Interview With Dirk Jan Struik

David E. Rowe

Dirk J. Struik was born in Rotterdam in 1894, where he attended the Hogere Burger School from 1906–1911 before entering Leiden University. At Leiden he studied algebra and analysis with J. C. Kluyver, geometry with P. Zeeman, and physics under Paul Ehrenfest. After a brief stint as a high school teacher at Alkmaar, he spent seven years at Delft as the assistant to J. A. Schouten, one of the founders of tensor analysis. Their collaboration led to Struik's dissertation, *Grundzüge der mehrdimensionalen Differentialgeometrie in direkter Darstellung*, published by Springer in 1922, and numerous other works in the years to follow.

From 1923 to 1925 Struik was on a Rockefeller Fellowship while studying in Rome and Göttingen. During these years he and his wife Ruth, who took her degree under Gerhard Kowalewski at Prague, met many of the leading mathematicians of the era—Levi-Civita, Volterra, Hilbert, Landau, et al. After befriending Norbert Wiener in Göttingen, Struik was invited to become his colleague at M.I.T., an offer he accepted in 1926. He taught at M.I.T. until his retirement, except for a five-year period during the McCarthy era when he was accused of having engaged in subversive activities. He has also been a guest professor at universities in Mexico, Costa Rica, Puerto Rico, and Brazil.

Beyond his work in differential geometry and tensor analysis, Professor Struik is widely known for his accomplishments as a historian of mathematics and science. His *Concise History of Mathematics* (recently reissued with a new chapter on 20th century mathematics) has gone through several printings and has been translated into at least sixteen languages. His *Yankee Science in the Making*, a classic account of science and technology in colonial New England, is considered by many to be a model study of the economic and social underpinnings of a scientific culture. As one of the founding editors of the journal *Science and Society*, Professor Struik has been one of the foremost exponents of a Marxist approach to the historical analysis of mathematics and science. At the present time he is completing a study on the history of tensor analysis while working on his autobiography. He is a passionate devotee of Sherlock Holmes.

This interview is excerpted from a December 1987 conversation.

Rowe: *You entered the University of Leiden in 1912 with the intention of becoming a high school mathematics teacher. What made you change your mind and how did you manage to break into the academic world?*

Struik: The man who enabled me to enter academic life was Paul Ehrenfest. Ehrenfest was born in Vienna and studied in St. Petersburg and Göttingen. He and his Russian wife, Tatiana (Tanja), had made a name for themselves with their book on statistical mechanics. It was the first work to take into account the achievements of Boltzmann and Gibbs, a great step forward at the time. In 1912 Ehrenfest was appointed professor of mathematical physics at Leiden, succeeding the great H. A. Lorentz. Ehrenfest felt greatly honored to serve as Lorentz's successor, but he was dismayed by the stiff formality of the Leiden academic world where students only saw their professors in class and half the student body disappeared by train before sundown. Having come from Göttingen, he was greatly influenced by the atmosphere there, and so he implemented some of the same reforms that Felix Klein had introduced. One of these was the mathematical-physical library, the *Leeskammer*, which on Ehrenfest's instigation was housed in Kamerlingh Onnes's laboratory. There students could browse

Dirk Struik during his student days.

through a wide variety of books rather than being confined to picking out a few at a time from the dusty university library. Just like in Göttingen, the *Lees-kammer* proved to be a central meeting spot for students and faculty alike, and before long there was considerable intermingling between them.

Rowe: *Were physics and mathematics closely allied fields at Leiden?*

Struik: To a considerable degree, although perhaps no more than elsewhere at this time. This was of course a period in which revolutionary changes were taking place in physics, and Leiden was one of the leading centers in the world with Lorentz, Ehrenfest, and Kamerlingh Onnes. Lorentz and Kamerlingh Onnes were Nobel Prize winners. The latter presided over his cryogenic lab where he ran experiments on the liquification of gases under low temperatures; only shortly before this he had discovered superconductivity. Lorentz was by now curator of Teyler's Museum in Haarlem, but he came to Leiden once a week to lecture on a variety of subjects from statistical mechanics to electrodynamics, all in his serene and masterful way. It was often said that his lectures were full of pitfalls for the unwary, as he had a way of making even the most difficult things look easy. We heard other esteemed visitors from around the world—Madame

Curie, Rutherford, Einstein—so it is little wonder that the most talented students were attracted to physics. I never felt quite at home in this field; I was always more adept at thinking in terms of mathematical and especially geometrical concepts.

Rowe: *Did Ehrenfest's ideas influence you in any definite way beyond the impact of his personality?*

Struik: Oh indeed, he himself had been influenced by Felix Klein's views, which stressed the underlying unity of ideas that were historically unrelated, like group theory, relativity theory, and projective and non-Euclidean geometry. The way he taught statistical mechanics and electromagnetic theory, you got the feeling of a growing science that emerged out of conflict and debate. It was alive, like his lectures, which were full of personal references to men like Boltzmann, Klein, Ritz, Abraham, and Einstein. He told us at the beginning that we should teach ourselves vector analysis in a fortnight—no babying. Ehrenfest's students all acknowledge how much his method of exposition has influenced their own teaching. I remember a digression he once entered into on integral equations that I later used in my own course. He also recommended extracurricular studies; in my case he advised me to study group theory (again Klein's influence) together with a fellow pupil. I once asked Ehrenfest what was then one of the difficult questions of that day: whether or not matter exists. He proceeded to explain not only the status of matter as of 1915 (when $E = mc^2$ had just been put on the map), but also how the facts of sound and electricity tie in with the three dimensions of space, noting that if the Battle of Waterloo had been fought in a two-dimensional space we would be able to detect the sound of its cannon fire even today.

Rowe: *Who were some of the other students you got to know in Leiden?*

Struik: There were several whom I got to know quite well, especially through our scientific circle "Christiaan Huygens." One of the most outstanding was Hans Kramers. He, too, came from Rotterdam, but he attended the *Gymnasium,* so we did not know one another in high school. He later took his Ph.D. under Niels Bohr at Copenhagen and eventually succeeded Ehrenfest at Leiden. Another was Dirk Coster who also studied under Bohr and co-discovered a new element (Hafnium—Hafniae is the Latin for Copenhagen). He later returned to the Netherlands and became professor of physics in Groningen.

Rowe: *What did you do after graduation?*

Struik: My stipend had run out so I had to look for

work, which was not difficult to find in the summer of 1917 with so many young fellows tucked away in garrison towns. I took a job as a teacher of mathematics at the H.B.S. (high school) in Alkmaar, twenty miles north of Amsterdam. But in November I received a letter from Professor J. A. Schouten in Delft inviting me to join him as his assistant there. Schouten was by training an engineer, but he eventually succumbed to his love of mathematics. His doctoral dissertation, which was published by Teubner, dealt with the construction and classification of vector and affinor (tensor) systems on the basis of Felix Klein's Erlangen Program. After some soul-searching, I decided to accept his offer, and I ended up spending the next seven years in Delft. The salary was less than at Alkmaar, but it gave me a wide-open window on the academic world.

Rowe: *It must have been an exciting period to work on tensor calculus.*

Struik: It surely was. Schouten had shown that an application of the ideas in Klein's Erlangen Program could lead to an enumeration not only of the rotational groups underlying ordinary vector analysis, but others like the projective and conformal groups for any number of dimensions. Schouten's formal apparatus was algebraic, but it was accompanied by suggestive geometric constructs. We now know, of course, that Elie Cartan was working on related problems from a different point of view. With his great mastery of Lie group theory and Darboux's *trièdre mobile*, Cartan was able to dig deeper and obtain his own results with an almost deceptive elegance. But none of us knew of Cartan's work in 1918; his fame came much later. Schouten's work appealed to me first because of its close ties with Klein's Program, which was already familiar to me through Ehrenfest, and secondly because of its close connection with Einstein's general theory of relativity. It was not just the formal apparatus of tensors that interested me, it was the dialectics involved. For Klein, these were the interplay between complex functions, Euclidean and non-Euclidean geometry, continuous and discontinuous groups, Galois theory and the properties of the Platonic solids, et al. For Einstein, his field theory established connections between geometry, gravitation, and electrodynamics.

Rowe: *To what extent was Schouten's mathematics related to recent developments in Einstein's theory?*

Struik: At the time I joined him in Delft he was busy applying his ideas to general relativity theory, i.e., the direct analysis of a Riemannian space of four dimensions. The algebra involved was fairly simple, but the differentiation required new concepts because the curvature is non-zero. Schouten was able to introduce co-variant differentiation on such a space by considering what he called geodesically moving coordinate systems. This enabled him to introduce new structure into the already existing tensor calculus utilized by Einstein. It was top-heavy with formalism, but Lorentz took an interest in it and helped to see that it was published by the Dutch Academy of Sciences. One day in 1918 Schouten came bursting into my office waving a paper he had just received from Levi-Civita in Rome. "He also has my geodesically moving systems," he said, "only he calls them parallel." This paper had in fact already been published in 1917, but the war had prevented it from arriving sooner. As it turned out, Levi-Civita's approach was much easier to read, and of course he had priority of publication. But few people realize that Schouten barely missed getting credit for the most important discovery in tensor calculus since its invention by Ricci in the 1880s.

Rowe: *You must have had a good working relationship with him.*

Struik: Yes, though Schouten was neither an easy chap to work with nor to work for, but we had few difficulties, especially after I outgrew the position of being merely his assistant and became his collaborator and friend. I certainly learned a great deal from him; especially the combination of algebraic and geometric thinking typical of Klein and Darboux. Our first common publication appeared in 1918; it investigated the connection between geometry and mechanics in static problems of general relativity. Thus it accounted for the perihelion movement of Mercury, then a crucial test for Einstein's theory, by a change of the metric corresponding to a corrective force.

Rowe: *When did you complete your doctoral thesis?*

Struik: Originally, I planned to write my dissertation with Kluyver at Leiden on a subject in algebraic geometry, either on the application of elliptic functions to curves and surfaces, or a topic related to the Riemann-Roch theorem in the spirit of the Italian and German schools. De Rham's work appeared shortly afterward, revealing that there was a future in this field of research, especially since he showed how one could utilize concepts from tensor analysis. But in 1919 I was not aware of these possibilities, and anyway I had become increasingly occupied with tensor calculus through Schouten. I therefore arranged to have W. van der Woude, the Leiden geometer, as my thesis advisor, although the actual work grew out of my collaboration with Schouten on the application of tensor methods to Riemannian manifolds. I finally completed my thesis in 1922 and received my Ph.D. in July of that year. It was written in German and published by Springer in Berlin. The title was *Grundzüge der mehrdi-*

Jan Arnoldus Schouten

Hermann Weyl criticized the "orgy of formulas" that Schouten produced using his direct methods. Struik recalled how after the appearance of Schouten's *Grundlagen der Vektor- und Affinoranalysis*, Weyl remarked: "I would like to throttle the man who wrote this book."

mensionalen Differentialgeometrie in direkter Darstellung. Following a time-honored tradition, I paid for the book myself, which was an easy proposition in 1922. The inflation in Germany was such that it is entirely possible that the little party I threw afterwards for family and friends cost me more in guilders than the whole dissertation of 192 pages.

Rowe: *I believe it was around this time that you and your wife first met.*

Struik: Yes, Ruth and I met at a German mathematical congress in 1922 and were married in the ancient Town Hall of Prague in July of the following year. She had a Ph.D. from the University of Prague, where she had studied under Georg Pick and Gerhard Kowalewski. Her thesis was a demonstration of the use of affine reflections in building the structure of affine geometry, a new subject at the time. After our marriage we settled in Delft for a brief time before travelling to Rome on a Rockefeller Fellowship. We spent nine months there while I worked with Tullio Levi-Civita.

Rowe: *What sort of a man was Levi-Civita?*

Struik: He was short and vivacious; his manner combined great personal gentleness and charm with tremendous will power and self-discipline. He was then about 50 years old and at the height of his fame as a pure and applied mathematician. His internationalist outlook derived from the ideals of the Risorgimento. His wife was a tall blonde woman of the Lombard type who was equally charming and graceful. She had been a pupil of his and was now his faithful friend and devoted companion; they had no children.

Rowe: *Did you learn a lot from him about tensor calculus?*

Struik: No, not really. In Rome he suggested that I should take up a new field. He showed me a paper he had recently published on the shape of irrational periodic waves in a canal of infinite depth and asked if I would like to tackle the same problem for canals of finite depth. It involved complex mapping in connection with a non-linear integro-differential equation to be solved by a series expansion and a proof of its convergence. Even though Levi-Civita's methods appeared applicable to this case, the problem was far from trivial. It also appealed to me, as I liked to test my strength in an unfamiliar field.

Rowe: *Did he give you any further guidance with this problem, or was he too busy with his own affairs?*

Struik: I had the benefit of seeing him often, either at his apartment in the Via Sardegna or at the University near the Church of San Pietro in Vincoli where I often had a look at Michelangelo's Moses, which I greatly admired. The Leiden philosopher Bolland once said that the Moses remains gigantic even in the smallest reproduction. Yes, Levi-Civita was one of those persons who in spite of a busy and creative career always seemed to find time for other people. He was remarkably well organized. I can still hear him saying, after I asked him to write a letter for me, "Scriveró immediatemente"—and he did.

Rowe: *How did your work on canal waves come out?*

Struik: It went well, and I was able to bring it to a successful conclusion. Abstracts of it were published in the *Atti* of the Accademia dei Lincei, and later the full text came out in *Mathematische Annalen*. It was evidently read and studied, and later the theoretical results were experimentally verified by a physicist in California.

Rowe: *Who were some of the other interesting figures you met during your year in Italy?*

Struik: On the floor above Levi-Civita's apartment lived Federigo Enriques, who was known for his research in algebraic geometry and the philosophy of science. When he heard that Ruth had graduated with a thesis in geometry, he invited her to prepare an Italian edition of the tenth book of Euclid's *Elements*. She accepted, and spent much of her time preparing the text with modern commentary. Maria Zapelloni, a pupil of Enriques, corrected her Italian. It was published along with the other books in the Italian edition of the *Elements*. Besides Enriques, there were a number of other prominent mathematicians whom I

met either at the university or at small dinner parties thrown by Levi-Civita and his wife. There was gentle Hugo Amaldi, who was then writing a book on rational mechanics with Levi-Civita. Then there was Guido Castelnuovo with his strong Venetian accent, and (but only at the university) the grand old man, Senatore Vito Volterra, President of the Accademia dei Lincei. I followed his lectures on functional analysis, which were largely based on his own researches. His delivery was impeccable, a style that reminded me of Lorentz's lectures. Volterra was a senator, as was Luigi Bianchi, who came to Rome from Pisa for sessions of the Senate, and whose books and papers had been among my principal guides in differential geometry. On a day excursion with the Levi-Civitas we also met Enrico Fermi, but since he was a physicist we saw little of him thereafter. Little did we imagine that he would one day be a man of destiny, a real one, not like the fascist braggart known as "Il Duce."

Rowe: *It seems that Italian mathematicians took a fairly active role in politics from the time of Cremona and Brioschi.*

Struik: Indeed, political involvement was not uncommon among Italian scientists ever since the Risorgimento. The ones I knew were all anti-fascists with the sole exception of Francesco Severi, another outstanding algebraic geometer. On the other hand, their antipathy towards the Mussolini regime was not a militant one, so far as I could see. Volterra was an exception in this regard. He and Benedetto Croce actively attacked the regime from their seats in the Senate. After 1930 Volterra was dismissed from the University and stripped of his membership in all Italian scientific societies. The same thing later happened to Levi-Civita. To the honor of the *Santa Sede*, he and Volterra (both of whom were Jews) were soon thereafter appointed by Pope Pius XI to his Pontifical Academy.

Rowe: *What was the political atmosphere like during your stay in Rome?*

Struik: One could not help be aware of fascism with all the blackshirts strutting through the streets of the city, but the political climate was relatively mild in those days, at any rate compared with what came later. The murder of Giacomo Matteotti, the Socialist opposition leader in the parliament, was still fresh in everyone's mind, and the resulting crisis in the government was very much unresolved. Mussolini tried to disavow the murder and tighten police control, but his dictatorship was off to a shaky start. Opposition papers could still appear, even the Communist *Unità*. I was able to establish a contact with one of their contributors, and I used the information he passed on to

me to write an occasional article for the Dutch party paper, *De Tribune*. I do not believe that I ever saw Mussolini in person, despite his high visibility. He used to parade around on horseback in the Villa Borghese, but I had too much contempt for the sawdust Caesar to go out of my way to see this spectacle.

Rowe: *I guess Rome had plenty to offer mathematically in those days. Were there many other foreigners who came to study or visit?*

Struik: Yes, there were other Rockefeller fellows in Rome, and we struck up an amiable acquaintance with Mandelbrojt and Zariski, both of whom went on to become famous in their respective fields of research, Mandelbrojt at the Sorbonne and Zariski at Harvard. Mandelbrojt had worked on problems in analysis with Hadamard, and Zariski was studying algebraic geometry. Paul Aleksandrov, the Russian topologist, also spent some time in Rome. At that time he especially enjoyed the relative luxury of Italy after enduring the many privations in his homeland, which was just recovering from hunger and civil war. He told us that to do topology in Russia at that time you had to convince the authorities that it was useful for economic recovery. So the topologists told them that their field could be of service to the textile industry. Aleksandrov admired my winter coat, and when he learned that I had bought it with money from my stipend he dubbed it the "paletot Rockefeller."

Rowe: *It sounds as though all in all you had a splendid time in Italy.*

Struik: Yes, we grew very fond of life there and enjoyed many memorable experiences. I remember visiting the Vatican on Christmas Eve to witness the opening of the Anno Santo, the Holy Year 1925, in which one could receive special indulgences. The enormous basilica was filled with throngs of worshipers who had come to see the pope. He entered through a special door, the Porta Santa, in an ornately decorated chair carried high on the shoulders of selected members of the papal nobility, while the crowd shouted: "Viva il Papa!" Another occasion I recall occurred at a meeting of the Accademia dei Lincei. Levi-Civita often took us to these sessions held in its ancient palace on the left bank of the Tiber. On this particularly ceremonious occasion the Academy was visited by *Il Re* Vittorio Emmanuele and his still handsome Queen, Helena of Montenegro, once a famous beauty. He had short legs—*eine Sitzgrösse* as the Germans would say—but both majesties did very well on their decorous chairs, he with a bored face listening to the speeches. After the ceremony one was permitted to go up and shake hands with the monarchs, and I was amused to see how the Americans in the

audience crowded around them. I preferred to sample the pastry and sherry instead, and I was pleased to see that Levi-Civita also kept his distance from the Presence.

It is said that when someone once asked Einstein what he liked about Italy, he answered, "Spaghetti and Levi-Civita." I felt pretty much the same way. I also learned to like, with some amusement, that curious blend of Catholicism and anticlericalism found among many intellectuals and socialistically-inclined workers. I had not encountered this attitude in the Netherlands, where Catholics (at any rate in public) faithfully followed their clergy in matters of morals and politics. Catholic anticlericalism in Italy, on the other hand, dates back at least as far as the Risorgimento, when the Pope was an obstacle to reform and unity, but may in fact have had its roots in the Renaissance. Galileo is a good example. As my colleague Giorgio De Santillana once told me, Galileo's attitude can only be understood if one is aware of the phenomenon of anticlericalism among Italian Catholics. Giordano Bruno's statue on the Campo di Fiore in Rome is a typical example of this challenge to the papacy.

Rowe: *Where did your ventures take you after Italy?*

Struik: My fellowship from the Rockefeller Foundation was renewed for another year, but on the condition that we continue our studies in Göttingen. In and of itself this was fine: Göttingen was after all the mecca of mathematicians. But we had grown fond of Italy, its people (except for the blackshirts), its history, art, and science. And we had come to take its atmosphere of courtesy among mathematicians somewhat for granted.

Rowe: *What was the atmosphere in Göttingen like?*

Struik: Mathematically it was very stimulating, of course, but you had to have a thick skin to survive; the Göttingen mathematicians were known for their sarcastic humor. Emmy Noether, who was shy and rather clumsy, was often the butt of some joke, as was the good-natured Erich Bessel-Hagen. In von Kerekjarto's topology book there is a reference to Bessel-Hagen in the index, but when you turn the page cited there is no reference to him in the text, only a topological figure that looks like a funny face with two big ears. That was the way they could treat you at Göttingen, where ironical jokes about one's colleagues were always in vogue. It was a world apart from the courteous atmosphere in Italy.

Rowe: *Did you have any contact with the older generation of mathematicians in Göttingen?*

Struik: Yes, although I never met Felix Klein, who

J. A. Schouten to Felix Klein, 12 May 1924: "It is quite possible that Mr. Struik will be going to Göttingen, and this pleases me very much. I have already made various attempts to find him a place elsewhere, as I believe this is better for a young man than always working under the same direction. Should he work for you personally, you will have in him a devoted and conscientious assistant." (Klein Nachlass, Niedersächsische Staats- und Universitätsbibliothek, Göttingen)

died in the summer of 1925. Ruth and I attended his funeral, which was attended by most of the academic community in Göttingen. There were a few short speeches, one by Hilbert, and I joined the group of those who threw a spade of earth over the grave. I felt as though I had lost one of my teachers. Ehrenfest had always emphasized the importance of Klein's lectures to his students, and we read many of those that circulated in lithograph form. They are full of sweeping insights that reveal the interconnections between different mathematical fields: geometry, function theory, number theory, mechanics, and the internal dialectics of mathematics that manifest themselves through the concept of a group. During my stay in Göttingen, Courant invited me to help prepare Klein's lectures on the history of nineteenth and early twentieth century mathematics for publication, which I did. These first appeared in Springer's well-known "yellow series," and they remain, with all their personal recollections, the most vivid account of the mathematics of this period.

Rowe: *What about Hilbert?*

Struik: I saw a fair amount of him in those days, although he was quite old by then. His main interest was foundations questions, as he was still in the thick of his famous controversy with L. E. J. Brouwer. Hilbert was very good at reinforcing his own enormous power and authority by making use of clever assistants whose time and brains he ruthlessly exploited, but not withholding credit where credit was due. Emmy Noether had been his assistant during the war years when he worked on general relativity. Hilbert was an East Prussian, and there was a distinctly Prussian quality about him that was reflected in his relationships with his assistants. Ruth and I once asked Hilbert's assistant, Paul Bernays, to join us on a Sunday morning walk. Bernays was then in his mid-thirties and already a well-known mathematician, but he actually had to ask the *Herr Geheimrat* (which was the title one used in addressing Hilbert) whether he could spare him for a few hours.

I often attended Hilbert's seminar, which generally had anywhere from forty to seventy participants. Often the speaker was a visitor who had come to talk about his research. It was a daunting experience to speak before such a critical audience, and many who came were justifiably apprehensive. Afterward came the chairman's *judicium*, and his verdict, usually to the point, could help or harm a young mathematician's standing considerably, at least in the eyes of his colleagues. I once spoke about my work on irrotational waves and was happy that it received a friendly reception. Others were not so fortunate. Young Norbert Wiener, for example, was too insecure and nervous to do justice to his excellent research in harmonic analysis and Brownian motion.

Rowe: *Are there any particular Hilbert anecdotes that come to mind?*

Struik: Oh sure, but a good Hilbert anecdote has to be told with an East Prussian accent, which he never quite lost. Once a young chap, lecturing before Hilbert's seminar, made use of a theorem that drew Hilbert's attention. He sat up and interrupted the speaker to ask: "That is really a beautiful theorem, yes, a beautiful theorem, but who discovered it?—*wer hat das erdacht?*" The young man paused for a moment in astonishment and then replied: *"Aber, Herr Geheimrat, das haben Sie selbst erdacht!*—But, Lord Privy Councilor, you discovered that yourself!" That is a true story—I witnessed it myself. Another episode I remember took place in one of Hilbert's lectures on number theory, which I followed during my stay in Göttingen. The previous day he had written the prime numbers less than 100 on the blackboard, and now he came rushing into class to tell us: "Ach, I made a slip, a bad slip. I forgot the number 61. That should not have happened. These prime numbers are beautiful; they should be treated well—*man muss sie gut behandeln.*" On another occasion we were waiting for him in the seminar room. He finally came rushing in only to berate us: "Oh, you smug people, here you are sitting around talking about your petty problems. I have just come from the physics seminar where they are playing with ideas that will turn physics upside down!" That was Max Born's seminar, which week after week was attracting a hundred or more physicists, mostly younger men. Heisenberg and Pauli were then discussing the new matrix theory approach they were developing as an alternative to Schrödinger's wave theory.

Rowe: *Did you ever get invited to Hilbert's home?*

Struik: Yes, he and his wife occasionally invited us to an evening party at their home, usually to meet some visiting celebrity. I have a better recollection of the parties at the Landaus'. He was a stocky fellow and looked more like a butcher than a scientist. Having married the daughter of well-to-do Professor Paul Ehrlich, the famous chemist who found the first effective remedy against syphilis, Landau lived in upper middle-class comfort in a large and splendid home on the outskirts of town. After a sumptuous dinner our host led us to his study, a large room whose walls were covered with books, all of them mathematical. There were complete runs of important journals, collected works of famous figures, and nearly every imaginable work in number theory and analysis. No frivolous stuff here. There was nothing frivolous about his writing either. He presented his ideas as precisely as possible, in the unemotional style of Euclid: theorem, lemma, proof, corollary. He lectured the same way: precise, some of us thought pedantically precise. Occasionally he would present a well-known theorem in the usual way, and then while we sat there wondering what it was all about, he pontificated: "But it is false—*ist aber falsch*"—and, indeed, there would be some kind of flaw in the conventional formulation.

Once the guests were assembled with refreshments, Landau started organizing mathematical games. One of them I still like to play once in a while. Suppose you define "A meets B" to mean that at some time A shook hands with B, or at any rate A and B touched each other. Now construct the shortest line of mathematicians connecting say Euler with Hilbert. Can you shorten it by admitting non-mathematicians in your chain, like royalty or persons who circulated widely and reached old age, like Alexander von Humboldt? All kinds of variations are possible. Can you forge a link to Benjamin Franklin? To Eleanor of Aquitaine?

Rowe: *That sounds a little like the present pastime of constructing a mathematician's ancestral tree or determining one's "Erdös number."*

Struik: Yes, only the possibilities are much more open-ended. I can't resist telling one more Landau story that my former M.I.T. colleague Jesse Douglass liked to recall. One day at Göttingen Landau was speaking about the so-called Gibbs phenomenon in Fourier series, and remarked: *"Dieses Phänomen ist von dem englischen Mathematiker Gibbs (pronounced Jibbs) in Yale (pronounced Jail) entdeckt."* Only my respect for the great mathematician, said Jesse, withheld me from saying: "Herr Professor, what you say is absolutely correct. Only he was not English, but American, he was not a mathematician, but a physicist, he was not Jibbs, but Gibbs, he was not in jail, but at Yale, and finally, he was not the first to discover it."

Rowe: *Who else did you meet in Göttingen?*

Struik: There were many mathematicians from all over
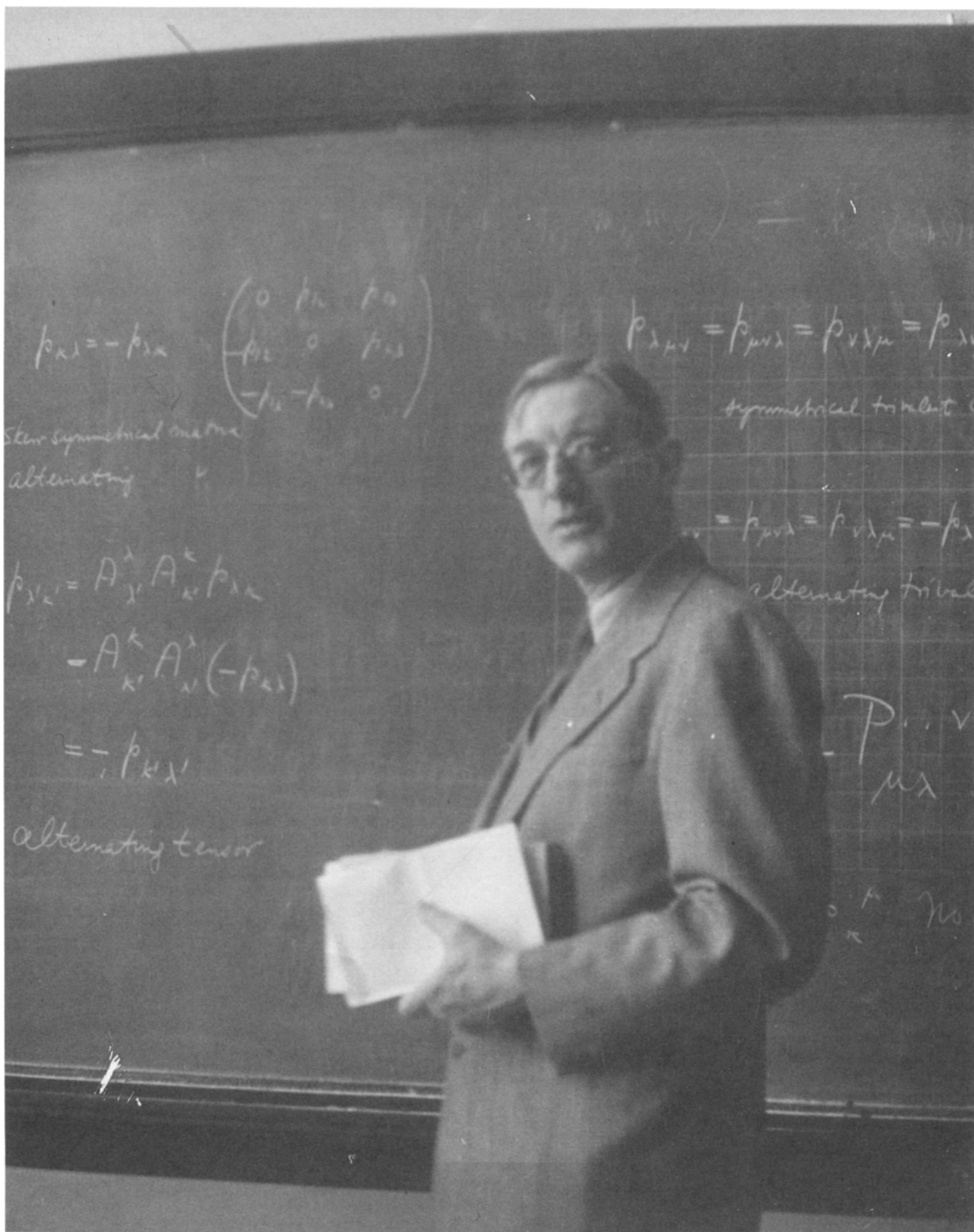
Dirk Struik lecturing on tensor calculus in 1948, the year he published his *Concise History of Mathematics* and *Yankee Science in the Making*.

the world: Harald Bohr, Leopold Fejér, Serge Bernstein, Norbert Wiener, Oystein Ore, and of course, B. L. van der Waerden. I had met him already at the Mathematical Society in Amsterdam. He and Heinrich Grell could often be seen strolling down the Weender Strasse on either side of Emmy Noether. They were sometimes called her *Unterdeterminanten* (minor determinants). I had some contact with Courant when I first arrived. I had met him in Delft a year earlier, and he and Levi-Civita had both supported my application for a Rockefeller fellowship. Courant was then working on existence questions connected with the Dirichlet problem as these bore on potential theory and solutions to partial differential equations. These ideas were at that time elaborated in the famous Courant-Hilbert text *Methoden der mathematischen Physik*. I was very interested in this field, and was already somewhat familiar with it through Ehrenfest's lectures at Leiden. Courant's assistant, Dr. Alvin Walther, took the time to introduce me to the latest developments, which was fortunate considering that Courant was burdened with his many academic obligations. Courant was of course a brilliant man, but to me he seemed then to lack Levi-Civita's talent for organizing his time.

Rowe: *When did you first begin to take a serious interest in the history of mathematics?*

Struik: It was on the historic soil of Italy that I met two historians of mathematics, Ettore Bortolotti from Bologna and Giovanni Vacca, and from this point on my interest in the field has grown steadily. I also met Gino Loria, who like Castelnuovo, Enriques, Bianchi, and Severi, was a geometer, though on a more modest scale. We talked about the desirability of having more ancient texts published with commentary. Vacca was a professor in Rome. I remember when we went to visit him and were looking for his apartment along the narrow street that he lived on. Some girls were playing outside, and we asked them where Professor Vacca lived. *"Mamma mia, siamo tutte vacche"* ("We are all cows"), they giggled. But we found the house, and talked among other things about ancient Chinese mathematics, a subject that was then hardly touched. "Learning enough Chinese characters for mathematical purposes is not difficult when you try," he said; but I never tried.

Later I met the director of the Dutch archeological institute in Rome, G. J. Hoogewerff, who was then working on Dutch Renaissance painters, the *Zwerfvogels* (wander-birds). When he heard of my interest in the history of mathematics, he suggested that I take a look at a Dutch Renaissance mathematician who had become an Italian bishop and advisor on calendar reform at the Fifth Lateran Council of 1512–1517. His name was Paul van Middelburg—Paolo di Middel-

borgo. It was more work than I anticipated, as it required reading Latin texts in incunabula and post-incunabula, but it was a nice occasional break from my work on hydrodynamics and function theory. For once I could profit from the Latin I learned in preparation for my entrance at the university. And so I persevered, my research leading me to a number of Rome's antiquarian libraries, like the Alessandrina and the Vatican. To get permission to enter the Vatican archives I had to go through the office of the Netherlands' ambassador, but at least it was no longer necessary to "prostrate oneself before the feet of his Holiness," which, as I was told, had been the case not long before. These libraries are only heated on cold days by a brazier with smoldering charcoal, so that you had to study with your coat on; luckily such ancient palaces had thick walls. In some of them you had to overcome the inertia of custodians who resented the intrusion of readers as an attack on their privacy.

I discovered some interesting things about the mathematical bishop who left his native Zeeland because, as he wrote, the people there considered intoxication the *summum* of virtue. An abstract of my findings was published in the *Atti* of the Accademia dei Lincei, and the full text appeared later in the *Bulletin of the Netherlands Historic Institute*. Only a few people have taken the time to glance at it, but let us say that the work was good for my soul.

Rowe: *When did you begin taking a wider view of the history of mathematics and science, taking into account the social context that shaped them?*

Struik: This question interested me from quite early on, and I followed the role played by science, and particularly mathematics, in the wake of the Russian Revolution. In fact, I saw this question of mathematics in society as a testing ground for my newly acquired Marxist views. Did "external" factors actually influence the "internal" structure of science, its growth or stagnation? Until fairly recently, it seems that everyone assumed this was not the case, that mathematics was a purely intellectual undertaking whose development is best understood by analyzing ideas and theories independent of the social system that produced them. But Marxist scholars had already shown that almost equally exalted fields like literature and biology could be successfully tackled using the tools of historical materialism. So what about mathematics? Around the turn of the century mathematics flourished in a state of blissful innocence. One could do geometry, algebra, analysis, and number theory in a delightful social vacuum, undisturbed by any extraneous pressure other than that exerted by one's immediate academic and social milieu. Even as late as 1940 G. H. Hardy could maintain that the "real" mathematics of the great mathematicians had, thank good-

ness, no useful applications. Yet fifty years earlier Steinmetz in the USA and Heaviside in England were already applying advanced mathematical concepts in electrical engineering, and probability and statistics were being utilized in biology, the social sciences, and industry. None of these developments, however, seemed to influence the mathematicians' purist outlook on the field.

When I assisted in editing Klein's lectures on nineteenth century mathematics during my stay in Göttingen, I learned how profoundly the French Revolution had influenced both the form and content of the exact sciences and engineering, as well as the way in which they were taught. This was especially due to the impact of the newly-founded Ecole Polytechnique in Paris, headed by Gaspard Monge. Quite clearly the educational reforms of this period were intended to benefit the middle classes and not the *sans culottes*. This realization gave me more confidence in the potential efficacy of historical materialism as an approach to the development of mathematics.

This confidence was strengthened a few years later when I read Boris Hessen's landmark paper on seventeenth–century English science. Hessen emphasized that even an Olympian figure like Newton was a man of his times who was inspired by problems that were central to the expanding British mercantile economy —problems posed by mining, hydrostatics, ballistics, and navigation. The British Social Relations in Science Movement, which included such figures as J. D. Bernal, J. B. S. Haldane, J. Needham, L. Hogben, and Hyman Levy, followed the trail blazed by Hessen, producing a number of germinal ideas for the history of science. These writers were a strong source of inspiration to me in thinking about the historical relationship between mathematics and society, and my views were strengthened by conversations with Levy and J. G. Crowther who were visiting the Boston area from England. Such an attitude also implies concern for the social responsibility of the scientist. In 1936 I helped to launch the quarterly *Science and Society*, which for fifty years now has been bringing this message of responsibility to the academic world. Some of my contributions to early issues of *S&S* deal with the sociology of mathematics.

Rowe: *I understand that it was through Norbert Wiener that you first came to the United States.*

Struik: Yes, Wiener was one of those Americans who had come to Göttingen in the mid-twenties, and he and I took to each other from the beginning. We talked a good deal of shop, as was wont in Göttingen and with Wiener. I became acquainted with his work in harmonic analysis and Brownian motion, which made it clear to me that I had met an exceptionally strong mathematician. But in matters of the world, such as politics, he was rather naive. He then seemed to think that the main problem in the world was overpopulation. But at the same time he was fiercely internationalist and detested the way many scientists from the allied countries still snubbed the Germans. Anyway, we drank beer together and took walks through the woods in the Hainberg overlooking the town. He asked me about my future plans and I admitted that they were rather vague and unpromising. I had spent seven years as an assistant in Delft, which was a very nice job but with no future prospects. Academic openings in those days were few and far between in the Netherlands. Wiener then suggested that I come to the United States. He told me about New England and M.I.T., where he was an assistant professor; they were looking for new blood and he thought I might fit in.

Rowe: *Were you attracted by the prospect of joining the M.I.T. faculty?*

Struik: Yes, I knew of M.I.T. through the *Journal of Mathematics and Physics* that it issued, where papers by C. L. E. Moore and H. B. Phillips on projective and differential geometry had appeared. So I knew there were congenial spirits in the mathematics department there. Wiener also made it all sound very attractive by describing the natural beauties of New England, his father's farm in the country, and the mountain climbing he and his sister Constance had been doing. Of course, I was footloose at the time and this would have been a step up the academic ladder, which was particularly important as it was then, as I said, quite difficult to land a promising job in mathematics. I told him that I might well take him up on this proposition if an offer came my way, and my wife Ruth also liked the idea.

Dirk and Ruth Struik, 1987.

Rowe: *So you were interested in coming to the United States, but perhaps open to other offers as well.*

Struik: Yes, and by the time I heard from M.I.T. I received another tempting offer from the Soviet Union where my brother Anton had been working as an engineer. Otto Schmidt, a mathematician and academician in Moscow, sent me an invitation to give lectures there. My work in differential geometry was not unknown in Russia, as I discovered in 1924 when I was invited to join the committee that was preparing the collected works of Lobatchevsky. Shortly before I left for the United States, Kazan University also bestowed on me its seventh Lobatchevsky prize. I sometimes wonder what might have happened had I accepted Otto Schmidt's offer and gone to work as one of his collaborators. Schmidt was not only a gifted scientist, he was also a first-rate organizer. Not long after I heard from him the conquest of the Arctic became an important part of the Socialist program, leading to the famous airplane expeditions of 1936–37 to the North Pole and the scientific expedition that spent 274 days on an ice floe. Schmidt was one of the leaders of these expeditions and the research that led to settlements in the huge wastelands of Northern Russia and Siberia. Under him I might have turned my attention to soil mechanics, for which my work on hydromechanics could have served as a preparation. Or perhaps I would have gone in for Polar exploration. . . . On the other hand, my natural Dutch obstinacy, also in politics, might have gotten in the way and brought me into conflict with the trend toward conformity typical of the later Stalin years. At any rate, I weighed this decision very carefully, including the factor of Ruth's health, which was not good at the time. We both agreed that life in the United States would be an easier adjustment, both in terms of the economic circumstances and the language and culture. And so I accepted the offer from M.I.T., with the idea that I might consider accepting the offer from the Soviet Union at a later date.

Rowe: *Was your choice by any chance influenced by an attraction to the culture of New England?*

Struik: Not at all. I really had no idea of New England and Yankees and the whole variety of American cultures at this point. As a matter of fact when I received the invitation from President Samuel Stratton of M.I.T. in September 1926, I had to take out my atlas to see where Massachusetts was located. I was surprised to learn that it was in the northeast and not on the Mississippi—perhaps I confused it with Missouri. Since that time I have always been very tolerant of those Americans who think that Hamburg is in Bavaria, or that Pisa and not Padua is near Venice.

**Rowe:** *You were a good friend of Norbert Wiener. What qualities did you admire most in him?*

Struik: I would say his courage and his sensitivity. He was a man of enormous scientific vitality which the years did not seen to diminish, but this was complemented by extreme sensitivity; he saw and felt things for which most of us were blind and unfeeling. I think this was partly due to the overly strict upbringing he had as a child prodigy. Wiener was a man of many moods, and these were reflected in his lectures, which ranged from among the worst to the very best I have ever heard. Sometimes he would lull his audience to sleep or get lost in his own computatious—his performance in Göttingen was notoriously bad. But on other occasions I have seen him hold a group of colleagues and executives at breathless attention while he set forth his ideas in truly brilliant fashion. Wiener was among those scientists who recognized the full implications of the scientist's unique role in modern society and his responsibilities to it in the age of electronic computers and nuclear weapons. I well remember how upset he was the day after Hiroshima was bombed. When I remarked that because of Hiroshima the war against Japan should now come to a speedy close without much further bloodshed—a common sentiment at the time and the official justification still heard today—he replied that the explosion signified the beginning of a new and terrifying period in human history, in which the great powers might prove bound to push nuclear research to a destructive potential never dreamed of before. He also recognized and detested the racism and arrogance displayed in using the bomb against Asians.

He just saw further than the rest of us. In Wiener's day robots were largely the stuff of fiction. His favorite parables concerned such robots or similar devices with the capability of turning against those who built them: Rabbi Loew's *Golem*, for example, or Goethe's *Sorcerer's Apprentice*, the Genie of the *Arabian Nights*, and W. W. Jacobs' *Monkey's Paw*. Today we all know that cybernetics, the science of self-controlling mechanisms, has an increasing impact on industry and employment, on warfare and the welfare of human beings.

Rowe: *You have continued to combine scholarship with political activism since you came to this country. Tell me something about your political activities.*

Struik: During the Second World War I stayed at M.I.T. and taught mathematics to the "boys in blue" sent to us by the navy. For some time I also spent weekends in Washington working at one of the Netherlands' desks in connection with the war effort, and I participated in the activities of the Queen Wilhelmina Fund, the Russian War Relief Fund, and the Massachusetts Council of American-Soviet Friendship. This
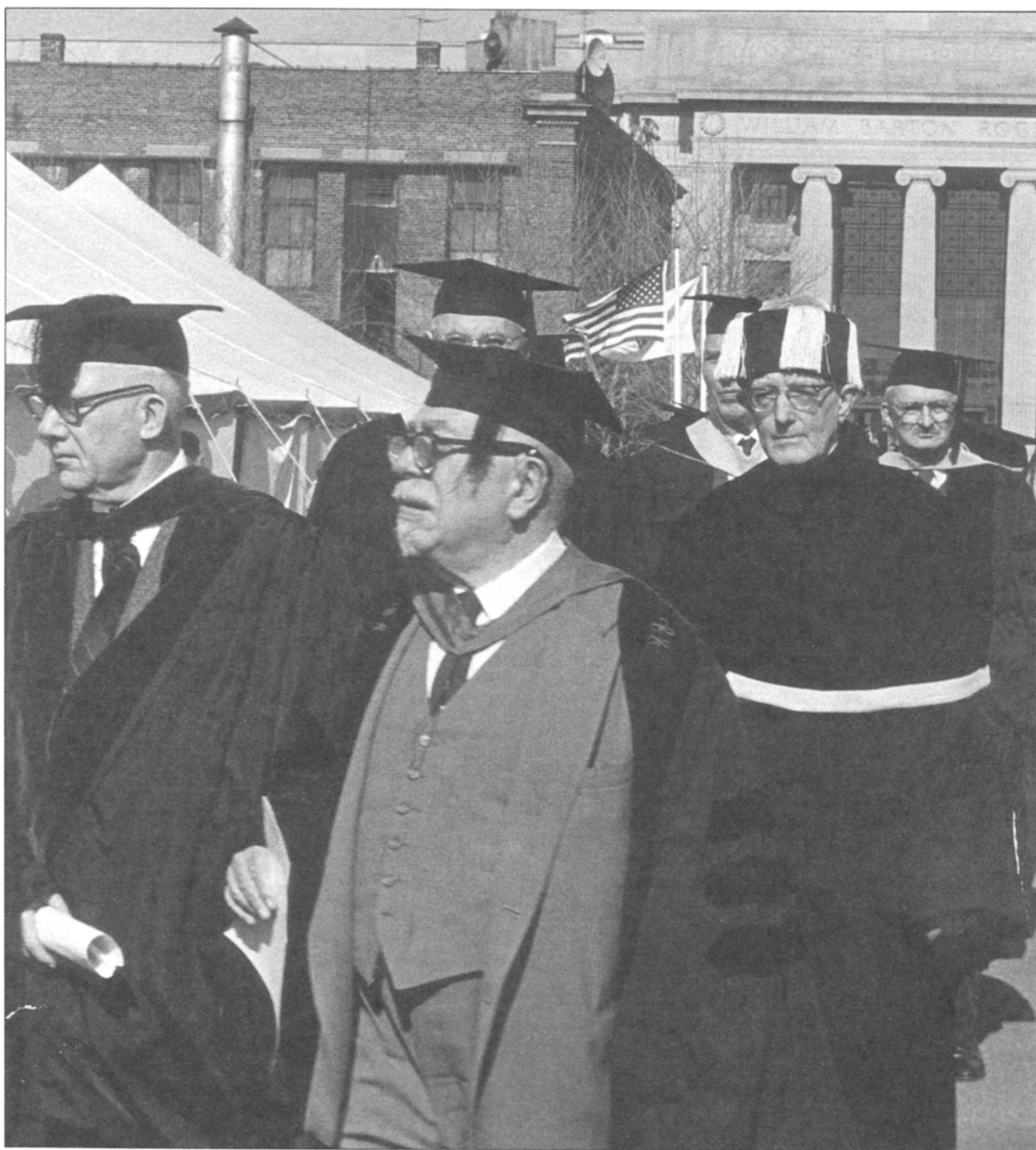
Norbert Wiener (center) and Dirk Struik (right) in the Centennial procession at M.I.T., April 1961.

latter work, which was a logical consequence of the anti-fascist campaign for collective security waged in the late thirties, together with my support for the Indonesians in their fight for independence and for the 1948 campaign of the Progressive Party, attracted the attention of sundry cold and hot warriors of the post-war period. I was called before the witch-hunting committees and an ambitious district attorney had me and my friend Harry Winner indicted on three counts of 'subversion.' That was in 1951, the beginning of the McCarthy era. There were wild newspaper headlines, M.I.T. suspended me (but luckily not my salary), and I was let out on heavy bail. Bertrand Russell was then lecturing at Harvard. When told that I was accused of attempting to overthrow the governments of Massachusetts and the United States, he murmured gravely,

Dirk Struik, 1952.

"Oh, what a powerful man he must be!"

Rowe: *What became of the charges against you?*

Struik: The case never came to trial, but it was not until 1955 that the indictment was finally quashed and I regained my position at M.I.T. It might have taken even longer if it had not been for the strong community support that Winner and I received, the dedication of our lawyers, and the Supreme Court ruling in the Pennsylvania case of Steve Nelson, which declared that subversion was a federal offense—Steve and I had been indicted under state law. During the five years of my suspension, I lectured all over the country on the right of free speech, and at home I worked on editing the mathematical works of Simon Stevin.

Rowe: *You continued to collaborate with Schouten throughout the 1930s. When did you give up doing differential geometry and concentrate on history?*

Struik: In the late thirties Schouten and I co-authored a two-volume work entitled *Einführung in die neueren Methoden der Differentialgeometrie*. This gave the first systematic introduction of the kernel-index method and incorporated a number of new techniques—exterior forms, Lie derivatives, etc.—that had since been developed. My last major mathematical publication was *Lectures on Classical Differential Geometry*, which appeared in 1950. After I became an emeritus in 1960 I gradually gave up following the course of new mathematical developments. I felt a little too old for that. My goal instead has been to learn as much as I can about mathematics up to about 1940. That's a big enough field for one human being, I think: the history of mathematics from the Stone Age to the outbreak of World War II!

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