theory of turbulent fluid motion in which, among other matters, Lorentz improved Reynolds' estimate for the limit of stability of laminar motion as derived from an energy criterion, by introducing a particular type of elliptic vortex. On the whole, topics governed by nonlinear equations had not yet come into fashion.

I remember that in the library for theoretical physics at the University of Leiden there was a German translation (by the mathematician C. Runge) of F.W. Lanchester's Aerodynamic Theory. However, nobody in our circles at that time understood this book, which made the impression of an incomprehensible phantasy. An important event for some of us, however, was the publication in 1917 of a little book by R. Grammel, Hydrodynamische Grundlagen des Fluges, in which the theory of the circulatory flow around wing profiles, as developed by Kutta and by Joukowsky, was explained. This made us aware that theoretical methods of treatment, here the application of conformal transformation in a two-dimensional field, could be of help to

gain insight into a practical problem. It was Ehrenfest who had directed our attention to this book.

The invitation directed to me in March 1918 was made by two professors of the Department of Mechanical Engineering and Shipbuilding: Professor C.B. Biezeno, who lectured on the theory of elasticity and on strength of constructions; and Professor C.P. Holst, who gave construction of machinery and who, being in many respects a self-made man, had great interest in and a deep admiration for theoretical work. They explained to me (and to the other candidates who were approached) that what they desired was a scientific attitude towards the subject. Although it was a period in which flying and airplanes attracted more and more attention, it was not their idea that the man they sought should become an expert in flight as a technical achievement, and become immersed in technical and industrial relations. The new teacher should bring the basic ideas necessary for the understanding of flow phenomena, and should work in this domain as a scientist. From the discussions it appeared that to cooperate with them, in particular with Biezeno, would be very attractive and would open many perspectives. At that time the mathematical methods used in hydrodynamics were still closely related to those applied in the theory of elasticity. I accepted the nomination, and after some time I was appointed. Much later I heard the names of two other candidates who were approached by the Committee. Both of them had declined on the ground that they saw specific careers before them: Dr. G. Holst (a nephew of Professor C.P. Holst just mentioned), as director of the Physical Laboratory of the Philips' Factories; and Dr. F.A. Vening Meinesz, who made his great reputation through his work on gravity measurements at sea.

My work at the Technical University officially started in October 1918. I was still very young and had little experience in directing or giving guidance to other people. I myself had to grow up in my new surroundings.





It helped me that from early years I had always had an admiration for the profession of an engineer. I felt that I could not serve fluid dynamics and atomic physics simultaneously; so I abandoned the latter subject for a time, and only in 1926, when Schrödinger's work came out, did I start to read about the new relations then coming forward.

The cooperation with Biezeno, soon established, developed into a close friendship, and many are the discussions we have had, not only on matters of mathematical interest, but also on problems connected with the welfare of the Technical University and on other matters where we became aware of differences in thinking. However, the first problem before me was to get acquainted with the existing literature and experimental work. In technical respects I was helped by the circumstance that in the same year, 1918, the Dutch Government had created an Institute for Research in Aerial Navigation, located in Amsterdam. A fruitful cooperation developed with the scientific staff of this Institute, which on one hand helped me to see what was done in the world of aeronautics, and on the other hand relieved me of the necessity to move too far into technical matters. I was soon able to amass a fair quantity of books in the office made available to me. In a rather haphazard way I mention the publications of G. Eiffel in Paris (introduced into Holland by the engineer Albert Kapteyn); we became aware of Prandtl's work, done together with Munk and Betz, on the vortex systems behind airfoils of finite span (which constituted an important extension of the topic treated in Grammel's book). We also heard of Prandtl's boundary-layer theory. The Reports and Memoranda of the British Advisory Committee for Aeronautics could be obtained through commercial bookshops; contact obtained through Dr. G. Holst with Professor J.J. Ames in Baltimore, then chairman of the United States NACA (forerunner of NASA), made available to us the Technical Notes of that Committee. We had the six issues of the "Bulletin de l'Institut Aerodynamique de Koutchino" from a private laboratory created

before 1906 by D. Riabouchinsky near Moscow. When Riabouchinsky lived in Paris as an émigré from Russia he visited Delft in 1919 or 1920. (On my second visit to Russia in 1929, I was shown his laboratory, now state property.) We received the publications of Prandtl's laboratory, the "Ergebnisse der Aerodynamischen Versuchsanstalt in Göttingen." Very important was that in 1921 R. von Mises started the "Zeitschrift für angewandte Mathematik und Mechanik," and as I will indicate below, the year 1921 was highly fruitful for me in many respects. Collections of data on the lift and drag coefficients of airfoils formed an important part of our library, and Prandtl's theory of induced resistance helped to understand them.

Also the problems of airplane motion needed attention, and L. Bairstow's Applied Aerodynamics was of great use. A part of my lectures had to be devoted to this subject. A paper by S. Brodetzky on the graphical integration of certain differential equations helped me to understand Lanchester's "phugoid theory," and I played with small models exhibiting features of phugoids. Moreover we had the collected works of Osborne Reynolds, which I liked to read (and although this did not relate to fluid mechanics, I have several times attempted to study parts of the third volume, on a theory of the Universe, based upon Reynolds' work concerning dilatancy). Some books on bird flight were also present.

Nor should I forget the importance for me of Oseen's work on his particular type of solutions of the Navier-Stokes equations, which I learned mostly through papers in the "Arkiv for Matematik, Astronomi och Fysik." (Oseen's book, Neuere Methoden und Ergebnisse in der Hydrodynamik, came out in 1927; it still makes a monumental impression.)

Conformal mapping, as a means for obtaining contours of airfoils and of propeller blades, for a long time was a dominant subject for me. I was particularly interested in looking for the simplest formulations

to be used in my lecture course. But it was also evident that everywhere in fluid dynamics attention should be given to vortex motion. Far more knowledge was required than was given in H. Lamb's Hydrodynamics, notwithstanding the importance of Lamb's work. I must observe that all my interest in that period was concentrated on incompressible flow; compressible flow came into view only much later. I began to see that transport of vortex motion, partly by convection through the general flow field and partly by diffusion as a result of viscosity, was of decisive importance in many cases, and I formulated a relation between the resistance experienced by a body (both as a result of pressure forces and of the part produced by viscous friction) and the momentum or impulse of the vortex system generated. Some years later (about 1924) I obtained a relation between the change produced in the Bernoulli quantity occurring in rotating machinery (propeller, centrifugal pump, ventilator, turbine, or windmill) and the total transport of circulation carried by the rotating blades plus the transport of vorticity. A publication of 1920, in which patterns of flow around a body were discussed as resulting from the interplay (or "competition") between convection of vorticity by the mean flow on one hand and diffusion of vorticity on the other, had helped me to see the meaning of Oseen's theory of flow around a body, with its unexpected sheets of discontinuity, as a special case of a more general problem. In Oseen's picture the convection was brought about by uniform rectilinear flow parallel to the axis of the field, but it appeared to be just as possible to introduce convection by another type of flow, in particular by the Dirichlet flow of an ideal fluid around a body. In the case of a two-dimensional symmetric field an approximate solution could be obtained making use of equations given by Boussinesg, which for vanishing viscosity (infinite Reynolds number, as was Oseen's limiting case) led to boundary-layer flow all along the body, with a singular "spur" of concentrated vorticity on the axis of the field downstream. This took away the strangeness of Oseen's solution and gave it a place as an instance of a method of treatment with wider possibilities.

In subsequent years I continued to work upon Oseen's approximation and its relation to Prandtl's theory. It appeared that Oseen's equations for the flow called forth by exterior forces acting on a fluid could be used for the description of the vortex system produced by a lifting system, and so gave a direct connection with Prandtl's theory of the finite wing. One of the publications that came forward from this idea was my part in the second volume of Durand's Aerodynamic Theory (von Kármán being the other author); here I used Oseen's concept as a starting point for many developments. Later I used Oseen's equations for the calculation of the resistance experienced by small particles in slow motion, at Reynolds numbers far below unity.

In the meantime other directions of research had come up, and I must mention something of our attempts in experimental work. A small but convenient laboratory had been built for me in 1920 and became available for work in 1921. One part of the equipment was a small towing tank (8 x 1 x 0.8 m), of the type used by Ahlborn, whose flow pictures had attracted much attention a few years earlier and had demonstrated the production of vortices in all types of real flow, as opposed to ideal non-viscous flow. Many flow pictures were made, and the tank could be used as a welcome demonstration instrument to make my lectures for the students more lively. The other main part of the equipment, built in 1921, was a small wind tunnel of the Eiffel type, with a working section of 4 x 0.8 x 0.8 m and free return of the air through the room. (Heating in winter was provided by water pipes that the builder had put at a height of several meters, so that usually they gave more heat to the roof than to the room itself; gently turning on the wind-tunnel fan was then a great help to make life more comfortable.)

In 1919 or 1920 I had read L.V. King's paper "On the convection of heat from small cylinders in a stream of fluid," which became the basic paper for the evolution of hot-wire anemometry. After some

unsuccessful experiments in water, I began to dabble with the idea of using hot wires in the wind tunnel. where the difficulties brought about by the wetness of water would be completely absent. I now come back to the importance of the year 1921. I have already mentioned that von Mises' Zeitschrift had started to appear in 1921, and in an issue dedicated to the "Naturforschertagung" to be held that same year in Jena there appeared four papers by von Kármán and his assistants on the following topics: the momentum equation for boundary-layer flow; practical solutions of that equation; the similarity law for turbulence and the deduction of the 1/7th-power law from the Blasius resistance formula for pipe flow; and the relation between turbulent resistance and heat transfer. These important papers opened new avenues in fluid dynamics. Moreover, and most stimulating for me, von Kármán, who recently had been appointed to the chair of fluid mechanics at the Technical University of Aachen, wrote me to suggest that I visit him. This I did in July 1921, and out of that first visit there developed not only insight into the interest of the new ideas fostered by von Kármán, but also a lively and very personal friendship, which lasted as long as von Kármán lived and which has been of the greatest meaning for me. In subsequent years I stayed several times at the apartment where he and his sister Josephine lived in Vaals, a small city of the Netherlands, close to the border and to Aachen. I made the acquaintance of his sister and also of his mother. To talk with von Kármán was always a great pleasure, and in many topics we needed only a few words to follow each other's thoughts. I went to the Naturforschertagung of 1921, and on these trips I made the personal acquaintance of several scientists working in fluid mechanics: Ludwig Hopf, the mathematician Otto Blumenthal, Erich Trefftz, L. Prandtl, C. Wieselsberger, R. von Mises, R. Courant, and others. I also met James Frank, the physicist, and was introduced to Felix Klein.

The interest generated in boundary layers, combined with the idea that hot-wire anemometers could help in their investigation, together with the availability of our convenient wind tunnel, made me suggest to the assistant of the laboratory, Ir. B. G. van der Hegge Zijnen, a very able cooperator, that he should start work



Figure 6 - Laboratory for Aero- and Hydrodynamics around 1930. From left to right: Bolsterlee, Van der Hegge Zijnen, Burgers.

on the distribution of the mean velocity in the boundary layer along a smooth wall. A glass plate, 167.5 cm long, 40 cm in breadth, and 1.2 cm thick was provided with a special smooth entrance to prevent untimely separation at the leading edge. This entrance edge was obtained by grinding and polishing the first 10 cm of the plate, a process carried out in our workshop with infinite patience. Wires for measuring the air velocity in the boundary layer were made from platinum or platinum-iridium; they had a length of about 2 cm and diameters of about 0.1 mm, later going down to 0.05 mm for Pt and 0.015 mm for Pt-Ir. The method of constant resistance (constant wire temperature) was used, current strength serving as indicator of the air speed, using a Wheatstone bridge or Thomson-bridge arrangement with a slow galvanometer. The wires were calibrated outside the boundary layer against a Pitot tube. An empirical correction was used when the wires came so close to the plate that there was some direct heat loss to the latter. The results obtained by van der Hegge Zijnen formed an important achievement in the early experimental investigation of boundarylayer flow. They brought to light the simultaneous presence of a laminar part of the boundary layer, with the Blasius velocity curve, and a turbulent part, with von Kármán's 1/7-power law, the latter part being situated downstream from the former. It gave a value for the Reynolds number at which transition occurred, which came to about 300,000 when based upon the distance from the leading edge, or about 3000 when based upon boundary-layer thickness. There was a clear indication of the influence upon transition of the degree of turbulence of the wind-tunnel flow outside the boundary layer. This work was carried out in 1923, and van der Hegge Zijnen obtained the degree of Doctor of Technical Sciences in 1924.

While van der Hegge Zijnen later worked with the mean flow along rough surfaces, I started (in 1925) to apply hot-wire anemometry to the study of (what we now would call "slow") velocity fluctuations. I found an equation for the relaxation time of a wire carrying a constant electric current, and I estimated that a Pt-Ir

wire of 0.015 mm diameter in an airflow of 8 m/s would have a relaxation time of about 1/350 s (later I heard that Professor Huguenard of the Conservatoire National des Arts et Métiers in Paris had derived a similar result for a more special case). At that time I had available torsion-string galvanometers with a period of about 1/50 s.

Observations were made on the correlation between the velocity fluctuations at different points. An anemometer with two parallel wires, which could be set at various distances from one another, gave information concerning the correlation between velocity fluctuations at points having the same x- and z-coordinates but different y-coordinates (different distances from the wall along which the boundary-layer flow dcveloped). Another instrument, with a single wire of about 4 cm length, on which six potentiometer wires had been welded, could be used for observing the correlation between velocity fluctuations at points having the same x- and y-coordinates, but differing in the z-coordinates. It was noticed that the z-correlation increased when the wire came close to the wall, which gave some evidence concerning the extension of vortices in a direction perpendicular to the flow and parallel to the wall.

I also constructed an instrument with two parallel wires very close to one another, to make simultaneous observations of the velocity and the direction of the flow. The instrument used the two halves of a loop of a single wire (0.015 mm diameter), stretched by a thicker wire (0.05 mm diameter, serving at the same time as a potentiometer lead), while the two free ends of the loop were soldered to two needle points brought very close together but not touching one another, and keeping the two wire halves parallel to each other at a distance of 0.05 mm. The plane of the loop (of the two wire halves) was adjusted to be perpendicular to the direction of the mean air flow. Fluctuations in the total resistance of the loop gave an indication concerning fluctuations in the absolute velocity of the air; fluctuations in the difference in

resistance between the two halves provided an indication of the direction of the actual flow, because with flow at a small inclination one wire half was more exposed than the other. The instrument was sensitive to directional changes of less than a degree. Using a double-bridge arrangement the variations of the sum and of the difference of the resistances could be observed simultaneously. The setting up of the wires and the adjustment of the instrument, however, was a very tedious affair, since small deviations from symmetry could cause spurious indications. Moreover, the chance of breaking the wire, for instance just after it had been calibrated, was large. I had been spending the late hours of many days at the laboratory to get some results with this instrument, and an interesting case of correlation between velocity and directional variations was recorded in the wake behind a thin flat plate. However, I finally gave up putting time into an instrument that, notwithstanding certain promises, brought so many possibilities for mechanical failures.

It had appeared in the meantime that in order to get further results, one should not rely upon galvanometers working in a simple Wheatstone bridge arrangement, but amplifier systems connected with much faster working electronic registration instruments should be used. Around 1928, another assistant of the laboratory, M. Ziegler, started to construct such systems, which enabled him to work with much thinner wires operating with weaker currents. The development of the instrumentation took much time, and when after five years Ziegler left the laboratory to accept a position at the Philips Physical Laboratory, he had not yet arrived at a far-reaching investigation of turbulent flow. Nevertheless, when making oscillographic records of the velocity fluctuations in the boundary layer along a glass plate, he again found that the boundary layer can be steady and laminar over a certain distance, while in the region of transition this laminar flow appeared to be interrupted at irregular intervals by short periods of complete turbulence.

These periods were found to grow in duration and number as one goes downstream. Thus Ziegler observed the intermittency of incipient turbulence.

After the Second World War, hot-wire anemometry with the application of electronic amplifier systems was taken up at Delft by Dr. R. Betchov, who later on went to the U.S. I must leave this aside, however, and now move to another part of my work.

With the many references to turbulence everywhere in the literature, von Kármán's stimulating ideas, and the availability of Lorentz's work and of Reynolds' scientific papers, combined with my education in statistical mechanics when a student at the University of Leiden, it will be understandable that theoretical problems connected with the turbulence phenomenon began to occupy a part of my thoughts. In 1923 I attempted to construct a theoretical model for turbulent flow between two parallel walls, in which an assumed distribution of shearing forces together with a distribution of viscous dissipation was introduced, based upon a kind of superposition of many of Lorentz's vortices. The model could be arranged in either of two ways: it could give a resistance proportional to the 1½-power of the mean flow velocity (in this case it had a uniform distribution over the channel breadth of vortices all of the same size), or it could give a resistance proportional to the square of the flow velocity (vortices of various sizes were then used, from the breadth of the channel down to a certain minimum, all put against the wall). As Blasius's law for pipe and channel flow said that the resistance should be proportional to the 7/4-power of the velocity, an intermediate model would be needed. It looked as if this could be obtained by introducing some randomness in the arrangement of the vortices, but no appropriate solution was found. What stuck in me was an idea about the importance of the dissipation condition for turbulent channel flow: all the energy put into the system by the pressure difference driving the mean flow should be dissipated, for a (small) part in the viscous dissipation

associated with the mean flow, and for the larger part in dissipation connected with the turbulent vortex system. For several years I played with the hypothesis that a statistical theory of turbulence might be built upon the example of the statistical theory used in the kinetic theory of gases or in other conservative systems, provided the condition of constant energy content was replaced by a dissipation condition. It became clear, however, that this method would lead to "equipartition of dissipation" for all degrees of freedom of the system, and as there is an infinite number of degrees of freedom so long as one keeps to the picture that the fluid is a continuum, there is the danger of infinite total dissipation. This was an analogue of the difficulty of classical statistical mechanics for conservative non-dissipating systems, which allotted the same average energy to every degree of freedom. In classical statistical mechanics the rescue came from the introduction of quantum theory, but there is no indication that the introduction of Planck's constant will be useful in a theory of turbulence, which must be valid alike for all types of fluids, whether continuous or endowed with a molecular structure.

This brought the conviction that dissipative systems are essentially different from conservative systems. I thought that it would be necessary, therefore, to study the behavior of dissipative systems, and that since the Navier-Stokes equations are so refractory, it might be helpful to replace them by a more elementary equation. It was then that I took as example the equation

$$\partial v / \partial t = Uv + v \partial^2 v / \partial y^2 - 2v (\partial v / \partial y)$$

and I prepared an extensive investigation of this and a few similar equations in a paper published in 1939, "Mathematical examples illustrating relations occurring in the theory of turbulent fluid motion." Later the equation was still further simplified and took the form of what now often is called the Burgers equation. Of great interest appeared to be a class of asymptotic solutions, and a particular basis could be found on which to build the statistical features of the system. On this topic I have continued to work at the Institute for Fluid Dynamics and Applied Mathematics of the University of Maryland. (At the Institute also a topic of a different nature attracted my attention: the relation between the Boltzmann equation for gaseous systems and the equations of fluid dynamics; this also led to interest in the properties of shock waves.)

Let me enumerate in rapid succession a few more points of the older history. I mentioned the personal contact with von Kármán, which started in 1921. Von Kármán, with the help of Professor T. Levi-Civita of Rome, organized the first international conference on hydro- and aerodynamics at Innsbruck in 1922, which attracted a delegation from the Netherlands (von Baumhauer, Koning, and their wives, Pigeaud, Thysse, van der Hegge Zijnen, and myself). It was there that I met C.W. Oseen (with whom there developed some strong personal ties) and Levi-Civita.

From discussions with von Kármán after this conference arose the idea that a larger international congress might be convened at Delft in 1924, and Biezeno and I, with two other colleagues, undertook the necessary preparations. Many difficulties had to be overcome in connection with the then existing poverty in several countries. In a visit we made to the German Embassy at The Hague, shortly before the date set for the Congress, Biezeno, as president, had to argue extensively with the Ambassador in order to get scientists coming to the Congress relieved from a newly instituted travelers' tax in Germany (sportsmen were free from this tax). The difficulty of obtaining cooperation between French and German scientists was still felt

at this first congress, but it was overcome at the second one, at Zürich in 1926, through the efforts of its president, Professor E. Meissner. At Delft, however, we were lucky to have six Russian scientists, four having come directly from the USSR, A.F. Joffe, A.A. Friedmann, A. Galerkin, E. Nicolai; and two coming from offices in London, A.N. Kryloff and Mrs. A.B. Foehringer. (After Friedmann's death in 1925 Joffe invited me to visit him in Russia - from this resulted trips in 1926, 1929, 1930, and 1936, which among other things brought much participation in Russian family life.) The Delft Congress was a great success, as have been all later congresses. It was also at the Delft Congress that for the first time I personally met Geoffrey Taylor, who came with a large collection of instruments for demonstrating motions in rotating fluids, in which he was assisted by Constance Elam (later Constance Tipper). Other names which come to my mind are those of L. Bairstow, R.V. Southwell, B. Melvill Jones, W.S. Farren, A.A. Griffith, and B. Lockspeiser (many of whom came to high positions in the Second World War). The acquaintance with Sydney Goldstein came somewhat later; I believe our first meeting was at a conference arranged in 1929 by von Kárman in Aachen, on the occasion of the opening of a new laboratory. Later we corresponded, and we met again in Cambridge at the International Congress for Applied Mechanics of 1934. I should also mention the name of H. Glauert, whose book on Aerofoil and Airscrew Theory was of great use, who was at the Aachen meeting. Moreover, to the 1929 meeting came W.F. Durand, from Stanford University, with his plans for an extensive book on Aerodynamic Theory, and he invited various scientists to write parts of this book. I was asked together with von Kármán to prepare the second volume, which resulted in an invitation to come to Stanford University during the first three months of 1931. At the 1929 meeting van der Hegge Zijnen and I presented to von Kármám a calibrated "Pitot sphere," with five openings, which permitted the simultaneous measurement of flow velocity, flow direction, and static pressure at an arbitrary point in a flow field.

From France I knew first professors A. Toussaint, J. Kampé de Feriet, and H. Villat, and later, after the Second World War, Professor J. Pérès and many others.

There are many more names, not all of which I can retrace and put in proper order.

There were many connections in The Netherlands. I appreciated the opportunity to work at the Technical University, where so many of the professors were engineers, often with important jobs in industry behind them, from whom I learned the importance of economic problems on one hand and of possibilities for construction on the other hand. I mentioned the cooperation with the Government Research Laboratory on Aeronautics at Amsterdam, in particular with the deputy director, C. Koning (later full director). Then there was that with J.Th. Thysse, who, as secretary of a committee for the study of tidal motions and storm dangers connected with the Zuiderzee project, working under H.A. Lorentz, has made extensive measurements of flow systems and resistances in the shallow seas around Holland that would be affected by the project. Later Thysse directed a laboratory in Delft for hydraulic investigations. At Wageningen there was created a towing tank for investigations on the flow around ships and on ship resistance, under the direction of L. Troost. During several years Koning, Thysse, Troost, and I held regular informal meetings to tell one another about our problems.

From time to time I was invited to do some research for industrial projects. Among other relations there was a close cooperation with chief engineer Th.W. Theunissen from the construction firm "Werkspoor" in Amsterdam, on problems concerning centrifugal pumps and ventilators. I must mention here also the name of F.K.Th. van Iterson, one of the Directors of the State Mines. Our laboratory was further consulted in the problems connected with the ventilation of the Maas tunnel in Rotterdam, and model experiments were

set up for measuring the resistance of the projected canalization and the ventilation slits. In much of this work van der Hegge Zijnen (then "Chief Assistant") took a large part. Understandably, living and working in Holland, we were consulted in problems of windmills.

Another track was followed by my work as secretary of a committee for the study of viscosity and plasticity, created by the Royal Netherlands Academy of Sciences at Amsterdam in 1934. This led to a deepening interest in the phenomena of plastic flow, interest in which had already been aroused through contact with Biezeno, and through work done by Taylor and others, It brought collaboration with my brother, WveG. Burgers, on the subject of dislocations in crystal lattices. At that time only Taylor's "edge dislocation" had been considered, and we pointed out that there should be a counterpart in the form of "screw dislocations." This work also brought many international contacts.

However, I think it fit to break off here. I have been fortunate in having been involved in many interesting problems, and in having benefited from the friendship of many great persons. I hope that from my side I may have been of help to demand to others in understanding features of fluid mechanics, and perhaps sometimes also in other matters. I ended my position at the Delft Technical University on October 31, 1955, as a result of an invitation to accept a position at the Institute for Fluid Dynamics and Applied Mathematics of the University of Maryland, which had been extended to me by the Director of the Institute, Professor Monroe H. Martin. I have been there since November 15, 1955, and am now in the position of Research Professor Emeritus.

PART II - JAN BURGERS AND THE UNITED STATES

by J.V. Sengers and G. Ooms

After an impressive career in fluid mechanics in The Netherlands, Jan Burgers became a Research Professor at the Institute for Fluid Dynamics and Applied Mathematics (IFDAM) of the University of Maryland in College Park, MD in 1955. In this part we focus on the activities of Burgers in the US in general and at the University of Maryland in particular.

II.1 Contacts with the US

Jan Burgers had an international outlook dating back already from his time at the University of Leiden under Ehrenfest. The early work of Burgers in fluid dynamics after his appointment in Delft received the attention of Theodore von Kármán, who had become a professor of mechanics and aerodynamics at the Technical University in Aachen, and who invited Burgers to visit him in Aachen in 1921. This meeting started a strong professional and personal friendship of Burgers with von Kármán, which continued after von Kármán went to the California Institute of Technology in Pasadena, CA in 1930. Within one or two decades the reputation of Burgers had already been recognized internationally. W.F. Durand, who had met Burgers in Aachen in 1929

at the opening of a new laboratory of von Kármán, considered von Kármán, Prandtl, Taylor and Burgers as the "big four' in aerodynamics at the time [1]. In a chapter on Taylor's foreign peers in mechanics, G.K. Batchelor [9] wrote "It is generally accepted that there were three giants in mechanics during the first half of the century: Theodore von Kármán, Ludwig Prandtl, and G.I. Taylor. Another prominent contributor to research in various aspects of mechanics in the thirties was Johannes Burgers, a younger man." John von Neumann in a 1949 review of recent theories in turbulence listed the work of J.M. Burgers and that of G.I. Taylor, G.K. Batchelor, Th. von Kármán and L. Howarth as the most important statistically oriented modern theories of turbulence prior to World War II, with a later footnote about the additional importance of subsequent work of Burgers in 1950 [10].

The first visit of Burgers to the US occurred in early 1931. Durand had asked von Kármán and Burgers to contribute a volume in a series on Aerodynamic Theory [11] and had invited Burgers to visit Stanford University in Palo Alto for this purpose. Together with his wife, Jeannette (Nettie) Roosenschoon, Burgers spent four months in California including three weeks with von Kármán at Caltech. In 1938, Burgers, with his colleague, C.B. Biezeno, planned to attend the Vth International Congress of Applied Mechanics in Cambridge, MA. However, upon his arrival in Baltimore, Burgers received a telegram that his wife, Nettie Roosenschoon, was seriously ill and he had to return home immediately. Mrs. Burgers died of cancer on August 5 1939.

Contacts between Burgers and the US were interrupted during the Nazi occupation of The Netherlands from 1940 till 1945. After World War II, Burgers was invited in 1949 to participate in a dedication ceremony for the new supersonic wind tunnels at the Naval Ordnance Laboratory (NOL) in White Oak, MD and he also started to work as a consultant for NOL. It may be noted that at about the same time NOL was developing a

close interaction with science in The Netherlands leading to the establishment of the Institute for Molecular Physics at the University of Maryland under A. Michels from the University of Amsterdam in 1952 [12]. During a short visit to Caltech, Burgers accepted an invitation to spend six months at Caltech during the 1950-1951 academic year. For this purpose Burgers travelled to the US in 1950 with his second wife, Anna M. Verhoeven, who he had married in 1941. Prior to their arrival in Pasadena, Burgers first lectured at Cornell University, attended the International Congress of Mathematics in Cambridge, MA, participated in a Symposium on Plasticity at Brown University, attended a Symposium on the motion of the crust of the earth in Washington, DC, revisited NOL in White Oak, MD as a consultant, lectured at Brooklyn Polytechnic Institute and the Johns Hopkins University, went to Dayton OH for some business at the Central Air Documents Office concerning printing of the proceedings of a symposium on Cosmic Aerodynamics, lectured at Indiana State University, lowa State University, and at the University of Utah, which illustrates how well known Burgers had become in the US. During his stay in Pasadena, Burgers not only worked at the Hydrodynamics Laboratory at Caltech, but also lectured at the Naval Ordnance Test Station in Inyokem, CA, at Stanford University, and at the University of California in Berkeley, CA.

After concluding his stay in Pasadena at the end of the 1951 spring semester, Burgers participated in the first US National Congress for Applied Mechanics in Chicago. It was at this congress that Burgers was approached about the possibility of coming to the University of Maryland. After the meeting in Chicago, Burgers gave some lectures at the University of Maryland. He concluded his 1950-1951 stay in the US with professional visits to Cornell University, the Johns Hopkins University, NASA at Langley Field, VA, and the National Bureau of Standards in Washington DC. Shortly after his return to The Netherlands, he received a formal offer for an appointment at the University of Maryland as a Visiting Research Professor in the Institute

for Fluid Dynamics and Applied Mathematics (IFDAM) from its Acting Director, Raymond J. Seeger. IFDAM had been established in 1949 and, prior to his appointment as Acting Director, Seeger had been Chief of the Mechanics Division at NOL in White Oak, MD, while Zaka I. Slawski of NOL had served as the host of Burgers' visit to Maryland.

Figure 7 - Visa application of Anna M. Burgers-Verhoeven submitted on 25 October 1951, but returned stamped with "Visa refused under authority of Immigration Act of 1924", signed by the Vice Consul of the USA in Rotterdam.

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This illustrates, as in the case of the appointment of A. Michels at the University of Maryland [12], that NOL had played a role in trying to get Burgers to join the University of Maryland. Burgers had become enthralled with the stimulating research atmosphere in the US and he gladly accepted. In view of the positive reaction of Burgers, he subsequently received an offer for a permanent position effective September 1 1952 from Monroe H. Martin, the first Director of IFDAM, who also was a consultant at NOL.

II.2 Immigration problems

On October 25 1951 Jan Burgers and his wife Anna applied for immigration visas at the American Consulate at Rotterdam but their applications were returned with a stamped message "Visa refused under authority of Immigration Act of 1924".

To understand the origin of the problem we need to go back to the time that Burgers was a young graduate student with Paul Ehrenfest in Leiden [7, 8]. Ehrenfest, born in Vienna, had married the Ukrainian and Russian educated Tatiana Afanasyeva. After working for some time in St. Petersburg, where the couple became friends with the physicist Abram Joffe, Ehrenfest had been appointed as professor of theoretical physics in Leiden in 1921. However, Ehrenfest continued to keep close contacts with Russia. After the Bolsjevik revolution, Ehrenfest shared with some intellectuals at the time the hope that through socialism a new culture could be realized with a close harmony between science and society. This inspired some of the students in Leiden, like Dirk Struik, who later would become professor of mathematics at MIT, Jan Tinbergen, who later would win a Nobel Prize in economics, and Jan Burgers [13]. In 1918, the Communist Party of Holland (CPH) was formed and Burgers joined the group of intellectuals involved in this party. Around 1930, Burgers became disenchanted with the CPH because of the undemocratic Soviet influences,

which had penetrated the CPH and he formally resigned as a member of the CPH in 1933. Prior to 1951, Burgers did not have any problems visiting the US, simply because this issue had not arisen. In the words of Burgers in a statement dated August 4, 1952: "Since the circumstances, prevailing in the Communist Party in Holland in the period I was a member of it, were so different from what is now the case in the communist movement, I thought it to be most near the truth not to speak about opinions and interests which I have left behind me already for more than twenty years, long before the second world war broke out. I have never been a member of a communist party organized on modern lines and I have never been a member of any sympathizing organization." However, in the early 1950's, the US had entered the infamous McCarthy period and an explanation of the early affiliation of Burgers with the CPH could no longer be avoided.

The first sign of a potential problem had already occurred in July 1951. Burgers had received an invitation from the Aberdeen Proving Grounds in Maryland to visit the Ballistic Research Laboratories for the purpose of delivering a series of lectures on "Non-Uniform One-Dimensional Compressible Fluid Flow" on or about July 27 1951. However, when Burgers was about to go to Aberdeen, he received first a telephone message on July 24 and subsequently a written statement, dated 27 July 1951, with the information that he was not allowed to visit the Ballistic Research Laboratories due to circumstances which could not be revealed. Upon an inquiry of Burgers, the Embassy of the Netherlands in Washington DC subsequently informed Burgers in August 1951 that he had indeed originally received clearance to visit the Aberdeen Proving Grounds but that permission had suddenly been withdrawn. Actually, prior to his trip to the US in 1950, when he needed to get clearance to visit NOL, Burgers had to submit a form concerning his personal history, which was more specific than he had to fill out in earlier years. He responded by sending Raymond J. Seeger, who at that time was still Chief of the Mechanics Division at NOL, a confidential statement about his

affiliation with the CPH during 1919-1930 and his early visits to the USSR to be given to the Security Officer of NOL if needed or otherwise to be disregarded.

As Secretary of the Committee on Science and its Social Relations of the International Council of Scientific Unions, Burgers had been interacting with John Desmond Bernal as President of this committee, who was a Marxist and pro-Soviet sympathizer [14]. Bernal had even visited Burgers in his home in Delft in 1938. However, there is no evidence that his interactions with Bernal played a role in the denial of his visa application. On the other hand, a serious problem appeared to be that his fellow student and friend, Dirk Struik, had been suspended from teaching at MIT in 1950 because he had been accused of being a Soviet spy (a suspension which would last till 1956). Burgers and his wife had visited Struik during the International Congress of Mathematics in Cambridge, MA in 1950. It is rumored that the FBI had a picture of Burgers with Struik from this visit [15]. Another potential problem was that Burgers had written in 1950 a sympathetic letter to Edward M. Corson, who had criticized in Physics Today the arrest of Klaus Fuchs of the Harwell Atomic Energy Research Establishment in the UK [16]. After having published that letter, Corson had lost his professorship, his position as Atomic Energy Consultant, and was no longer allowed to work or teach anywhere in the US.

In a letter, dated 15 December 1951, Burgers notified Raymond Seeger in his capacity of Acting Director of IFDAM that his request for an immigration visa had been denied. In response Monroe H. Martin, who had become Director of IFDAM and Nathan L. Drake, Head of the Department of Chemistry and who had also become Director of the newly established Institute for Molecular Physics [12], embarked on a campaign to resolve the visa problem. Monroe Martin also invoked the help of Th. von Kármán, as well as of some other scientists. In addition, George W. Fogg, Director of Personnel at the University of Maryland

sent a formal request to the American Consulate in Rotterdam to issue Burgers a visa under the Exchange Program No. P-793 in June 1952. The Consulate responded that the invitation under this Exchange Program was not materially different from the invitation offered to Burgers in September 1951. On July 23 1952, Monroe Martin wrote to Burgers: "It is my considered judgment that if you can establish and document your resignation from the Communist party years ago and that since that time your convictions and actions demonstrate your sincere belief in the democratic ideals as exemplified by the United Nations, your visit will be granted. A straightforward statement of your political beliefs as a young student, the reasons for your change in conviction, and a convincing demonstration of your belief in the ideals of this country since that time will go a long way, in my estimation, towards convincing our authorities that your request for a visa should be granted." Following this advice, Burgers prepared a "Statement concerning my political opinions". In this statement he described his early intellectual interest in the CPH, how he became disenchanted with the CPH leading to his formal resignation as a member 1933. In addition he gave an account of his visits to the USSR in 1926, 1929, 1930, and 1936 and his visits to the US starting in 1931 and his subsequent visits to the US after Word War II. He expressed how he and his wife had been impressed with the great friendship, hospitality, and courtesy experienced in the US resulting in their desire to come to the US. He presented this statement in person at the American Consulate in Rotterdam on August 5 1952. In addition, he supplied written testimonies concerning his democratic attitude from A.J. Kluyver, President of the Royal Netherlands Academy of Sciences and a Foreign Associate of the US National Academy of Sciences, from O. Bottema, Rector Magnificus of the Technical University Delft, and from H.R. Kruyt, President of the Netherlands Organization of Applied Scientific Research. The verbal response he received was not encouraging although a promise was given that the documents would be sent to the State Department in Washington. Subsequently, in a letter, dated October 17 1952, Monroe Martin informed Burgers that all attempts of the

University of Maryland to secure a visa for him had failed. Burgers responded with a lengthy letter, dated October 24 1952, in which he expressed his great disappointment of not being able to join the University of Maryland. In a follow-up letter of January 30 1953, Monroe Martin wrote: "I have now had an opportunity to consult with a number of people in a position to advise us of the chance for success of any plan to bring you into this country as Research Professor in the Institute. From all sides I gain the understanding that, under the new Immigration Act, this will be even more difficult than before [17]. Certainly under present conditions it is not possible to set a date when we might initiate proceedings with some hope of eventual success."

Thus started a difficult period in the life of Burgers. He had already told his colleagues in Delft that he was about to leave for the University of Maryland and had already paid 2/3 of the price for his passage to the US on a ship of the Holland-America line. Burgers tried to address his situation in correspondence with several well known scientists, including Th. von Kármán, S.A. Goudsmit at Brookhaven National Laboratory, B.J. Bok at the Harvard College Observatory, Michael Polyani at the University of Manchester, S. Goldstein at the Institute of Technology in Haifa, Edward A. Shills, Editor of the Bulletin of Atomic Scientists. Actually, Burgers was one among many other foreign scientists who were not being able to visit the US at the time. The situation had become so bad, that the Bulletin of the Atomic Scientists published a special issue on the visa problems for scientists with testimony of eminent American scientists on American visa policy, including Albert Einstein, Hans A. Bethe, Harold C. Urey, James Frank, Samuel Goudsmit, Cyril S. Smith, Arthur H. Compton, William P. Murphy, Victor Weisskopf [18]. In addition, Allan T. Waterman, Director of the National Science Foundation, and Howard A. Meyerhoff, Administrative Secretary of the American Association for the Advancement of Science, submitted statements at a hearing of the Commission on Immigration and Naturalization of Congress under the titles "Some thoughts on International Scientific Communication" and "Scientists and the Visa Problem", respectively [17, 19].

Monroe Martin had informed Meyerhoff about the visa problem of Burgers and an appeal to give adequate consideration to foreign scientists who had their membership renounced "when full comprehension of the implications of Communism was acquired" was included in the testimony of Meyerhoff.

In 1953 Burgers tried to get help from A. Michels, a colleague at the University of Amsterdam. Michels had founded the Institute of Molecular Physics at the University of Maryland and commuted between Amsterdam and the University of Maryland on a regular basis without any visa problems [12]. On a somewhat encouraging note, Michels informed Burgers confidentially, after his return from the University of Maryland in October 1953, that the visa problem was being reconsidered. Actually, Zaka Slawski of NOL had continued to try to get the visa problem resolved. And indeed, on January 30 1954, Burgers received an invitation from the American Consulate in Rotterdam for a discussion of his visa application with the vice consul Frank M. Wyle. In the interview on February 1 1954, Mr. Wyle informed Burgers that the only possibility for solving the visa problem would be for Burgers to submit a detailed statement what he had done after his resignation from the CPH in opposition to communist principles. In response Burgers prepared a "Statement concerning the development of my political opinions" in March 1954 that was submitted to the American Consulate. This comprehensive statement, prepared by Burgers in support of his visa application, is a fascinating document elucidating the evolution of his ideas about science and society and his personal struggle to reconcile these two aspects in his personal life. A copy of this statement is contained in Appendix II. Nevertheless, no positive decision seemed to be imminent. It appears that the statement of March 1954 had not sufficiently resolved to what extent Burgers had opposed communism after his resignation as a member of the CPH. Hence, Burgers submitted to the American Consulate in February 1955 an addition to his 1954 statement concerning the development of his political opinions, presented in Appendix III.

When one reads these statements one is impressed with the openness, honesty, and personal integrity of Burgers. This latter statement appeared to resolve any remaining issues and in June 1955 Burgers received word from both Zaka Slawski of NOL and from the American Consulate in Rotterdam that a visa would be granted. Thus a long tortuous path had come to a happy ending. Burgers was relieved from his position as Professor at the Delft University of Technology effective November 1 1955 and gave his farewell address on November 2 1955, reflecting on his research in hydrodynamics in Delft since 1918. Two days later Burgers and his wife left for the US, where Burgers joined the Institute for Fluid Dynamics and Applied Mathematics at the University of Maryland as a Research Professor.

One may wonder why Burgers desired to go to the US sufficiently strongly so as to put up with many roadblocks. First, as mentioned in Section II.2, from his travels to the US Burgers had been impressed with the dynamic research culture in the US. Second, it seems that Burgers had found the small-town atmosphere in the city of Delft intellectually a bit confining. Third, after many years of administrative duties in Delft, he probably was longing to return to full-time research. Finally, he may have experienced this opportunity to embark on a new life with his second wife Anna Verhoeven. And indeed, as described in the remembrances of his son Herman Burgers presented in Appendix IV, the move to the US has turned out to be beneficial for the personal well-being of Jan Burgers.

II.3 Burgers at the University of Maryland (1955-1981)

Burgers found a vibrant research climate with people like Shih-I Pai, Elliott W. Montroll, and many other prominent scientists in fluid mechanics, applied mathematics, and statistical physics during his years at the

Figure 8 - Willy Burgers (left) with his brother Jan (right) in College Park (around 1956).



University of Maryland, where he felt at home immediately. For instance, in his speech, given at a Symposium held in 1965 in his honor [20], Burgers remarked: "The way in which we were received at the University of Maryland surpassed anything which we could have imagined when we came to America with the hope of settling here. We felt at home immediately and a deep love for this country has grown in us. The friendship one can find in United States and in particular in its scientific circles is a source of everlasting joy, which pervades all phases of one's life and one's work." And "You will know how great an honor it has been for me to be a part of the University of Maryland ever since I came here, and although I have seen several other universities in this country, I have never felt the desire to move away from Maryland".

Early after his arrival in College Park, Jan Burgers was visited by his brother Willy (1897-1988), with whom he had been collaborating on dislocations in crystal lattices leading to the concept of the Burgers vector [24] (Figure 8).

At the University of Maryland, Burgers embarked on a very productive second research career, first as a Research Professor [21-53] and subsequently as a Research Professor Emeritus [5, 54-69]. A characteristic feature of these publications is that they were almost all singly authored by Burgers. This does not mean that Burgers did not have graduate students and collaborators at the University of Maryland, but Burgers preferred that his students would publish their work independently.

The personal inclination of Burgers was towards research of a theoretical nature. However, Burgers also supervised PhD research in experimental fluid mechanics. First he resumed his interactions with NOL and his first student was Jacob Pomerantz, whose PhD dissertation was based on experimental work he had been pursuing at NOL [70]. In addition Burgers did inherit a laboratory for experimental shock-wave research



Figure 9 - Laboratory of Jan Burgers in the Physics Building at the University of Maryland in 1959. From left to right: Jan Burgers, Ir. Ruys (a previous student of Burgers visiting from The Netherlands), and Tobias de Boer.

from Edward L. Resler, Jr. [71], who was leaving for Cornell University at about the time Burgers arrived at the University of Maryland. (Figure 9).

Burgers organized an experimental research group consisting of graduate students Murray Scheibe, John P. Barach, Wim J. Witteman, P.C.T. (Tobias) de Boer, a postdoctoral research associate Jerome Daen, and a master student Andrew T. Mazzella. Murray Scheibe, a graduate student inherited from Resler [71], left after his PhD to pursue a scientific career in California. Barach had an undergraduate degree from Princeton University and became after his PhD at the University of Maryland in 1961 a professor of physics at Vanderbilt University. Witteman and de Boer were students attracted from The Netherlands. Witteman [72-74] was an exchange student with the Technical University Delft, who after leaving the University of Maryland went to the Philips Research Laboratories in The Netherlands; since 1969 he has been a professor in applied physics at the Technical University of Twente working on high-power laser systems. De Boer [43, 75-77], after his PhD at the University.

Mazzella was a master student who did experimental work in the research group of Burgers under the supervision of de Boer [78]. Daen [79] had been a postdoc with Peter Debye at Cornell University before becoming a postdoc with Burgers, and eventually became an administrator at the US National Science Foundation. Burgers also had a Swiss student in the laboratory, Albert Haberstich, who received his PhD in 1964 on "Experimental and Theoretical Study of an Ionizing Potential Wave in a Discharged Tube" and who became a scientist at the Los Alamos National Laboratory. Burgers must also have had some PhD students in theoretical fluid mechanics, but this information is not easily accessible from the records at the University of Maryland.

Besides supervising graduate students, Burgers was very active in pursuing research himself. At the time it was customary to publish research results as Technical Reports. The reason was that most of the research in fluid mechanics was supported by the US Air Force Office of Scientific Research, which expected Technical Reports. In Maryland, Burgers became one of the first to specialize in new fields related to hypersonic hydrodynamics, i.e., on high-speed and high-temperature flow problems [22, 23, 26, 28, 31. 35]].

He applied the basic equations of magneto-gasdynamics to the penetration of an originally plane shock wave through a homogeneous gas possessing a high electric conductivity, and originally at rest, in the presence of a magnetic dipole field. First he assumed that the magnetic field is affected by the motion of the gas, and that the shock wave and the motion of the gas are unaffected by the magnetic field. Thereafter the effect of the field on the motion of the gas was treated [27, 29, 36, 38].

Calculations were made by Burgers concerning the momentum that can be given to a jet of water by mixing it with a jet of high-speed steam. Apart from an application of the equations of momentum and enthalpy, this raised questions concerning the speed of condensation and the acceleration of the water [30].

Burgers formulated an extended form of the Boltzmann equation for the effect of collisions upon the distribution function, in which account is taken of chemical reactions that may result from collisions and of spontaneous processes. In order to prevent the scheme from becoming too complicated, a number of assumptions had to be introduced. He elucidated the various terms that must be included on the right-hand side of the Boltzmann equation [40].

In his book "Flow equations for composite gases" [58] Burgers developed complete sets of flow equations for composite gases, including those giving the components of heat flow and the pressure tensor.

Attention was given to both the single-and multi-fluid pictures. Topics treated included the Bhatnagar-Gross-Krook and Fokker-Planck approximations, charged particles in electric and magnetic fields, and the effects of chemical reactions and of radiation. Separate treatment of the left and right sides of the Boltzmann equation facilitated discussion of various collision problems, cross-section cutoff effect with two-particle distribution functions, the standard Maxwell-Boltzmann collision integral, and other topics [58].

Burgers paid much attention to the study of statistical plasma physics. He used, for instance, an approximate linearized equation for the pair correlation function, containing a Debye potential instead of the Coulomb potential, to derive by direct integration a solution for the pair correlation function for the case of long-wavelength plasma oscillations in a fully ionized gas without a magnetic field. The correlation was then applied to describe the effect of collisions in a linearized Boltzmann equation for the single-particle distribution function [42, 44].

Burgers also continued his work on statistical physics of turbulence [52, 60, 63] leading to the publication of his book "The Nonlinear Diffusion Equation" in 1972 [66]. The Burgers equation has turned out to be too simple for turbulence, but the model has been successful in other areas of science [80]. The book was reprinted on the occasion of an Inaugural Symposium of the Burgers Program for Fluid Dynamics at the University of Maryland in 2004. Inspired by the intellectual heritage of J.M. Burgers, the mission of this Burgers Program is to enhance the quality and international visibility of the research and educational programs in fluid dynamics and related areas at the University of Maryland. The Burgers Program for Fluid Dynamics at the University of Maryland has established a close collaboration with the J.M. Burgerscentrum in The Netherlands [81, 82].

In addition to scientific work in fluid mechanics, Burgers was also interested in social subjects [54]. in the foundations of quantum mechanics [47, 51, 57], in the philosophy of science [53, 67-69], in the emergence of order and the origin of life [46, 55, 59, 61]. Von Neumann's finding that the outcome of a measuring process is independent of the measuring apparatus effect was examined by Burgers in terms of quantum theory by using two methods for calculating the outcome of an interaction of a system of particles with the measuring process. He found the two results obtained to be equivalent [47]. Burgers discussed the problem whether an explanation of the phenomena of life can be deduced from an exclusive reliance on causal relationship as a (at that time) accepted physical description, or whether attention should be given to something more. For this more, the concept of anticipation was taken. Starting from what is observed in our own minds various notions involved in the idea of anticipation were brought forward [59]. His reflections were based on the philosophy of A.N. Whitehead, whose ideas he tried to develop in his own book "Experience and Conceptual Activity", published in 1965 [53]. However, he was rather disappointed with the general lack of interest in this book. Burgers also raised the question whether all forms of order observed in nature can be explained as results of the laws of physics. He proposed that, to explain the emergence of organization in the living world, we must introduce a principle that constitutes a relationship to an impending future [46, 55, 61].

At the Universality of Maryland Burgers showed himself as an impressive and eclectic scholar, highly appreciated by his colleagues. The research accomplishments of Burgers were celebrated in a Symposium on Dynamics of Fluids and Plasmas on the occasion of his retirement as Research Professor in 1965 [20]. However, he remained professionally active for another decade as Research Professor Emeritus [54-69]. Burgers died on June 7 1981, and his wife, Anna, died on November 4 1983.

A J.M. Burgers Centenary Symposium was held at the University of Maryland on May 4 1995 with a keynote lecture of F.T.M. Nieuwstadt from the J.M. Burgerscentrum in The Netherlands on The Legacy of J.M. Burgers.



Figure 10 - Herman Burgers (left) in his home in The Hague with Jan Sengers in 2014.

II.4 A note about the children of Burgers

Jan Burgers had three children, a daughter Anneke Burgers (1920-1966), a son Herman Burgers (1926-2016) and a second daughter Marion Burgers (1932-1997). Anneke became a practicing nurse in The Netherlands and in Switzerland and eventually became a teacher in nursing in Rotterdam. Marion became one of The Netherlands' most prominent radiation oncologists and an international ambassador for excellence in care [83]. J. Herman Burgers studied law at the University of Amsterdam and political science at Stanford University (Figure 10). He had a career in the Department of Foreign Affairs of The Netherlands. He inherited from his father a passion for human rights and was actively involved in international human rights activities [84].

All three children had been familiar with Anna Verhoeven before her marriage with Jan Burgers as "Aunt Annie" and she remained for the children of Burgers for them "Aunt Annie" throughout their lives. For the purpose of this article, it is interesting to note that Anneke and Marion were the first two children to visit their father in the US after he had joined the University of Maryland. They came back with the impression that their father seemed very anxious since he felt he had to demonstrate his competence as a scientist all over again.

When the death of Jan Burgers appeared to be imminent, his son Herman wrote a text about his memories of his father in the expectation that he would have an opportunity to speak at the memorial service. Unfortunately, it turned out that he did not have an opportunity to read his text at that occasion. Herman has given us permission to reproduce his memories in Appendix IV, so as to preserve his tribute to his father for posteriority.

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APPENDIX I

Curriculum Vitae of Johannes (Jan) Martinus Burgers (1895-1981)

Biographical data

Born in Arnhem (The Netherlands), January 13, 1895. Parents: Johannes M. Burgers and Johanna H. Burgers -Romyn. One brother: Dr. Wilhelm G. (Willy) Burgers.

Married on July 30, 1919 with Jeanette D. Roosenschoon (deceased August 5, 1939); three children: Anna Charlotte, Jan Herman, Jeanette Marion Veronica. Married again on August 20, 1941 with Anna Margretha Verhoeven.

Education

Primary and secondary schools in Arnhem, 1901-1912; supplementary courses in Latin and Greek, 1912-1914, together with studies of advanced mathematics and topics in theoretical physics. University education at the University of Leiden, 1914-1918: physics from P. Ehrenfest, J.P. Kuenen, and W. van der Woude;

astronomy from E.F. van de Sande Bakhuyzen and W. de Sitter; geology from K. Martin. Candidate 22 May 1915; doctorandus 1 December 1917, PhD 12 December 1918 on a thesis "Het Atoommodel van Rutherford-Bohr" with P. Ehrenfest as thesis advisor.

Honors and awards

 Elected Member of the Royal Netherlands Academy of Sciences 	1931
 Honorary Doctors degree, Université Libre de Bruxelles, Belgium 	1948
 Honorary Doctors degree, Université de Poitiers, France 	1950
 Ridder (Knight) in de Orde van de Nederlandse Leeuw 	1955
 Josiah Willard Gibbs Lecturer, American Mathematical Society 	1959
 Modesto Panetti Medal, Academia delle Scienze di Torino, Italy 	1961
 Foreign Member, Academia delle Scienze di Torino, Italy 	1964
 Bingham Medal, Society of Rheology 	1964
 ASME Medal, American Society of Mechanical Engineers 	1965
 Annual Award for Scientific Achievement, Society of Sigma Xi 	1966

Positions

University of Maryland, College Park, MD

 Assistant, Cryogenic Laboratory, University of Leiden 	1916-1917
 Conservator, Physical Laboratory of Teyler's Foundation, Haarlem 	1918
 Professor of Aerodynamics and Hydrodynamics, Department of Mechanical Engineering 	1918-1955
and Shipbuilding, Technical University Delft	
• Secretary, Department of Mechanical Engineering and Shipbuilding, Technical University Del	ft1921-1924
Chairman, Department of Mechanical Engineering and Shipbuilding, Technical University Del	ft1929-1931
 Research Professor, Institute for Fluid Dynamics and Applied Mathematics (IFDAM), 	1955-1965
University of Maryland, College Park, MD	
• Research Professor Emeritus, Institute for Fluid Dynamics and Applied Mathematics (IFDAM)	1965-1976
University of Maryland, College Park, MD	
 Research Professor Emeritus, Institute for Physical Science and Technology (IPST), 	1976-1981

Professional activities

 President Netherlands Physical Society (NNV) 	1922-1923
 Secretary Committee for International Scientific Cooperation, Royal Netherlands 	
Academy of Sciences	1934-1947
• Delegate to the meetings of the International Council of Scientific Unions in 1934 (Brussels),	
1937 (London), 1946 (London), 1949 (Copenhagen), 1952 (Amsterdam), 1955 (Oslo),	
1958 (Washington DC)	
 Vice-President, Section of Physical Sciences, Royal Netherlands Academy of Sciences 	1947-1950
 Secretary, Committee for the Study of Viscosity and Plasticity, Royal Netherlands 	1934-1950
Academy of Sciences	
 Chairman, Committee for the Study of Viscosity and Plasticity, Royal Netherlands 	1950-1954
Academy of Sciences	
 Organizer, 1st International Congress for Applied Mechanics, Delft 	1924
 Member, International Committee for the Congresses for Applied Mechanics 	1924-1946
 Founder of the International Union of Theoretical and Applied Mechanics 	1946
 General Secretary, International Union of Theoretical and Applied Mechanics 	1946-1952

 Secretary, Committee on Science and its Social relations, International 	1937-1952
Council of Scientific Unions	
 Secretary, Joint Commission on Viscosity and Plasticity, International 	1947-1952
Council of Scientific Unions	
Co-organizer of International Congresses on Rheology in 1948 (Scheveningen) and in 1953 (C)xford)
 Victor Emanuel Distinguished Visiting Professor, Cornell University 	1965
Professional Societies	
 Koninklijk Instituut van Ingenieurs 	
 American Institute of Aeronautics and Astronautics (Fellow) 	
 American Institute of Physics (Fellow) 	
 American Geophysical Union 	
 Philosophical Society of Washington 	
 American Academy of Arts and Sciences 	
 New York Academy of Sciences (Fellow) 	
 American Association for the Advancement of Science 	
 Society of Sigma Xi 	
 Sigma-Pi-Sigma Honors Society 	



APPENDIX II

J.M. Burgers: Statement concerning the development of my political opinions (submitted to the US Consulate in Rotterdam in March 1954)

From my parental home I have received the urge to form an independent judgment and to think freely. I also learned the utmost importance of moral and intellectual honesty, and of faithfulness. Together with that, I was educated in the conviction that one must strive towards enlightenment for everybody and towards providing every man and woman with the possibility of living without poverty and fear. I learned to consider peoples of other nations as being just as valuable and important as those of my own country.

Moreover, from my parental home I received the desire to inquire and to understand. My father had great abilities expounding popular science; his wide interests, which my mother shared as far as she could, were communicated to my brother and me. In this way we were introduced into the elements of physics, astronomy, geology and biology, and at an early date we were shown the wonders revealed by the microscope. Gradually we took over from our mother the task of assisting father with his popular lectures and demonstrations, and we helped him when he was collecting or arranging material for these lectures. The secondary school, which we visited in our native city Arnhem, widened my interests towards ancient history and geography.

Nevertheless, since I was good at mathematics and physics, the latter subjects became preponderant and determined my further career. My brother took to physical chemistry.

In 1914, when I was nineteen, I went to the University of Leiden to study mathematics and physics. I listened in particular to lectures of Lorentz, Kamerlingh Onnes and Ehrenfest. Among these three, it was Ehrenfest (who gave the main course in theoretical physics) who has had the greatest influence on me, through his sincere and complete friendship and through his powerful analytic mind, which deepened in me the desire for inquiry and understanding without limit and without fear. In later years I understood that I possessed something which Ehrenfest missed: a conviction of the meaning of life, a faith in the sense of all things around us, which I owe to my father. This has protected me and has helped me over difficulties that Ehrenfest has not been able to conquer.

My student years coincided with the war of 1914-1918 in Europe, which had cruelly shattered the ideals we had cherished in my parental home. The development of the war brought atrocities and the beginnings of suppressions of freedom, which later on have become more and more severe in several parts of the world. Under these circumstances it is not strange that young people, like myself, were deeply struck by the fact that in the beginning of 1917 a regime of oppression was overthrown in Russia. We were even more impressed by the first words of peace that were spoken in November of that same year by the group of men who had then come to power in that country. In comparison with the stagnant ideas of the other political groups, it looked that under the conditions then existing, the program of the men who tried to find some solution out of the terrible plight in which the Russian people found itself, was the best approximation available to the ideals of freedom from want and freedom from oppression, which always stood before me. I use the word "approximation" intentionally: as a scientist I am convinced that our thoughts cannot be

otherwise than approximations to a truth which itself is beyond us; one pursues an approximation so long till one is prepared to discard it when it is superseded by a better one.

Under these circumstances I felt induced to take up contact with a group of "left-wing" people in Holland, who had broken away from the social-democratic party already in 1909 and who looked with great hopes to what was now being attempted in Russia. The members of this group were mainly intellectual people and some poets; there were amongst them at that time comparatively few people from what is called the "working class". Several of them wrote essays on political theory. They did not intend to "start a revolution"; their theory was (and I think it goes back to Marx's own ideas) that a revolution may occur in a country, when economic or other difficulties have risen to such a point that the existing government is unable to find a solution; if under such circumstances great masses of people would come into motion, a revolution might be the result, and in view of that it would be useful to contemplate whether some other solution, some other economic and political system could help.

A book, that had appeared in 1918 and had made a certain impression on me, was by the late Henriëtte Roland Holst, who is honored (still at the present time) as the greatest female poet in Holland of our era, on "Revolutionary Mass Action." It attempted to demonstrate that in times of crisis the masses could be trusted to have the proper feeling for what would be the right thing to do, and it gave a historical account of many situations where this had happened. I hoped that this doctrine would be true, fearing that ordinary education works too slowly and may not reach its purpose when it is not stimulated from time to time by some great shock. Later I have realized that when large masses of people come into motion, too many stupid and crippled souls penetrate into the leading groups, the consequences of which have appeared, very cruelly, in Russia and in Germany, as well as in many other regions.

To come back to the group of "left-wing" people I have mentioned, all of them had started from democratic principles and though they often quarreled frightfully amongst themselves, they still had respect for each other's personality. The relations existing in their organization were quite different from what has developed afterwards; there was no party organization in the later sense of the word. In 1918 they had started to call themselves "Communist Party of Holland" (CPH), but at that time and even for many subsequent years they did in no way resemble the organization that is presently known as communist party. My belief at the time, that this group was a bearer of ideals directed at promoting well-being and happiness for the greatest number of people, more adequate than any other then existing political group, was strengthened by the state of conservatism and reaction which prevailed in many countries of Europe in the years immediately following 1918. The complete failure of the social-democrats of that time to give a clear analysis of the crisis that was tearing Europe apart, had caused a bitter disappointment.

The League of Nations, which had been created at the end of the war, was loaded with so many internal quarrels and such weaknesses, that it did not look like a promising instrument. It was only several years later that it began to embody certain important ideals. An organization for international scientific cooperation, created in 1918-1919, suffered under rules that locked out scientists from Germany and Austria. It was only in 1928 and finally in 1931 that these rules were changed and that a better climate for cooperation appeared. Therefore, my own involvement in the field of international scientific cooperation started in an independent way, to which I shall come back later.

I have mentioned this to explain how uninspiring the outlook was in the years immediately following 1918. What was going on in Russia at that time seemed to be the only glimmer of light. It is difficult at present to form a proper picture of that period; so much of our outlook has changed since then and it will be

more difficult for an American, in particular for the middle and younger generations, for whom problems and conditions were so completely different.

I state again, and this is of importance when one attempts to form a judgment about those times, that in the so-called CPH of these years (from 1918 into the nineteen-thirties), there was no oath of allegiance, no promise which bound members, no party documents giving any rights or putting one under certain obligations, no registration numbers, no cells, no secrecy. Holland was a slow country in several respects and those who sympathized with the communist ideas were imbibed with democratic convictions and did not like to be bound hands and feet. The persons whom I met personally were older people, schooled in the days before 1914 and keen on open discussion. The group in Delft was not important and was very small, a few people from the working class, a few from offices, a schoolteacher, all of them were democratic by nature. My interest was theoretical, directed at understanding and observing, in the belief that insight in communist opinion would provide understanding what happened in history.

I have never taken part in any action. Never did I take side in open discussions, whether at meetings or in writing; nor did I write any essays. I kept myself in the background, as an observer, and I have always kept complete personal independence. I have never mixed up anything of my political interests with my university work. The rules concerning the adherence to certain political parties valid for civil servants, did not apply to university professors. It was once pointed out to me that I should not make any political propaganda among students: I never attempted nor intended to do so. I did not start any discussion group, nor did I attempt to influence any student privately. I am not a propagandist for a subject which cannot be stated in mathematical formulas.

On the whole I had little contact with students beyond my lectures during the first years of my professorship; and I was not inclined to expound to other people political views of which I felt the limitations and uncertainties myself.

In all those years, just as later, I have done all my work, in Holland and elsewhere, whether scientific, educational or administrative, honestly, loyally and carefully, as bound by my conscience and by my oath to my own government, and to the best of my abilities, without ever suffering myself to be led astray by any subversive doctrine or thought. I have never broken any trust that was placed in me, never given out any information to people for whom it was not destined. I have never accepted any rule of conduct from communists. I was trusted, therefore, by all my university colleagues and I took my part in the usual administrative work. Although many of my colleagues knew of my political views, my opinions were appreciated in all university matters. I have been secretary of the Department of Mechanical Engineering and Shipbuilding from 1921 to 1924, and chairman from 1929 to 1931.

I have never suffered myself to be blinded by a tenacious keeping to beliefs. I was interested in Marx's views on history and on economic development, but I found that, whereas some of his original ideas are highly illuminating, there was much in the development of so-called historic materialism, which I could not accept at face value. I began to observe errors of judgment in the people who professed communist ideas, and my sympathy with and my faith in communists decreased in the measure in which Russian influence began to show itself in the party in Holland and became a threat to independent thinking (to my change of attitude also contributed the circumstance that more hopeful signs were making their appearance in other quarters). In the communist party in Holland people whom I had respected for their judgment, were pushed out. I perceived that tendencies developed towards doing away with an independent attitude. I remember

having had a long discussion with somebody, who professed that loyalty to the party should go above all, above one's independent judgment, above honesty to other people, but I could not convince him of my entirely different point of view. I also remember that once I was asked what I would do if a revolution would break out. My reply was: "first wait and see what is happening, in order to understand something about it." This was considered quite insufficient: I should have said that I would come immediately to the party and follow its orders, which I refused. I began to distrust the soundness of the judgment of people who were directing communist policy, and I was extremely shocked by the fact that the communist party in Germany in the beginning of 1933 directed its members to vote for Hitler and against the democratic groups, thinking that once Hitler would come to power, the communists would be able to take over within a few months. For a party that professed to have an almost perfect organization for obtaining information about workers' movements in all countries, such an error of judgment proved that it was blind on essential points. By doing so it has co-operated with the occurrence of the worst calamity that has befallen Europe in this century. In 1932, in a leading German communist periodical on "Marxism" I found a series of articles on the meaning of Marx's work, which utterly disgusted me by the way in which every author repeated the same slogans, without ever bringing a single original thought. It demonstrated the low level of thinking that official teachers of Marxism had adopted.

The result of all this, in addition to the increasing pressure of my scientific work, was that I ceased going to meetings already before 1930; it may well have been already some years earlier. Some personal bonds of friendship with a few of the older people still held me for a time, until I formally resigned in September 1933. A translation of my letter of resignation, dated 22 September 1933, is attached as an Appendix.





I did not undertake any open action against the communist party after my resignation; there was no reason for it and I have always been an enemy of advertising myself. I have never allowed party people to use my name for any purpose during my membership of the CPH; I did not want to have my name to be associated with any opposition action. Neither would this have been in the interest of my University; such things would have been thoroughly disliked. I have not been a conspirator and nobody has thought that I ever would be one. On the other hand, I have not been treated badly by the party members; many of them had already observed the change in my attitude for a long time; they respected me and let me go without difficulties. Many of them still were democratically minded and could accept a personal conviction. Also there were no improper deeds in the party that might have required exposition. Its yearly conventions still were open to the public, and its aim was to spread a system of political ideas, not to engage in secret conspiracy. It was not a communist party in the sense as is currently understood. There were many non-communist persons at that time who believed, as I did, that as one of the possible forms of political opinions the CPH still had its use. As for myself, I had already seen that for any attempt to do something in relation to problems of human society, I would have to go quite a different way, to which I shall return below.

The party had ceased for me to provide a satisfactory background for ideals and for an understanding what was moving the world. It had become barren for me and it is not in my nature to fight what is dead for me. Hence, I could leave it without scars in my conscience. I had no internal difficulties or doubts about doing this, which sometimes can hinder people greatly. Nobody had ever attempted to ask from me any dishonest thing; that did not happen among the people whom I have known. I was certain about myself and did not need to defend finding myself back again. I have not been so naïve as several people who contributed to the book "The God that failed", most of whom became communists when I had already

turned away and had directed my thoughts in a different direction. I have not been upset in any way as some of them have been.

American citizens, judging from present-day affairs, cannot form a picture of what existed in Holland in the years before 1933. The people, who had established the communist party in Holland in that period, did so because they believed to have a vision of an ideal for the entire world; they also believed that Russia was on its way to realize much of it; but they never considered themselves to be just fighters on behalf of the U.S.S.R. and they fought against the penetration of Russian methods into party organization. Later on, other views may have come to prevail, but it was not so in my time, and it is not logically correct to put the group, which I have known, into the same category as the people who believe in the present Russian brand of communism.

A point, which I want to stress, is that I have taken great care that my children would not be inoculated with some communist doctrine which would be difficult for them to get rid of later. At home we always have discussed freely and with an open mind, giving attention to the various aspects of every problem. In 1933 my children (they are from my first marriage) were still young. When they grew up and became interested in what happened around them, I have done my best to give them a clear all-around view and I have expressed to them my distrust of communist tactics or opinion, in those cases where any piece of news opened an opportunity to discuss it.

In later years my older daughter has turned to a more religious attitude of mind than I have myself. She is now a hospital nurse. My son has studied law. He was in Netherlands military service from 1947-1950; two years of this were spent in Indonesia, where he was not in the fighting forces, but worked at the

court-martial in Batavia. In 1950 he was entrusted with the direction of the judge-advocate's office. At present he has a position at the Ministry of Foreign Affairs in The Hague. My younger daughter is studying medicine in Amsterdam, and she has the same openness of mind and the same fearlessness we all have, which was present in my father's home and in that of my first wife, and which is fully shared by my present wife.

In the meantime I had taken up work directed to furthering international cooperation in science. Later I have been occupied several times with the organization of research into problems connected with the relations between science and society. This work was aimed at promoting an attitude of mind, quite different from the one advocated by the communist party. I therefore think it appropriate to give some details about that work, the more so as it belongs to the circumstances that have shaped my mind. By the way, it were my international scientific relations which led to some contacts with Russia (these had not come though party connections) and to a close contact with the U.S.A.

I had been internationally minded from the start; to be otherwise is not possible in science. As a student in Leiden, I had worked with professor Ehrenfest and in 1916 I succeeded in proving a theorem referring to the earlier form of quantum mechanics, which was of importance for Ehrenfest's work. Ehrenfest then brought me into correspondence with physicists outside Holland, among these with Dr. P.S. Epstein, who at the time worked in München (since 1922 he is professor of theoretical physics at the California Institute of Technology). Ehrenfest also introduced me to Einstein, when the latter visited Leiden in 1916.

In 1918 I was appointed as full professor of aero- and hydrodynamics at the Technical University of Delft, a position which I still hold at present. The appointment came a few months before I had obtained
my doctor's degree at the University of Leiden; I was then 23. The professorship position that was entrusted to me was a completely new one in Holland; I do not even know whether at that time there were similar ones elsewhere. I had to create the subject and its method of teaching and I also had to create a (although modest) laboratory. In passing I mention that aeronautical research as required by industry and by the military was entrusted to a newly created Government Research Institute (founded in April 1918) with which I always have had strong personal and scientific relations. In 1937 I was appointed as a member of its scientific advisory committee.

In 1921 I became acquainted with Dr. Th. von Kármán, then professor of mechanics and aerodynamics in Aachen; this was the beginning of a lifelong friendship, by which I feel greatly honored and which did not change when Dr. von Kármán in 1930 went to the California Institute of Technology. It still lasts until the present time. Dr. von Kármán was then working on subjects of boundary layer friction and turbulence; in 1923 and 1924, by making use of a special type of apparatus, my assistant and I succeeded to do some fundamental research in my laboratory at Delft, which confirmed Dr. von Kármán's theories.

Dr. von Kármán also invited me to take part in the first international conference on Hydro- and Aerodynamics in Innsbruck in 1922. The next year Dr. von Kármán visited me and proposed that I should try to organize an International Congress on Applied Mechanics at our University in Delft. I consulted my colleague Professor C.B. Biezeno. Both of us felt greatly attracted towards the idea; we started working on this project and the Congress, which took place 22-26 April 1924, was a great success; scientists came from 19 countries, including the U.S.A. (Dr. J.C. Hunsaker), U.S.S.R., Spain, Turkey and Egypt. Professor Biezeno was chairman and I was secretary; after the Congress we were editors of the Proceedings. We had invited a number of scientists from various countries to form a "sponsoring committee"; this committee became a

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permanent body which decided to arrange regularly for such international congresses. Since then they have been held in 1926 (Zürich), 1930 (Stockholm), 1934 (Cambridge, England), 1938 (Cambridge, MA, U.S.A), 1946 (Paris), 1948 (London), 1952 (Istanbul); the next congress will be held in Brussels in 1956. I have always remained a member of the International Congress Committee and have taken part in its discussions and decisions.

We did this independently of the then existing "International Research Council", the organization which I have alluded to earlier and which at that time suffered under rules barring the Germans and Austrians. We found sufficient support among our scientific colleagues to continue in our way without difficulties.

In 1928 the International Research Council was replaced by a new body, the "International Council of Scientific Unions", that could accept scientists from all countries. My colleagues in applied mechanics, in Holland, Great Britain and in the U.S.A, thought it wiser to remain independent with our international congresses. However, after the second war, at the VIth International Congress of Applied Mechanics in Paris, I proposed that we should change our attitude and form an "International Union of Theoretical and Applied Mechanics", which could ask admission to the International Council. This proposal was accepted and I was appointed secretary of the new International Union. The International Council accepted our Union as a member in 1947. I have held the position of secretary until 1952. When I stepped down so as to get more free time for purely scientific work. I could then feel certain that the Union was well under way and leave the secretarial work to my successor, Professor F.H. van den Dungen in Brussels.

During my time as secretary I came into contact with UNESCO, which helped the Unions with financial grants. Together with Professor J.H. Oort in Leiden, representing the International Astronomical Union, we

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have initiated two "inter-Union" symposia on the gas dynamics of the interstellar clouds, one in Paris in 1949, and the other one at Cambridge. England in 1953. For each of these symposia UNESCO gave a grant of \$ 6000 for the two Unions together, to enable us to pay travel expenses for a selected group of invited scientists from European countries and from the U.S.A (naturally, most of the money had to go to the latter country because of the larger distance). Through the recommendation of Dr. von Kármán, the Proceedings of the first meeting (the editorial work was done by me in cooperation with Professor van de Hulst in Leiden) have been printed for us free of charge by the Central Air Documents Office of the U.S Army-Navy-Air Force in Dayton, OH, in view of the importance of the results for the U.S.A. Another "inter-Union" symposium, for which I also had taken the initiative, was held in 1950 at Hershey, PA through cooperation of the International Union of Geodesy and Geophysics and the International Union of Theoretical and Applied Mechanics, the subject being "Plastic Flow and Deformation within the Earth", while Dr. L.H. Adams of the Geophysical Laboratory, Carnegie Institute of Washington, acted as chairman. Again UNESCO gave a grant of \$ 6000 for the two Unions together.

I pass over some other international bodies of which I have been secretary and where my main task was to coordinate the work.

In 1929, when Dr. von Kármán was still in Aachen, Dr. W.F. Durand from Stanford University in California came over to invite European scientists for cooperation in the preparation of a standard work on aerodynamics, which he was editing under a grant from the Guggenheim Fund for the Promotion of Aeronautics. Dr. Durand considered Dr. von Kármán, Professor Prandtl in Göttingen, Professor Taylor in Cambridge, England, and me to represent the "big four" in aerodynamics at that time; and he asked von Kármán and me to prepare one of the volumes. For this purpose Dr. Durand invited me to come to Stanford

University in the beginning of 1931. I was in California for four months (including a stay of three weeks at the California Institute of Technology). This was my first visit to the U.S.A.

I now come to a discussion of my contacts with Russia. As mentioned earlier, they were of a scientific nature. Apart from that I have been no more than a visitor and a friend of several scientists.

At the first International Congress of Applied Mechanics in Delft in 1924, three Russian scientists attended, among them Professor A.F. Joffe (who was then one of the great masters in physics and who had done fundamental work on the properties of crystals, on which subject he gave a series of lectures some years later, in 1927, at the University of California in Berkeley, CA) and Professor Friedmann, who brought a pioneering paper on turbulence. Friedmann died in Leningrad in 1925, and when in the fall of that year Joffe visited Holland again, he asked me whether I would be willing to become Friedmann's successor. I felt inclined to consider the proposal and in 1926 I visited Leningrad and Moscow for about six weeks, to see what was possible. It proved to be impossible for me; the conditions were too uncertain and moreover, I had to consider the state of health of my wife, for whom the difficulties of living in Russia would have been too great.

In 1929 and in 1930 I was invited to give a series of lectures on hydrodynamics in Moscow and in Leningrad. On both occasions I met many persons and I saw again several of those whom I had seen in 1926. Without exception they were persons of my class: scientists, professors, students; they were nice and friendly and hospitable to the utmost as the not very rich conditions of their country permitted. I spoke some Russian, sufficient for my lectures and for personal conversation; I could find my way alone and did not need a guide, and I visited many of my friends in their homes.

There still followed an invitation from the Central Aero-Hydrodynamic Institute in Moscow in 1936. I visited Russia in the summer, at a time when there were many American and other tourists, and I made a short trip to the Northern Caucasus. I did not stay, however, to give lectures as originally intended, since I received an invitation for a scientific conference at Edinburgh, Scotland, in which I had to take part. This obliged me to go back before the school season started.

Moreover, some things had given me an unpleasant feeling about the conditions in Russia. We had heard in Holland about convict labor, and in Moscow I was told in a local train that the new canals in the country around that city had been made by convicts. Of course, one cannot say anything about convict labor when one does not know the conditions, and about these we did not speak. However, there was more. The great physiologist Pawlow had died and the semi-popular Russian journal "Science and Life" published an issue in which, in the name of the Russian Academy of Sciences, various scientists expressed their admiration for Pawlow's work. As far as I could read the articles, they were disappointing: almost everyone stated that Pawlow had been such a good Marxist, but there was no real explanation of the meaning of Pawlow's work, no precise description of the experiments and their results, which would enable the reader to form a judgment for himself.

When I returned from the trip to the Caucasus, the first one of the well-known trials had just started. Together with one of my fellow passengers I read the newspaper report. I told him that I could not understand how a communist could have debased himself so far as to seek allegiance with Nazi-people and with capitalists in Germany. My fellow passenger, who made a very honest impression, told me that he could not understand it either. In Moscow I observed that every journal, including weekly periodicals, published exactly the same report; there was no individual reporting as we know in our countries. Then I read a story

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in the Izvestia, entitled "Treason amidst our own ranks", in which it was told that in a factory in Leningrad a young man worked whose father was among the accused (no sentence had been pronounced as yet), and it was decried as intolerable that the young man should be allowed to stay there. A meeting of the workers had been called to consider this and to require that he be fired. No mention was made of any bad deed or bad move the young man had made; as the worst item it was mentioned that he had been active in a youth organization. In the Izvestia of the next day there appeared a report on a workers' meeting at another factory: acting upon what had been reported about the previous meeting, the workers of the second factory expressed their indignation against the same young man and asked that he be chased from the ranks of faithful people. It was all too clear that this case of "guilt by association" was staged.

I then heard some things about trials in Russian prisons (not such things as have been told later or have been guessed from what the official reports of trials would make believe us), but still sufficiently impressive for me. It was about a man who had been before the court on a certain night and was told that he has forfeited the right to live and he knows that within a few minutes he will be sent out into the corridor where he will be shot immediately. In this state of nervous tension, the man may be offered a cigarette by the acting judge and this may lead to renewed questioning. The examination then perhaps may end with his being sent out through another door, back to his cell. This was in 1936; I do not know what has happened since then, but I am afraid that things have become worse. I could, of course, not speak in public about these matters, which would have endangered those from whom I had heard them. But I have mentioned them often in private conversation.

I also had observed the stubbornness of an official, whom I had to ask something about my passport, but on the other hand I still must record the helpful way in which another official, in a similar position but in

another city, gave me an extension of my visa when it was nearly ended and made things easy for me.

Towards the end of 1936 it was told in Europe that it was better not to write to Russian colleagues any more, for fear that they might suffer from it in their own country. The consequence was that I lost all contact with the people whom I had known.

Shortly thereafter, it became known in Holland that several Dutch people, who in the years before had emigrated to the U.S.S.R. and who had given all their energy to their new country, had disappeared. It was impossible to obtain any information about them.

I repeat that the people with whom I had made acquaintance on my travels in the U.S.S.R. were scientists, university people. Of them I have the best recollections: they were friendly and sympathetic. I would say "just like Americans", but scientists are apt to form a brotherhood all over the world. I could trust them and I was trusted by them; of that I have proof. I have never had any contact with party members or with their organizations in the U.S.S.R. I was afraid that party people would make use of my presence for propaganda purposes, something I most heartily disliked and still dislike; I did not want being driven into a false position. I also knew that those scientists, like Joffe and some others, who had hoped that I would come to their country to work with them, in no way wanted me to be involved in party politics. They were men who loved science and loved mankind; they accepted their government for what it gave to them in the form of possibilities for research, they had hope in its future, but they were no party politicians. I know that one of my colleagues took care to keep Russian newspapermen away from me; he also told me that already at that time there were factions fighting each other to death. I have never been employed by the Russian government or by a Russian firm, neither directly, nor as a consultant.

I have not been so naïve as the French writer André Gide, who was a member of a delegation of artists much honored in Moscow (I believe it was also in the summer of 1936). Then Gide made a trip to the Caucasus, he wanted to send a telegram to Stalin to express his gratitude and he was upset when he was told that he should use the prescribed formulas for praising Stalin. Any man who used his brains should know that when one is honored as a special guest, one must pay for it in the form of some official statement of admiration. I have carefully kept away from these things and, therefore, I did not need to write a book "Retour de I'U.R.S.S." as Gide did, in which he gave some criticism along with his praise of the country (for which criticism he then was promptly decried by all communist papers in Western Europe).

As I mentioned, I spoke Russian sufficiently to make myself understood, and I could move freely through the streets, buy what I desired and travel in trains without the need of a guide. I have stayed with people at home and partook in their life. My trip to the Caucasus was arranged by a friend who belonged to the Scientists' Club in Moscow and who gave me an introduction to a tourists' camp; I travelled alone, talked with my fellow passengers, shared their troubles when a truck in which we were transported broke down at night, or when on the road back heavy rains threatened to flood the road and to throw our bus into the river.

Then the second war came: the Russian-German pact and the partition of Poland, the forceful removal of the entire populations from the Baltic countries to Siberia, the attack upon Finland, etc. This showed us how much things had changed with the Russian government and with the party on which it bases itself. I have read later about German communists, who years before had come to the Soviet republic to escape from the Nazis, but who in 1940 were turned over by the Russian government to the Nazi-police. What had become of the ideals for a new era of human freedom, which had been upheld before us some twenty years ago?

When in the second part of the last war the Russian armies were able to expel the Germans from their country, we all were very glad in Holland: it meant the turn of a tide which had enveloped us. The successes of the Russian armies were followed with much enthusiasm and we hoped at the same time for a better state of affairs in Russia herself, which would bring greater freedom to the Russian people and make possible a genuine contact between them and the Western nations. It is not necessary to stress that our hopes have failed in this respect.

I will now mention some points concerning the development of my world picture to make clear how far I have gone away from crude Marxist ideas.

I have already mentioned that about 1930 there were several developments which gave much more hope than in 1919 and 1920. The League of Nations proved to stand for some good things and I was impressed by the report the League had prepared on the Japanese aggression in China in 1932; I read this report with much interest and I saw that it was written without concealing any of the facts. Another feature was the address read by General J.C. Smuts from South Africa as president of the Centenary Meeting of the British Association in London, in 1931, on "The scientific world picture of today" in which he spoke the following words (which for many people have become a guiding thought):

"One of the greatest tasks before the human race will be to link up science with ethical values, and thus to remove grave dangers threatening our future. A serious lag has already developed between our rapid scientific advance and our stationary ethical development, a lag which has already found expression in the greatest tragedy of history. Science must itself help to close this dangerous gap in our advance, which threatens the disruption of our civilization and the

decay of our species. Its final and perhaps most difficult task may be found just here. Science may be destined to become the most effective drive towards ethical values, and in that way to render it's most priceless human service. In saying this I am going beyond the scope of science as presently understood, but the conception of science itself is bound to be affected by its eventual integration with the other great values."

A similar spirit was expressed in many books by the British author H.G. Wells, of whom I had read the "Outline of History" and the "Work, Wealth and Happiness of Mankind" and several others. Wells always gave evidence of flashes of deep insight; he had a peculiar form of expressing in a single sentence, by an apt choice of words, ideas which help one to arrive at fresh views on matters that for long times had been loaded with tradition. One of his observations was that it is now becoming inadequate to picture the struggles of mankind in terms of a contest between "haves" and "have-nots" as a "class war", as the Marxists did: far more, it is now becoming a struggle between people who have insight and look into the future, and people who are restricted in their outlook, being afraid of losing what made life appear secure for them.

I also became more aware of the meaning of what usually are called spiritual matters. I left behind me the simple concept of materialism, in the sense that all what happens in the universe and in man's soul might be explained uniquely and completely on the principles and results which we deduce from physics and mechanics. I recognized the limitations of scientific reasoning on the basis of causal and statistical laws (which, of course, does in no way decrease my love for science and my admiration for the insight it provides).

All this pointed to the need of a far more fundamental understanding than the political theories of the communist creed provided. It was not a domain that could lead to easy discussion, the more so as my

way of expressing these matters on the whole is rather abstract. I cannot popularize my thoughts on these problems, since I am constantly seeking for the most adequate words. But on many occasions I came to express parts of my views in private letters and in discussions with some of my physicist friends.

If I try to formulate what I consider as the purpose of human life, I cannot say otherwise than that it is to strive continually after truth; to be continually aware of the task God has put upon us in giving us powers for understanding in forms that others can share with us. I have used the word "God" not to express a belief in a personal Father, on whose will the world depends, but to account for the consciousness of an urge in us, which I believe to be connected with something that is active in the whole universe. I am also convinced that every expression we give to a truth that we have experienced, be it in human relations, in science, in artistic expression, or in religion, needs continuous rethinking, reformulation and re-interpretation. No final expression can be given once and for all.

My political views derive from this conviction. The best society is one which gives the greatest possibilities and inducements to search for truth and to its expression in manifold ways. Freedom is necessary for this: freedom of thinking and freedom of investigation, and in no way less freedom of expression. The only limits to this freedom are to be found in the help one must give to other people; a form of expression that would do serious harm to other people, does not serve truth but is a corruption of it.

In our present time not states, nor parties, but individuals and small, freely cooperating groups can make the greatest advances in this respect. And the greatest stimulus for their creative activities is derived from free interactions with others, free exchange of opinions in writing and in speech, by means of letters or publications, and by personal contact (for which free traveling is necessary), in order to stimulate a free





exchange of opinions and free mutual criticism between the most diverse elements of our human society. What we call "democratic society" offers the best possibilities for this aim. I know that we have not found a definite form of democratic society, there must always be development, but the best approximation to our democratic ideals must embody the thought expressed by Wells in one of the chapters of "The Work, Wealth and Happiness of Mankind": "Our world is now launched upon a perpetual investigation and innovation, and its ideals of education is no longer the establishment of static ideology, but the creation of a receptive and co-operative alertness".

My scientific work did not permit me to go into philosophical studies as far as would have been needed for writing essays about these issues, although I have sometimes gathered material for this purpose. Some thought have been expressed in minor publications, namely, in a paper on "Entropy and Relation to Function of Life" (1943), in an essay on "Aspects of Modern Western Science) (1944), and in an invited address read before the joint meeting of the two divisions of the Royal Netherlands Academy of Sciences in April 1953) (all these papers have been published, in Dutch, in periodicals of the Academy).

There was one aspect of these problems which came more close to scientific work proper: the relations between sciences and society. These had begun to attract attention in wide circles of scientists since 1920, and, in particular between the years 1930 and 1940, when the leading British scientific journal "Nature" devoted much space to this subject. I found interest for these matters among several members of the Royal Netherlands Academy of Sciences. In particular, this applied to Professor H.R. Kruyt, who was vice-president of the section of sciences from 1931 to 1947. We considered whether it might be possible to start some work on an international scale, through the International Council of Scientific Unions (Professor Kruyt and I have been regular delegates to the meetings of the International Council for the Academy of

Sciences since 1934). At the same time we felt the need for research to be carried out in the Netherlands. In 1938 Kruyt mentioned to me that Professor R. de Joselin de Jong, emeritus professor of medicine, then living in Driebergen, was very much interested and was prepared to undertake research on this subject. A small group of scientists joined us, and we proposed an organization to promote research into the relations between science and society. We had many meetings in Driebergen and drafted a set of statutes, on the basis of which a "Stichting" (Foundation) was created by a deed before Mr. Arie de Mos, notary public in The Hague on 7 February 1940. However, the war of 1939-1945 had already broken out. It came to the Netherlands in May 1940 before any work could have been undertaken. It was hopeless to continue anything of this kind under the German occupation.

During the Nazi occupation the Technical University in Delft suffered many difficulties. The professors of Jewish birth were dismissed in November 1940; the students went on strike (the same happened in Leiden) and both the universities in Delft and Leiden were closed for teaching to the students. The universities in Amsterdam, Utrecht and Groningen closed at the same time on their own account. The University in Leiden never re-opened during the Nazi occupation, but the Technical University Delft could resume its teaching in May or June 1941. Much was done by the staff to make up for the loss of time suffered by the students (during the period of suspension of teaching I had taken part, for instance, in a private seminar on aeronautics). Very serious difficulties arose in February 1943: on the 6th of February the Cerman police suddenly came to several buildings and arrested the students because of an alleged cooperation with an attack on some pro-German Dutch official in The Hague. Teaching again had to be suspended. A number of students were transported to Germany for forced labor. After some months it was announced that teaching could continue for those students who would sign a declaration of loyalty with the prospect of

having to work in Germany after having completed their courses. The alternative was to be taken to Germany immediately (unless one could go in hiding, which was impossible for many of them). The University Senate, after long discussion, advised the students to sign (we were prepared to graduate not a single student and to keep a secret record of their progress for graduation after the occupation would be over). The Netherlands government in exile in London advised the students by radio to go into hiding. With a greatly reduced number of students the University continued operating until the beginning of 1944, when further teaching became completely impossible through a combination of circumstances. Moreover, in September 1944 the big railway strike came as ordered by the exiled government; all transportation stopped and soon after that gas and electricity ran out.

I have taken full part in all discussions with my colleagues, privately, in the Department of Mechanical Engineering, and in the Senate; moreover, I often contacted groups in other cities. Together with my colleagues, I have done my best to help students where I could.

During the war, thoughts about social problems kept many of us busy. When during the fall and winter of 1944-1945 the possibility for doing scientific work was practically reduced to nil, talks with some academic friends at Amsterdam, Leiden and The Hague again raised hope for creating some center of research for such problems. Since communications between the various cities were becoming difficult, it was not possible to get into close contact with Professor de Josselin de Jong, who also was not in very good health. However, I found others who were very much interested and after many discussions on what we considered as most important to be done, we drafted statutes for a new society with a larger scope than the previous foundation. Among the persons cooperating were Dr. F. Bakker Schut, director of the Government Bureau for the National Plan; Professor W. Schermerhorn, who became the first prime minister of the Netherlands after the

liberation; Ir. F.Q. den Hollander, who afterwards became director of the Netherlands Railways; Professor N. Posthumus, who became director of the Government Bureau for War documentation; Mr. J. in 't Veld, who became minister for housing ("volkshuisvesting"), Ir. L. Mansholt, who became minister of agriculture, Ir. H. Vos, who became minister for commerce and industry; Dr. C.H. van der Leeuw, director of "Van Nelle", Rotterdam. On July 19, 1945, by a deed before Mr. Jacob Tiedama, notary public in The Hague, a new foundation was created, called "Research Center for Social Problems".

Before the liberation and also in the first months of freedom, we had worked hard to prepare a set of memoranda on various social problems which we expected to come to the foreground in the reconstruction of our national life (general reconstruction questions; planning for the entire country; medical care; housing; public works; protection of nature and care of the landscape; education; cultural meaning of daily work; principles of economic reconstruction; etc.). Meetings were arranged for discussing these and other subjects.

Our financial means, however, were very limited. We had hoped to obtain semi-official status for the Research Center by support from the government. I even seriously considered whether I should devote myself completely to work in this direction which would imply leaving my scientific work, but Professor Schermerhorn had so many other things to attend to, that we were not put in the priority program. We might have obtained recognition, if we could have worked somewhat in the way of the British Society "Political and Economic Planning", which from its proper financial means could undertake social research leading to publications. Our means, however, could not be compared with those of this British organization and what proved worse: most of the people with whom I had planned the work, suddenly proved to be so heavily charged with obligations, that they had no longer time for discussions, still less for undertaking any definite research work. Hence, all responsibility fell on my own shoulders.

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Then in August 1945 I was invited to travel to England with a group of assistants of our Technical University, in order to study scientific literature and scientific progress made during the war years, from which the Netherlands had been completely cut off during the occupation. It was my task to provide for the necessary introductions (I had many relations with scientists and with scientific institutions in England) and to guide the assistants in their work. I also received full freedom to study what had been done in research on social programs. The trust put in me was very gratifying and I was glad to accept the offer. It took some time before all the necessary preparations were completed; I went alone during the month of November 1945, and then from January 1946 until the end of March a group of 12 assistants came over with me: all very capable young persons, who had been carefully selected.

Our mission was a great success. We were introduced to the National Physical Laboratory in Teddington and to a great number of university laboratories; we had contact with the Department of Scientific and Industrial Research, and with the Patent Office; we went to Cambridge, Manchester and other universities; everywhere colleagues and officials helped us and a large number of references, abstracts and data was brought back to Holland. The Netherlands Government provided an extra grant for the purchase of scientific periodicals, in particular back numbers which were missing in Holland; and the British "Help Holland Council" gave us a grant of £b 2000 for the purchase of scientific equipment. Much goodwill was obtained.

Although Holland had been cut off from contact with other countries, it turned out that our work during the occupation had not been unfruitful. In my own area of aerodynamics I found that some of the questions with which I had occupied myself, were of interest to foreign scientists. I was asked to give some scientific lectures.

Gradually this drew me back to my original domain of work, and although I gathered interesting information and research on social problems and on various questions of education, science began to regain the heavier weight.

Upon coming back to Delft, it appeared that the Netherlands Navy, engaged in dismantling some submarines and other ships, was willing to offer part of the equipment to several laboratories of our University, and my laboratory in particular could get high-pressure equipment. Moreover, I was told that I could get additional laboratory space, while at the same time the staff of the laboratory would be enlarged. This opened perspectives which required due consideration, and since the new developments of high-speed flow interested me very much and offered a promising topic for theoretical as well as for experimental work, I felt all the attractions of a scientific occupation coming back to me. Since at that time all my collaborators in the Research Center project had departed on their various ways, I took the unavoidable decision: I returned fully to my University work.

(Although not directly connected with the history of the "Research Center", it may be of interest to mention that during the last winter of the occupation several more projects for the creation of new organizations had been made. One of these was the "Nationaal Instituut" for the purpose of making men and women more conscious of our cultural heritage. This "Nationaal Instituut" obtained large government grants and a sumptuous building in Amsterdam to organize its work. Our "Research Center" had personal ties with this institute and there was a kind of agreement concerning cooperation; however, the "National Instituut", was not directed at research. The "Nationaal Instituut" was dissolved a few years after the liberation).

I am convinced that the only effective way to protect human society against the spread of disruptive trends of thought, as Nazism, the present form of communism, race conflicts and what further may develop, must be (a) helping those peoples and those social groups which suffer deprivation, and (b) extensive research into man's reactions to changes of his environment, to the increase of knowledge and to the tremendous increase of technical power, including attention to influencing thought and emotional life.

If we do not embark on such research on a much wider scale than is currently pursued, with determination to give full attention to its results, there will always reappear maladjustments with the consequence of misunderstanding, fear and outbreak of destructive instincts.

I have done my best to organize and to stimulate such research, in discussions, in letters, in articles, and in the way I have described in the preceding pages. That is what I did in order to cope with the destructive trends that appeared in communism and elsewhere. Had I found active cooperators, not tied down by other work, I might have achieved something, either in my own country or in some international form. I went as far as I could with the means at my disposal and gave a great deal of my time to it. In this way I have tried to do something much more useful than would have been the case if I had initiated some action against the communist party in Holland after I had left it. I left it without scars, as I mentioned before, but not with indifference. As far as I could stretch my time, I have tried to prepare myself for a better understanding and for finding means to help others towards understanding. At last, however, I had to come to the conclusion that I could better restrict the application of my abilities to science.

Since then I have given all my time to my task at the Technical University and to the scientific work connected with it. As mentioned before, I was secretary of the International Union of Theoretical and Applied

Mechanics from 1946 until 1952. I arranged various symposia and took care of many publications while publishing at the same time my own theoretical research. I was also secretary of an international committee for rheology (problems of viscous flow and of plasticity) and took a leading part in the organization of the first International Congress of Rheology in Scheveningen, Holland, in 1948.

I was elected vice-president of the section of sciences of the Royal Netherlands Academy of Sciences in 1947, a post from which I resigned in 1950 in connection with my impending stay in the United States from August 1950 to August 1951. I received honorary doctor's degrees in recognition of my work in 1948 from the "Université Libre de Bruxelles" and in 1950 from the Université de Poitiers, France.

Finally, I shall make some remarks about my connections with the United States. I have already mentioned my first visit to California in 1931 upon the invitation of Dr. W.F. Durand from Stanford University.

In 1938 I wanted to attend the Vth International Congress of Applied Mechanics in Cambridge, MA, and Professor Biezeno and I had planned to make a trip to the West Coast after the Congress. On my arrival in Baltimore, however, I was called back by telegram in connection with a serious illness of my wife, which obliged me to return home immediately. She died in August 1939.

After the war new contacts with American scientists were made at the Congresses for Applied Mechanics in Paris (1946) and London (1948).

In 1949 I was invited by the Naval Ordnance Laboratory in White Oak, Silver Spring, MD to participate in the dedication ceremonies for the new supersonic wind tunnels and to give one of the lectures scheduled for that occasion. I took as subject "Boarderline regions between aerodynamics and physics." I also worked at the laboratory as a consultant for twelve days. During a short visit to the California Institute of Technology

at Pasadena, I was invited to stay again in that institute in the academic year 1950-1951 for a period of six months to work in the Hydrodynamics Laboratories collaborating in a project concerned with turbulent fluid motion and to give a series of lectures.

I also received some other invitations in addition to the one from the California Institute and the possibility was foreseen for staying in the U.S for an entire year. I had married again in 1941 and Mrs. Burgers and I sailed for New York in August 1950. We spent a few days in New York and Brooklyn; then we went to Cornell University, Ithaca, NY, to give a lecture and to do some work in the Department of Aeronautical Engineering. From Cornell we went to Cambridge, MA, to participate in the International Congress of Mathematicians, as a delegate both from the Royal Netherlands Academy of Sciences and from the International Union of Theoretical and Applied Mechanics. This was followed by a short stay in Providence, RI, to take part in a Symposium on Plasticity at Brown University. From there we went to Washington, DC.

I then had to take part in a symposium on problems of motion of the Earth's crust, which as mentioned before, had been arranged upon my recommndation by the International Union of Geodesy and Geophysics. I further worked for some time as a consultant at the Naval Ordnance Laboratory in White Oak, MD, and gave a lecture at the Brooklyn Polytechnic Institute and one at the Department of Aeronautics of the John Hopkins University in Baltimore.

On October 15 we left Washington DC and visited Dayton, OH, where I had to discuss some arrangements at the Central Air Documents Office referring to the printing of the proceedings of the first Symposium on Cosmic Aerodynamics. I gave lectures at Indiana University in Bloomington, IN, at the State University of Iowa, Iowa City, IA, at the University of Utah in Salt Lake City, UT. After a short visit to Death Valley

and to the Hoover Dam, we arrived in Pasadena on November 1, 1950.

During my stay in Pasadena I worked daily in the Hydrodynamics Laboratories of the California Institute of Technology, gave a series of lectures, and took part in many seminars. On several occasions we were taken out by colleagues on trips or on larger excursions. I also was twice invited to give lectures at the Naval Ordnance Test Station, Inyokern, CA. In addition, I gave a lecture at Stanford University and one at the University of California, Berkeley, CA.

We left Pasadena on May 27 1951, visited the Grand Canyon, Flagstaff, Monument Valley in Utah, Santa Fe; spent one day in Denver and then went to Chicago, where I had to take part in the first U.S. National Congress for Applied Mechanics. From Chicago we returned to Washington DC. I had to give some lectures at the University of Maryland. Thereafter, we again went to Cornell University for work as a consultant at the Aeronautics Department; to Baltimore for a lecture and to work at the Aeronautics Department of the Johns Hopkins University; and to Langley Field, VA, for a lecture. After a further stay in Washington and work as a consultant at the National Bureau of Standards, we spent the last week in Brooklyn and sailed home in the end of August 1951.

Already during the Congress in Chicago, there had been discussions about a lasting connection with the University of Maryland (I know that the California Institute of Technology was likewise interested in having me). Soon after my return in Holland, I received an offer for a full professorship, which would make it possible for me to reside permanently in the United States.

Travelling through the United States and the many contacts we had made with people in that country had made a deep impression on Mrs. Burgers and on myself. We had obtained an understanding of

many aspects of life in that country and of its problems. We have been struck by the openness with which problems are discussed, and by the freshness of outlook and the eagerness to do things. We have been impressed very much by the way in which the problems of education are attacked and are held in the center of attention. There is an interest for experimenting, for looking towards new ways and a readiness to do work on those, which is extremely refreshing and which gives great hope for advance. After our return home we have talked about this with many of our acquaintances; Mrs. Burgers also gave lectures to some small groups. Moreover, we have been profoundly impressed by the great friendship, hospitality and courtesy with which we have been received everywhere and we have learned to love this country very much.

Before our marriage, which as mentioned took place in 1941, Mrs. Burgers had worked for 13 years in Rotterdam as an official of the Society for Fighting Tuberculosis. After our marriage, she became interested in childcare, in particular related to children who cannot be taken care of by their parents or whose parents have been deprived of the rights of parenthood by a court decision. Until 1950 she worked in Delft with the society "Tot Steun", a protestant society for taking children in foster care. I mention this, because during our stay in Pasadena Mrs. Burgers succeeded in making contact with organizations of a similar nature in the United States and she assisted at a regional congress on childcare in Pasadena in April 1951. She took part in the discussion and was invited to give an informal talk about childcare in Holland. She was also invited to visit some children in their foster homes. Having come across a book by Mrs. Ann Perrot Rose, "Room for One More", which treats the problem of taking foster children into one's family in a very clear and attractive way, she decided to translate this book into Dutch. After a publisher had been found and the necessary copyright permit was obtained from Mrs. Rose, the book has appeared in Holland under the title: "Er kan nog meer bij" (D. van Syn en Zonen, Rotterdam, 1952); 1800 copies have been sold.

One can see that both of us have done our best to promote a mutual understanding and respect between Holland and the United States.

Both of us love our country and the surrounding countries of Europe; we have seen much of England, France, parts of Switzerland, Italy, Germany, and Denmark; in 1952 we were in Istanbul. What Holland and Europe have given to us is firmly anchored in our minds. Both of us have given all our energies to work in the interest of our country. Our connections with Holland and Europe will never be lost.

However, if, after having been in Europe for so many years, the opportunity would be granted for us to share the life of the United States, we would consider this as a great happiness. It would widen our horizon and we would like to collaborate with the Americans towards the realization of their ideals, so that we may become good and helpful inhabitants of that country.

Epilogue

The ideal of communism as it presented itself around 1918 promised to open a road towards a better understanding of social problems and towards widely improving conditions of living and of education, in a world deeply wounded by the bloody struggles of World War I. It was hoped that the revolution in Russia had liberated great forces which would bring immense progress in the world. What has happened in Russia, after an initial period which had looked promising, has blurred the ideal of communism so much, that it is totally impossible to reconstruct it. If I would be asked to state my opinion about communism, I shall therefore be unable to give an answer, unless a clear definition is provided of what is meant by the word.

However, comments can be made with respect to two forms in which this word is used:

Concerning the results of the so-called "communist government" in Russia, I am fully prepared to say that it has failed in realizing any of our hopes; instead of bringing freedom to man, it has destroyed freedom of discussion, freedom of expression and freedom of thought to such a degree that it will take a long time before the damage can be repaired. I do not even see reliable signs of a proper beginning towards such a restoration. Concerning the activities of the communist parties, in so far as I can judge from the available information, nowhere do they give any evidence of independent constructive thinking. I am speaking here about the present situation; it was different 30 years ago. Nor do I know anything about trends in Yugoslavia. I add that in view of what has happened in Russia, no sensible and honest man can preach that adherence to Russian and to Russian thinking can bring happiness. To destroy social relations in democratic countries with the purpose of helping the policy of the present Russian Government is an activity that I fully condemn.

Appendix

Translation of my letter of resignation from membership of the communist party: Delft, 22 September 1933. To the Bureau of the division Delft of the CPH.

Dear comrades,

Here I inform you that I wish to end my membership of the CPH. Actually I have not taken part in the work of the party already during a very long time and neither is it possible for me to do this in the future. It seems better to prevent that a false situation will result. J.M.B.



APPENDIX III

J.M. Burgers, Addition to the "Statement concerning the development of my political opinion" of March 1954 (submitted to the US Consulate in Rotterdam in February 1955)

In my statement of March 1954 I wrote "after my return (from Russia) I have mentioned these things (i.e., what I had read and heard about matters connecting with the trial that started in 1936), often in private conversation".

It seems to have been surmised that the words "I have mentioned" referred only to the period immediately following my return home.

I have spoken, however, about these matters also after the war, in particular since the publication of the well-known book by A. Koestler, 'Darkness at Noon", which I have read. On such occasions I have pointed out that certain points in this book have resemblance with things I had heard about when I was in Russia, so that I have the impression that several aspects of what Koestler wrote in his book must be genuine. What I have heard myself in 1936 was much less cruel than the things Koestler tells about, but when speaking about these matters I have added that a development in the direction of Koestler's statements might well have occurred under circumstances prevailing in Russia, in particular if one thinks of what has happened in German concentration camps.

I have spoken about these matters not only with friends in Holland, but also several times when being abroad, for instance, when talking with some French colleagues at Poitiers in 1950, with a British colleague at a meeting in Naples in 1954, and quite recently again with Belgian colleagues in January 1955. There must have been many more such occasions.

In such conversations I also have often expressed my feelings about a striking point in the report on conditions in Russia, published by the American author H. Shapiro and translated in the beginning of 1954 in one of our leading newspapers. Shapiro mentions that after the release of the medical doctors who had falsely been accused of treason and after the trial of Beria, "a wave of relief spread through the country". In any Western country one would have expected an outburst of indignation against former rulers who have made use of such methods as Beria's people had done. I have drawn attention to the fact that the absence of a wave of indignation, and the mere statement that there was a feeling of "relief", in my option demonstrated how far the Russian people had been influenced by terrorism.

I have followed the debate on the subject of genetics in Russia, which attracted the attention of the scientific world some years ago. Amongst the articles I have read on this matter, there was a reply made by a Russian professor, Prof. N.I. Nuzhdin of Moscow, in the British scientific weekly journal "Nature", vol. 165, pp. 704-708, May 6 1950 – the reply referred to articles written by Prof. J. Huxley and by others. I found that Nuzhdin's article presented a style and a method of argumentation that condemns itself, using invectives that make it impossible for any scientist to take it seriously. The fact that a domain of science has got into such a state proves that a stupid dogmatic rule has gained an influence in Russia that I consider as absolutely intolerable. I have spoken about this with colleagues, but it is hardly necessary; we all are of the same opinion.

There is no doubt in anybody who knows me, nor within myself, about my feelings with respect to what we have heard about the methods used by the Russian communist party against the Russian people, about the deportation to Siberia, or about the handing over of German political refugees from the Nazi-regime to the German authorities, which occurred after the Russian-German pact of 1939. Nor is there any doubt about my feelings with respect to the communist parties in countries outside Russia, which still advocate allegiance to a doctrine, so much soiled and made into a betrayal of good faith by what has happened.

In Holland, however, it would have made no sense for a person in my position as a scientist, to start to broadcast any of these feelings. I am respected and trusted for my balanced judgment, and this puts upon me a responsibility to be careful about what I say. I know only very little about Russia from direct observation, and that refers to a period which now is long past. Most of what comes to us about present day conditions in Russia arrives through the press or through books written outside Russia. I can try to judge some reports on their intrinsic probability; I can discuss them with other people; but as a scientist I cannot go further than that. I am not acquainted with conditions in Russia as they are developing now and we must reckon with the possibility that there may come a change to greater freedom. What I observed of the Russian people during my visits to that country, show me that they are not different in character from us and that they are capable of great sympathy. There is much to be loved in their country when one abstracts from party activities, and I have not lost all confidence that within the Russian people our ideals of freedom will again evoke a strong response, so that they can influence their rulers to recognize them and to act according to these ideals. It is our task not to close the way for arriving at an understanding of each other's points of view.

I have no inside information about the activities of the communist party within any Western country. I can qualify as silly or as stupid public utterance or proposals for certain actions, but I must leave it at that.

So much of debates on these matters is not based on facts, but on convictions arising in unknown ways. My task as a scientist is not to be carried along by expressions of feelings that cannot be properly verified.

In Holland we cannot forget deeds that have been committed by Germans before and during the war. Now we must consider Germany as an ally, but, while we appreciate all efforts that are made to create a better social atmosphere in Germany, there still are indications, not less probable than the reports we have heard about Russia, that the purge from Nazi influence has not been complete. And even in other Western countries the desire for economic gain or racial feelings from time to time have developed to such extremes that cruelty and severe loss of personal freedom for many men and women was the result. If in my position I should start a campaign against communism, I would be obliged to speak also about these other matters and I should soon land in a position where I would lose track and there would be an end to my independent judgment. This would also put an end to doing properly my work in fluid mechanics, which is the work I am charged with (and for which, by the way I have been invited to come to the United States).

There is a genuine ideal of righteousness and fairness, which makes that cases of greed and of oppression, still possible under democratic rule, are keenly felt. Errors made by leading groups in reacting to such cases can be very effective propaganda for communist sympathies and what criticisms communists have made regarding such matters is not necessarily always false. It would be a very grave error to attempt to kill a tread of thought completely: thoughts are not packed in tight compartments, but are ramified over wide domains of feelings. We recognize the dangers for mankind of the methods practiced by the communist parties, but we know that extensive movements attempting to standardize ideas and endangering freedom of thought may arise also from wholly opposite sides. Happily such movements can still be combated in our Western countries, but this is possible only when sufficient freedom of expression is

left to opposing views. Once it were believed that some trend of thought should be eradicated completely, it is quite certain that this would destroy all freedom of thought.

The world cannot be painted in black and white only. Some of the best aspects of honesty and even of life are killed, when one should be obliged to present opinions in a prescribed form, and even more, when one should have to broadcast them.

The only sure way of combating destructive tendencies in the social structure, amongst them the destructive effects of what is happening in social life. This was the purpose of the "Research Center for Social Problems", which I attempted to create during the end and just after the war (see my "Statement" and the deed of creation of that Center before a notary public, annexed to the Statement). The program of the "Research Center", the investigations which were started, and the support given by people who since then have occupied foremost positions in our country, show very clearly that the spirit of this work was quite different from that of communists or of Marxists. That this Research Center did not become effective publicly, was caused by the fact that all the cooperators after the liberation were called to other duties (see "Statement"). I still consider this attempt as an activity directly against the propaganda of communists, in the sense in which the U.S. law requires.

From my public utterances on the spirit of science in recent years, I can mention the second part of a lecture given before the anniversary meeting of the Royal Netherlands Academy of Sciences at Amsterdam, April 13 1953. In the first part of that lecture I spoke on questions relating to my subject of fluid mechanics; but in the second part I passed over to what science means for us (this was not the first time that I have expressed thoughts on such matters before the Academy). In the past year I was invited to give the opening

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address on "The Value of Science" for the congress on Freedom and Limitations of Science, organized by the Netherlands Universities in The Hague, September 17 and 18, 1954, in connection with a request from Columbia University of New York, on the occasion of its Bicentenary Commemoration.

I may state that in both cases it was a great honor to be called upon to give these addresses. In particular the one before the Congress on Freedom and Limitations of Science was meant to set a keynote to the further lectures and to the discussions that should take place. I was chosen for this task by the Congregation of Deans ("Rectoren") of the Universities, professors themselves, who were well aware of my ideas, who appreciated my way of thinking, and who were acquainted with the fact that I had difficulties with my U.S. visa. That they chose me to speak on the "Values of Science", which necessarily entails the subject of Freedom of Science, may be considered as an expression of their trust in me.

In my address I stressed the freedom individual men and women must enjoy in order to be able to form their thoughts in their own ways, since this gives the only guarantee that all human thinking can be continually subjected to revision and correction, and that again and again it can be interpreted in fresh terms. It is very clear that this requirement is against all party dogmatism of whatever kind.

Coming to the end, I will stress once more, as I wrote in my "Statement", that I fully condemn all attempts at destroying social relations in democratic counties with the purpose of serving the policy of the present Russian government. It will be clear, moreover, from all what I have said in the Statement, as well as in the present pages, that I fully condemn and despise every attempt to betray secrets.

I also mention that in several conversations I have had with colleagues in Holland, I have still expressed my desire to live in the United States in order to work in that country and become better acquainted with

it, notwithstanding the fact that most of these colleagues express their preference for Holland or for Europe, and consider the U.S. as a country lacking in culture. I have drawn their attention to the freshness of ideas, to the development of progressive thought, and to cultural expression in the United States, and I have stated my confidence in the future of this country. So has done my wife. We have loved the country.

Almost every letter I receive from colleagues in the U.S. ends with: "We hope soon to see you in this country."

Finally, I mention that the Royal Netherlands Academy of Sciences in December 1954 has again elected me as one of their three delegates to the meeting of the "International Council of Scientific Unions" in Oslo, Norway, in August of the present year, which Council is the highest body in the domain of international scientific organization.

Delft, February 8, 1955.

Professor dr. J.M. Burgers

(Full professor of aero- and hydrodynamics at the Technical University, Delft)


APPENDIX IV

Remembering Jan Burgers

by J. Herman Burgers

Jan Burgers stood out by the exceptional force of his personality, in at least three respects: by the power of his intellect, the intensity of his feelings, and the strength of his will and energy. If I should characterize him in one expression, I would call him a passionate man; He was also a man of robust physical health, until those very last years when an insidious disease step-by-step demolished his mind and body.

When I say that he was an exceptional person, I should add immediately that he was the son of exceptional parents. His father had only had elementary schooling and worked as a clerk at the railway post office, but he was a man who already in his young years had developed a fascination for the natural sciences and who had acquired a vast knowledge of physics, astronomy, geology, and so on, by sheer self-education. In the provincial town where he lived, he gave lectures year after year on such subjects as magnetism, electricity, optics and astronomy, for audiences of like-minded admirers of the miracles of science. He also assembled in the course of his life an enormous collection of microscopes, physical instruments and many kinds of curiosities, gradually transforming his house into a museum.

Such was the home in which Jan Burgers grew up, together with his brother, Willy Burgers, and several foster-brothers. His intellectual brilliance was recognized in his high-school years, and so it was decided that he should go to college. Having finished high school he had first to learn Latin and Greek, since this was at the time still a requirement for admission to a university. In 1914, nineteen years old, he started his academic studies at the University of Leiden.

There he had the luck of becoming the pupil of a unique teacher, Paul Ehrenfest, an Austrian who had been appointed professor of physics at Leiden in 1912. Ehrenfest was a bosom friend of Albert Einstein and knew many other European scientists; he was thoroughly acquainted with all the latest developments in physics. Under his stimulating guidance, Jan Burgers completed his university studies in three and a half years. His productivity during these few years was remarkable. Besides doing research that resulted in articles that were published in leading European journals of physics, he wrote a fundamental treatise on the then brand-new theory of Ernest Rutherford and Niels Bohr concerning the structure of the atom. The University accepted this treatise as a doctor's dissertation and awarded him the doctor's degree in November 1918.

Even before that, the Technical University in Delft had taken an unusual step and appointed Jan Burgers, 23 years old, as a full professor to do teaching and research in aerodynamics and hydrodynamics. This was one of the few fields of physics to which my father had hardly given any attention during his student years so he had to acquaint himself quickly with the subject before starting his teaching. Soon afterwards, he found his principal mentor in the science of aerodynamics in Theodore von Kármán, a Hungarian, who was at that time professor in Germany and who after the Second World War became the scientific adviser of the United States Air Force.

Jan Burgers and von Kármán became close friends. We can safely assume that the three people who have had the strongest influence on the development of Jan Burgers as a scientist were his own father, professor Ehrenfest, and professor von Kármán.

I shall not go further into Jan Burgers' performance as a scholar of fluid dynamics and applied mathematics, since I am not a scientist myself and, therefore, absolutely incompetent to appraise his merits in that field. But I would like to mention that his scientific interest extended to much more than just his professional field. He was by nature not a specialist but a universalist, and he kept abreast of developments in many fields of modern science. He always had an absorbing interest in its most fundamental problems, such as the structure of the atom and the structure of the universe, quantum physics, relativity, causality, finality, and the origin of life. One problem that fascinated him in particular was that of the relationships and contrasts between the phenomena of life and the phenomena of physics. He felt a need for an encompassing philosophical vision in which those two sets of phenomena would be brought together.

In his forties he became familiar of the American scientist and philosopher Alfred Whitehead, and he became convinced that those ideas came closest to solving the mystery that obsessed him. Ever since, his ambition was to develop Whitehead's ideas into a coherent and comprehensive view of the world. This resulted in several writings and eventually in his book on Experience and Conceptual Activity, which was published in 1965, when he was seventy years old. The lack of response met by these writings must have been one of the gravest disappointments in his life.

From his childhood Jan Burgers had a profound belief in the value of the scientific quest for knowledge. But the urge to know and to understand was only one side of his personality; there also was the

emotional and artistic side. Those who have known him intimately are aware that he was a man of strong emotions. Music was one of his principal emotional outlets. In his musical tastes he was a typical romanticist, as he was in his tastes for novels and short stories. This side of his character also inspired his interest in the visual arts, as well as his intense love of nature. He was fond of walking and hiking, all by himself, in lonely landscapes; and he was a prolific maker of sketches and drawings, in particular of flowers, trees and mountains.

Jan Burgers had an astonishing energy. His scientific productivity was impressive, from his first book that appeared when he was 23, up to his last book, which he published when he was 80. There was something intense and passionate in many things he undertook. For instance, when at some time he took an interest in seaweeds, he accumulated within a few years a huge collection of seaweeds, each of them neatly dried and pressed on cardboard. All his life he was a passionate collector of stones and minerals, and he meticulously registered each piece with its name and the time and the place where he had found it. In general, keeping accurate records was one outlet for his unremitting activism. He made precise notes during all his major travels and he was for many years an enthusiastic photographer. Later on, this was superseded by an enthusiasm for drawing. It is simply incredible how many good sketches he could make during one boat trip on the Hudson or one bus ride in Mexico. Incidentally, it is typical for his activist character that he came to prefer drawing instead of photographing. Likewise, his customary way of experiencing music was not by listening but by playing the piano, although sometimes he could be deeply moved by listening to music played by others.

My father's productivity was the joint product of his energy and his will-power, This will-power gave him his self-discipline that enabled him to concentrate on the aims he had chosen. On the other hand, this

dominating will sometimes put a strain on the members of his family and the assistants in his laboratory. Jan Burgers, who had been much admired already at an early age, took it for granted that those who were close to him would respond to his needs and wishes. At the same time he felt little inclination to adjust himself to the society in which he lived.

This has been a peculiar trait of my father's character during most of his life in Holland. He had grown up in a poor but very unusual household and he had an aversion to the tastes and styles of what he regarded as bourgeois society. He was highly successful and highly esteemed in his work, but socially and politically he felt himself an outsider and he preferred to stay that way. He was a non-smoker and a non-drinker, he did not engage in small talk, nor in any organized sports, and during the nineteen-twenties he was a member of the Communist Party. His capacity to devote himself intensively to various fields of interest was matched by his capacity to completely ignore other fields. His many-sided interests were always addressed to subjects of his own choosing. He eagerly studied, for instance, the geography, history and culture of China, Central Asia and the Western Hemisphere, but he had only limited attention for the history and architecture of the Netherlands.

This does not mean that he was a solitary man. He had enjoyed the family 1 ife with his brother and foster-brothers in his parental home. During his student years at Leiden he made very good friends with his fellow students in the science department, and together they formed a sophisticated but cheerful club in which he played a leading role. In this club he met his first wife, my mother, whose life later on was plagued by prolonged illnesses and who died in 1939. At the University of Leiden he also met Anna, who in 1941 would become his second wife.

In the years after the Second World War my father seemed to become more lonely. Several of his old friends had died, some others had settled abroad. At the same time he got bored of doing his work from 1918 onward in the same job and even in the same street. In these post-war years he paid several visits to the United States, In particular, he and Anna lived a full year in Pasadena, where he worked at the California Institute of Technology. In America he not only found interesting work, but he and Anna also became acquainted with interesting and congenial people. All this contributed to their decision to leave their native country and to start a new life in the United States. In 1955 Jan Burgers laid down his professorship at the Technical University in Delft and entered the University of Maryland.

Emigrating, when you are sixty years old, may seem a very hazardous enterprise! You cut yourself off almost completely from the ties you have formed over the years. How will you ever manage to build a new circle of friends at that age? Miraculously, this is precisely what happened; Jan and Anna really started a new existence and before long they had also made many new friends, not just acquaintances but really intimate friends. After some years in College Park, they were already in regular contact with more genuine friends in America than they were in Holland at the time they left.

Probably the United States is the only place where such a miracle can happen, because American society is so open and so flexible and because it is so accustomed to receiving and integrating people with entirely different backgrounds, I think, even my father's peculiarities were more readily accepted in the US than they were in the stiffer social climate of Holland, It may sound paradoxical, but I am sure my father was a better integrated member of society during the American part of his life than during the Dutch part.

The fact that Jan Burgers succeeded so well in taking root in his new country is something he owed partly to himself and partly to his wife, Anna, who was an excellent complement to his personality by her independent character, her practicality, and her keen personal interest in people. Whereas in the beginning it was, of course, mainly Jan Burgers' professional contacts that brought them into touch with new friends, she soon brought in friends of her own. It was on her initiative that they entered the Unitarian Church. This proved to be a most valuable step, because there they found themselves members of a community of likeminded people who responded to my father's needs for intellectual and personal contact. I am deeply grateful for the friendship he experienced in that community. I am particularly thankful for the sense of solidarity displayed by those Unitarian friends who gave tireless help to my parents when their life became clouded by my father's creeping disease.



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