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In Lieu of an Autobiography

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The author recalls some of the issues related to his professional work, first at the Politechnika in Warsaw and at the Polish University College in London and subsequently during his tenure as a professor of engineering at Brown University in Providence, Rhode Island.

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1. ORIGIN

When Jan Sengers and Bill Wakeham wrote to me in April 1992 and asked me to write an autobiography for a special issue of the *International Journal of Thermophysics* on the occasion of my eightieth birthday, I was very flattered, but my first reaction was to say, quite truthfully, I never celebrated a birthday. However, I realized that to say no would be ungrateful in the face of one more proof of genuine friendship. So, I said YES, thank you. I realized that to write an autobiography I would need a fair amount of not otherwise committed time to research for dates and other reminders. Circumstances beyond my control robbed me of most of the time that I thought I could devote to this task. I also remembered that the events of 1939–1945 destroyed all my earlier papers, and as a result, I lost the urge for collecting mementos and other archival materials which people normally consult when the time comes to yield to one's friends' request to write immodestly about oneself.

Consequently, I could not do more than go in my memory on a search of time lost and be satisfied with an unresearched collection of reminiscences.

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2. BEGINNING

My pedagogical and scientific activities turned around *thermodynamics* like a gyroscope on a pivot. I was introduced into the subject as a student of Mechanical Engineering at the Politechnika in Warsaw: first in my second year, rather cursorily in the course of general physics, and then in more depth in the third-year course of engineering thermodynamics. The former emphasized concepts and phenomena; the latter, under Professor Bohdan Stefanowski, put much weight on applications to heat engines and all the other machines which a mechanical-engineering designer was thought likely to encounter in his professional life. I, therefore, retained the conviction that thermodynamics is there to be applied but that applications must be based on a sound theoretical and conceptual foundation.

Every student of mechanical engineering at the Politechnika was obliged to present a major design or experimental project for his graduation. Professor Stefanowski agreed to supervise my efforts and suggested the measurement of the viscosity of steam. This introduced me to the field of measurement of thermophysical properties, which later was to claim much of my research time. Thus, from the very beginning, I associated thermodynamics with sound science, professionally important practical applications, and the creation of experimentally determined, reliable data on which to base effective designs.

My first measurement of viscosity followed the style of Professor Stefanowski's institute. The method chosen was to measure the pressure drop ΔP across a smooth pipe, machined like a barrel of a cannon, about $D = 100$ mm in diameter and 5 m long. In such an arrangement, the flow was turbulent and the flow rate approached 3 ton/h, which required a crew of three to operate one of the institute's large boilers. This scale of operations greatly enhanced my feeling of self-importance.

The viscosity was to be calculated by deriving the Reynolds number, $\mathrm{Re} = uD/\nu$, where u is the fluid velocity and ν the kinematic viscosity, from Blasius' empirical formula for the flow associated with the pressure drop ΔP. No paper on viscosity was ever published on this work, and the resulting thesis was entitled *On the High-Speed Flow of Steam*, because it soon became apparent that viscosity was the last thing that could be measured this way. The thesis disappeared during the war, but its lessons stayed with me. These were to the effect that an installation for the measurement of any physical quantity must be small and compact; that the working equation must have a sound physical and mathematical basis; that the flow regime, if employed, must be stable and reproducible; and, finally, that the calculated quantity, here ν, must be related to the measured quantity, here ΔP, by a sensitive function. (I first mentioned this early disaster when I was invited in 1979 to speak on the "Experimental Determination of Viscosity and Thermal Conductivity of Fluids" at a symposium to celebrate the 200th anniversary of the birth of Jöns Jacob Berzelius [1]. The lecture formed part of an international Nobel Symposium on "The Chemistry and Geochemistry of Solutions at High Temperatures and Pressures," organized by Frans E. Wickman, Secretary of the Royal Swedish Academy of Sciences, to whom I was recommended by Professor E. U. Franck of Karlsruhe University.)

In the autumn of 1939 and the spring of 1940, I found myself in the city of Lwow, then under the recent occupation by J. Stalin's Red Army. I was unemployed and unable to undertake any formal studies. However, through the courtesy of Professors Ocheduszko (later of Wroclaw) and Fuchs of the local Engineering University, I was given access to the libraries and seminars which enabled me to read and think. I then discovered from reading in *Physica* the merits of the Coulomb oscillating-disk pendulum as a viscometer which met all the preceding criteria. This reading period also made me into an implacable enemy of falling-ball viscometers.

3. LONDON

In the fall of 1943, I had the good fortune of being seconded by the Polish Army in Great Britain to the recently formed Polish College of Engineering in exile, later to become the Polish University College of the University of London. This institution enjoyed the hospitality of the City and Guilds College of Imperial College. I was introduced by A. K. Oppenheim, who quickly became a very close personal friend, to Dr. O. A. Saunders (now Sir Owen Saunders FRS), who enrolled me as his research student in thermodynamics. At the same time, Professor S. Ptuzański, Head of the Polish College, appointed me as a Senior Assistant and Lecturer in, what else!, *Thermodynamics*.

I combined my first steps as a pedagogue with work on a doctoral thesis which introduced me to the subject of *gas dynamics*, a fertile field for the application of classical thermodynamics. I reverted to this field of research several times later in my career. The thesis, *On the High-Speed Flow of Gases Through Channels*, was accepted by my supervisor, Dr. O. A. Saunders, and by Professor E. Giffen, representing the University of London. The thesis was written under a sky filled with flying bombs, another fertile application of the principles on which my thesis was based, soon to be followed by the arrival of V2 rockets. A paper written jointly with A. K. Oppenheim on this and related work [2] won the Water Arbitration Prize from the Institution of Mechanical Engineers of Great Britain.

In spite of all these "distractions," I was able to translate from the

German Ernst Schmidt's *Thermodynamics*. The translation, sponsored by Professor Sir Alfred Egerton FRS of Oxford University Press, did not appear until after the war, in the year 1949 [3]. It was republished as a paperback by Dover Publications in 1966. As soon as the war ended, Professor E. Schmidt was invited to spend a couple of years at the Royal Aircraft Establishment in Farnborough (first under strict security supervision) working on his proposal to cool gas-turbine blades by near-critical steam produced in the shaft of the rotor, hollowed for the purpose. I then made E. Schmidt's personal acquaintance. Slowly, as time passed, he was transformed from a supervised enemy into a cooperating scientist.

I worked on the translation when I taught the subject, because I thought that it was the best text for engineering students available at the time. During the Farnborough period, we utilized the close contact considerably to enlarge the book by adding to it chapters on gas turbines, gas dynamics, and rocket design. Ironically, Professor Schmidt had been the scientific director of the laboratory at Völkenrode, where the idea of the pulse-jet engine of the flying bomb was born as a result of a failed experiment on the propagation of shock waves through a gas in a long pipe. E. Schmidt was also well advanced in the theory of rocket design, and against my skeptical attitude, he inserted in the book the words,

> Calculations show that velocities for satelliting the earth and even circling the moon *can be reached*.... After escaping the gravitational field of the earth, it is not impossible to reach the other planets.

It is noteworthy that this opinion was written circa 1945, well before Alan Shepard went into space and Yuri Gagarin and John Glenn circled the Earth.

Ernst Schmidt was one of the originators of the prewar International Conferences on Steam Tables, later to become the raison d'être of the International Association for the Properties of Steam, which I was invited to join in 1954 (first as a member, later as head of the U.S. delegation, and ultimately as president of the Association) by Professors Joseph H. Keenan and F. G. Keyes of MIT. By then, in 1952, I had already taken up an appointment at Brown University.

4. POLISH UNIVERSITY COLLEGE OF THE UNIVERSITY OF LONDON

As a consequence of the Yalta and Potsdam agreements, the British Government withdrew its recognition from the Polish Government in exile. This undercut the basis on which the war-time school of engineering existed. Nevertheless, the British Government generously provided funds and a legal basis for the transformation of the school into the *Polish*

University College, which later became an *External College of the University of London.* The original purpose of training cadres for the return to Poland after the war lost its validity, because a majority of its students (recruited from among the demobilized soldiers of the Polish fighting units) and personnel decided against returning to the country, which had become a Soviet satellite.

I was appointed Head of the Department of Mechanical Engineering and given a budget as well as a group of enthusiastic colleagues to design and build a teaching laboratory (which ultimately comprised seven units) in a disused swimming pool in the suburb of Putney. I later never commanded such generous means and such devoted collaborators to build and to operate a serious engineering facility. The laboratory was also equipped to conduct research which I continued in the field of gas dynamics.

Following prewar practice, our students were required to present a design study or an experimental thesis for graduation. I launched several such studies aiming at designing an oscillating-disk viscometer to measure that property in gases and steam at high pressure and temperature. One such design, by Karol Pilarczyk, showed enough promise to induce me to sponsor its author as a Ph.D. student. Since the Polish College was not authorized to conduct postgraduate studies, I recommended the candidate and the design to Dr. O. A. Saunders, who in the meantime became the Head of the Department of Mechanical Engineering at the City and Guilds College.

Mr., now Dr., Pilarczyk performed very accurate measurements on a number of gases and achieved a reproducibility, that is a precision, of one part in a thousand [4]. The results were evaluated with the aid of a nineteenth-century theory which I had unearthed in my Lwow days. Frankly, I did not understand the mathematics employed in the theory but used its result because I had great faith in articles published in *Physica.* It soon became apparent that the so-calculated results, especially those for argon, for which good comparative data existed in the literature, were inaccurate and too large by about 3%. I was able to resolve this conundrum a couple of years later [5, 6], when I had already accepted (in 1952) an invitation by John Marchant, Chairman of the Division of Engineering at Brown University in Providence, in the state of Rhode Island, to join his department.

The laboratories at Putney survived for about a decade, after they were taken over and operated by Battersea Polytechnic, later to become the University of Surrey.

5. BROWN UNIVERSITY

Although initially (in 1952) invited for 1 year, I still am (in 1993) affiliated with Brown University. I was again given reasonable funds to design and build a laboratory, this time limited to thermodynamics and heat engines. This laboratory did not exist beyond the year 1958, when I went on my first sabbatical leave back to Imperial College in London. In general, over the years, the amount of student time devoted first to experimental work and later to thermodynamics itself was considerably reduced.

I was soon able to secure funds from various Government agencies to start a modest effort to measure the viscosity of gases and, later, of steam. In this, I was in great measure assisted by Dr. Wolfgang Leidenfrost (a descendant of the "dancing" steam bubble), my first Post-Doctoral Fellow, who had recently obtained his doctorate under Ernst Schmidt. The first instrument designed and operated in cooperation with Dr. Leidenfrost proved to be capable of a precision of about 1 part in 10,000 (i.e., 10 times better than in London) under easy operating conditions. But the results continued to be off in the same way as in London.

It became clear to me that the theory of the instrument we used was at fault and that it underestimated the drag on the disk which was compensated by an apparent increase in viscosity. In other words, my lack of understanding of the mathematics of the paper in *Physica* turned out to be the paper's shortcoming. Insufficiently trained in mathematics, I was able to enlist the help of Brown University's Division of Applied Mathematics in the person of Gordon F. Newell, now Emeritus at the University of California at Berkeley. He was able to derive a set of very accurate formulae for various oscillating bodies [7–10], following an earlier development in cooperation with Leif N. Persen [11], now Emeritus at the Tekniske Høgskole in Trondheim in Norway. The progress was due to Newell's successfully rigorous treatment of the increase in drag produced by the sharp edges of a disk. Newell's theory survives to this day and is being successfully used as a basis of several types of oscillating viscometers. Much later, J. C. Nieuwoudt and Jan V. Sengers of the University of Maryland extended Newell's theory [12] and made it possible to employ the oscillating system to measure simultaneously the viscosity and the density of the fluid in the container [13].

These progressive improvements enabled me and my later "Postdocs" to put into the open literature a large body of data on viscosity, to be supplemented by similarly accurate data on thermal conductivity. As far as the latter property is concerned, we developed the transient hot-wire method [14, 15] and did not achieve satisfactory accuracy until we completely reworked the mathematical theory of the instrument. In this endeavor, I was greatly assisted by Dr. W. A. Wakeham, now Head of the Department of Chemical Engineering and Chemical Technology of Imperial College in London. Professor Wakeham continued this work on a much enlarged scale in his London laboratory, with satellites at East Kilbride, Lisbon, and Salonika. He also succeeded me as Chairman of the Subcommittee on Transport Properties of the International Union of Pure and Applied Chemistry, which I headed from 1981 until 1991.

My ability to produce a large body of accurate and reliable data was due in no small measure to 12 gifted Japanese students who were sent to work with me over a period of 20 years by Ishimatsu Tanishita, then Head of Mechanical Engineering at Keio University. Among them Akira Nagashima, now a professor at Keio in his own right, taught me much about Japanese culture, because he had a sensitive feel for both. In 1986 he arranged for me and my wife a 20-day tour of Japan by invitation from the Japan Society for the Promotion of Science.

6. MY CREDO

My interest in the measurement and correlation of thermophysical properties was always strongly motivated by the needs of industry and machine design. In 1981 I was invited to deliver a keynote lecture entitled "Thermophysical Properties in Science and Industry" at the 8th Symposium on Thermophysical Properties held in Gaithersburg, Maryland [16]. I then said,

> Without quantitive data, science, meaning physics and chemistry in this context, would soon become an empty shell of unverified speculation. Progress would be severely impeded and would, eventually, stop. Without numerical data describing the properties of working fluids and structural materials, industry, meaning machine and process design in this context, would be condemned to a laborious, empirical, step-by-step construction of nearly full-scale equipment. This would be prohibitively costly and would bring economic progress to a standstill. These are truths which... I hold to be self-evident. Are they also self-evident to the external world, to the scientific establishment, to the industrial decision makers, to national policy setters? Even though the answer is not a desperate NO, it is also not a triumphant YES. ... The task is not one for a lonely philosopher who creates in isolation, but is one that requires cooperation between the academic research worker, the industrial practitioner, the scientific worker employed by the Government and the administrative decision maker in the public as well as in the private sector. ... [As time progressed, I realized that the] number of pure substances and mixtures, the number and complexity of the properties needed over staggering ranges of pressure and temperature are... vast, [and the effort is neverending].
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> All of us who find work on thermophysical properties challenging and interesting must patiently explain to the providers of funds why this work is

necessary. We shall never achieve an *ultimate* explanation because the most important aspect of our work is that its *absence* would be disastrous. Secondly, the superficially rational demand for one-hundred-per-cent quantification, for an account in dollars and cents, or any other currency for that matter, is not possible. I trust that our experiences with the "whiz-kids" of the Vietnam war has taught us to mistrust excessive quantification and to make some room for good old common sense. The latter is particularly rewarding when we come to discuss accuracy.

I always found aesthetic satisfaction in achieving high accuracy, but also believed that accuracy, contemptously referred to as *gold-plated* numbers in some circles, combined with *reliability*, is essential for the building of data bases which are now so effectively managed by computer codes. I do not advocate, or think it possible, that we should make every measurement at the limit of the attainable. There must be a measure in things, but I deplore the attitude which is hostile to anybody who cultivates accuracy. Moreover, some misguided cost-accountants do not realize that skillful experimenters produce good accuracy at less cost than others who settle for "engineering accuracy."

The belief in the need for a cooperative effort induced me to take part in the work of the International Association for the Properties of Steam and the International Union of Pure and Applied Chemistry, as already mentioned. I participated in many conferences and symposia on these topics and became acquainted with the leading contributors, both in this country and abroad.

7. IAPS–IAPWS

I derived particular satisfaction from my association with the International Association for the Properties of Steam (IAPS). This was (and, as the International Association for the Properties of Water and Steam, IAPWS, still is) broadly based internationally, with Soviet scientists having played an important role, in spite of the cold war. As far as I am concerned, the culmination came during the Tenth International Conference on the Properties of Steam, held in 1984 in Moscow. The important action undertaken by the General Assembly was an international agreement to endorse two comprehensive, computer-programmable codes for the thermodynamic properties of H_2O and D_2O [17]. These formulations are in the form of fundamental equations from which all properties can be deduced by the use of classical relations. They have been designated IAPS Formulation 1984 for the Thermodynamic Properties of Ordinary Water Substance for Scientific and General Use and a similar document was promulgated for heavy water. These formulations are now used in science and industry throughout the world as recognized standards. As head of the U.S. delegation, I was invited to make a statement on this achievement on Moscow television on September 8, 1984.

My colleagues generously elected me an Honorary Fellow of IAPS and added a citation in 1992. The U.S. delegation to IAPWS is now in the very capable hands of Dr. J. M. H. Levelt Sengers ("Anneke") of the National Institute for Standards and Technology, formerly the National Bureau of Standards.

8. EXTENDED LAW OF CORRESPONDING STATES

Being in possession of accurate data on the viscosity and thermal conductivity of gases, we were able to formulate an *extended law of corresponding states* for their equilibrium and transport properties [18]. This effort was undertaken with my postdoctoral fellows, Sung-Tak Ro and William A. Wakeham. Our effort was firmly based on statistical mechanics which we cultivated by personal contact, first with John Ross, now chairman of the Department of Chemistry at Stanford University, and then with E. A. Mason [19], who joined the faculty at Brown University with appointments in Engineering and Chemistry.

The extended law of corresponding states rests on the hypothesis that all intermolecular force potentials of binary interactions in the noble gases can be made congruent if properly scaled. By means of this law it became possible to calculate the equilibrium as well as the transport properties at low density of the five noble gases and of their mixtures (31 systems in all) in a consistent way [20]. To this end it was enough to use the Chapman–Enskog solution of the Boltzmann equation together with empirically established correlation equations for the collision integrals. Each binary interaction is characterized by a pair of molecular constants ("scaling factors") σ_{ij} and ε_{ij}, which together form a kind of periodic table.

The initial result proved so encouraging, and so credible thanks to the accuracy of our data, that we looked for and found ways to extend the law to more complex molecules. The now famous Mason–Monchick solution of the Wang–Chang–Uhlenbeck hierarchy of Boltzmann-like equations for polyatomic gases made it possible to include the viscosity and diffusion coefficient of polyatomic gases. Further extensions of this law, in modified form, enabled E. A. Mason to extend the initially covered temperature range and to include thermal conductivity as well [21, 22]. Our latest "periodic table" makes it possible for a computer to calculate the viscosity of several hundred different systems over a very wide temperature range [23]. Two colleagues, R. DiPippo and E. Khalifa, contributed substantially to the success of these extensions.

9. ODD RESULTS

The very precise data on viscosity revealed certain features which, though not obviously important, appeared somewhat puzzling at the time. First, we found a range in which the viscosity of helium decreased with increasing density [24]. Later, Dr. P. D. Richardson (a pupil of Sir Owen and now a Professor at Brown University and Fellow of the Royal Society of London) found a fairly large range of pressures and temperatures in which the viscosity of superheated steam also decreases with increasing density [25]. In another context, J. H. Whitelaw (now a Professor in the Department of Mechanical Engineering at Imperial College) brought into evidence a rather small enhancement in the viscosity of carbon dioxide in the critical region [26], which stood in great contrast to the very large enhancement in the thermal conductivity of this substance in the same region of states as measured by J. V. Sengers in the Van der Waals Laboratory in Amsterdam, headed by A. Michels at the time [27].

All these details appeared to be counterintuitive when first obtained. Later, some of them were used as test material for the formulation of statistical mechanical theories of transport properties in dense gases. One such test proved negative. We were able to make very precise and very closely spaced measurements of the viscosity of a couple of gases over an extended density range in the search of the logarithmic term in the density expansion of viscosity discovered by E. G. D. Cohen and J. R. Dorfman [28]. Jan Sengers formulated an algorithm with the aid of which he could systematically scan the viscosity against density and so determine each consecutive term in a series together with its uncertainty. Our data seemed to demonstrate either that the logarithmic term did not exist or that it was multiplied by a very small factor [29, 30].

10. TWO BOOKS

Parallel with my efforts in the field of transport properties, I continued to work on several other projects, all under the general heading of thermodynamics.

As a result of my lectures to undergraduates and graduate students, I published a two-volume textbook entitled *A Course in Thermodynamics* [31, 32]. Volume I was published in 1966 and Volume II was published in 1968. The book was contracted by Ginn and Company, which sold its College Division to Blaisdell Publishing Company, which produced two handsome books. Almost simultaneously, Blaisdell was taken over by the Xerox Corporation, which unfortunately, due to a computer error, put out the message "out of print" for 4 years running, without my knowledge, when 4000 unsold copies existed.

The book was reprinted in hard covers by my friend, W. Begell, then President of the Hemisphere Publishing Corporation, in cooperation with McGraw–Hill, and later on his own as a paperback. Hemisphere was able to sell a reasonable number of copies, and some paperbacks are still available through Taylor and Francis, which acquired Hemisphere in the meantime.

As I think back to those days, it occurs to me that I then believed that thermodynamics as a science had reached completion. If asked, I would probably have said that competence in thermodynamics was a matter of mastering a closed chapter. I do not think so now. Nevertheless, the American Society of Mechanical Engineers saw fit to confer on me in 1981 the James Harry Polter Gold Medal for Thermodynamics.

On Jan Sengers' suggestion, J. R. Dorfman cooperated with me in the production of our *A Course in Statistical Thermodynamics*, which was published by Academic Press [33].

11. TRANSLATIONS

Soon after my arrival in Providence, I became interested in the exploration of the effect of free-stream turbulence on heat-transfer rates. The science of heat transfer was undergoing a vigorous development, and in order to participate in this movement, I thought that I ought to learn something about the mechanics of boundary layers. E. Schmidt introduced me to H. Schlichting, who produced a remarkable monograph on this subject in 1952. I was very much impressed by it and agreed to translate it into English. The translation appeared in 1954 as *Boundary-Layer Theory*. This book also had a bizarre history.

The first English edition was published by Pergamon Press, whose owner was the now notorious Robert Maxwell. At a certain point he tried to persuade the author and the translator to forego royalties because "he was making both of us famous." Since the copyright holder, G. Braun-Verlag of Karlsruhe in Germany, had contracted with Pergamon for one printing only, we looked for an American publisher for the second printing. The first company we approached was not interested "because there were no worked examples and no problems" in the treatise. McGraw–Hill, on the advice of R. Folsom, published a second printing of the first English edition [34]. At the same time, Pergamon Press also published a second illegal printing. As a result of court action, Pergamon Press settled and paid us modest damages. Between 1952 and 1980, *Boundary-Layer Theory* enjoyed eight editions, four in German and four in English. I profited substantially from my collaboration with H. Schlichting and was able to keep up with the subject through the periodic revisions of this, now classic treatise.

Upon a challenge by K. Jacoby of Academic Press, I agreed to translate A. Sommerfeld's *Thermodynamics and Statistical Mechanics*, which constitutes Volume V of his monumental Lectures on Theoretical Physics [35]. I found much satisfaction in this work, not the least because it was truly challenging. Sommerfeld commands a terse, but clear style of writing in which every word in a sentence counts and must be sensitively placed. I would describe Sommerfeld as the Ernest Hemingway of the German scientific literature. In the course of this work, I discovered a couple of minor errors which convinced me, if convincing were needed, that *errare humanum est*, without in the least diminishing my highest regard for Sommerfeld's talent.

My work on this translation had a very pleasant "fallout." because on this occasion I became personally acquainted with Professor Josef Meixner, the editor of Volume V, which Sommerfeld's tragic death left incomplete. J. Meixner became a personal friend and mentor who was always ready to answer questions and to dispel doubts. He taught me a lot of classical and statistical thermodynamics, and in Chapter 21 (which he contributed), as well as in his article written with H. G. Reik for the *Handbuch der Physik*, he introduced me to the systematic study of irreversible processes in near-equilibrium states and in the spirit of the *local-state approximation*. Much later I personally became involved in research in this field [36, 37]. This study revealed to me that the theory of boundary layers in Schlichting's book constitutes an experimentally verified, predictive mathematical description of an irreversible process, even though the Second Law was hardly mentioned in it. All these insights left me with the puzzle as to how to fit turbulent flow into a credible thermodynamic formalism.

My hobby as a translator continued with a work on thermodynamic derivatives for steam (by S. S. Rivkin, A. A. Aleksandrov, and E. A. Kremenevskaya) from the Russian [38] and a short booklet on heat conduction (by U. Grigull and H. Sandner) from the German [39].

12. MECHANICS OF SOLIDS

When I took up my appointment there, Brown University was the world-famous center of excellence of study of a vast spectrum of subjects in the field of solid mechanics. The *spiritus movens* was William Prager, formerly of Göttingen. He was invited to Providence from his exile in Istanbul, just before the United States entered the Second War, by President Henry M. Wriston, with a special dispensation granted by Edward R. Stettinius, President Roosevelt's Secretary of State. W. Prager organized a vigorous Division of Applied Mathematics and was able to attract to it a veritable galaxy of talented people. Among them, I still count D. C. Drucker and R. S. Rivlin as my closest friends, as well as the late Hans Ziegler. Over the years, I became friendly with P. Germain, the "perpetual" Secretary of the French Academy of Sciences, with A. E. Green FRS, Turan Onat of Yale, E. H. Lee of Berkeley, and many eminent visitors, such as L. I. Sedov of the USSR Academy of Sciences, too numerous to list here.

In those days, the Divisions of Engineering and Applied Mathematics cooperated very closely. The many joint colloquia and seminars revealed to me the importance and aesthetic satisfaction of the study of plasticity and inelastic behavior of metals and other structural materials, all examples of *irreversible processes.*

Slowly, it struck me that, in spite of the relevance of thermodynamics, the study of these irreversible processes took an independent line and developed without it. As a result, the discipline created its own vocabulary and concepts, which made it difficult to establish a connection between it and classical thermodynamics. I regret it that I never had the time and concentration to make a systematic study of solid mechanics and its associated mathematics. Nevertheless, I made several attempts to describe elastic and inelastic processes on the background of thermodynamics. Several, in retrospect rather naive, attempts are discussed in my book [31, 32]. The first paper on the subject was presented at the IUTAM Symposium held in Vienna in 1966 and entitled "Irreversible Aspects of Continuum Mechanics" [40]. This symposium was the scene of a historic clash between P. Mazur, a representative of classical methods, and C. Truesdell, who vigorously subscribed to Rational Thermodynamics in his paper "Thermodynamics for Beginners," a new and radical formalism proposed by B. D. Coleman and W. Noll.

At first I thought that the application of thermodynamics to solid mechanics was a routine matter. I do not think so anymore.

I continued to make slow progress in this task in cooperation with several colleagues. Among them, I wish to mention Professor Jean Bataille [41, 42], whom I first met as a student, when in 1966 he audited my graduate course given at the Sorbonne. I was then on sabbatical leave in Paris. I continued my contact with him on my 1974 sabbatical leave at the École Centrale in Lyon; he was already a faculty member there. I was able to invite him to Brown University on several later occasions. In turn, he was instrumental in my receiving an honorary doctorate from his university (Claude Bernard in Lyon).

My interest in solid mechanics continued and led me to examine more deeply the foundations of the subject. This I was able to do with a minimum of distraction during several extended visits to Germany. In 1985, I was invited to become a Fellow of the Institute of Advanced Study (Wissenschaftskolleg) in Berlin, where I entered into a vigorous exchange of ideas with R. S. Rivlin, now of Lehigh University, and our host, Professor W. Muschik of the Technische Universität, the famous prewar Technische Hochschule of Charlottenburg in Berlin. In 1987, I was honored by a Humboldt Prize of the German Government, which I consumed in two parts. In 1987, I resided at the Technical University of Stuttgart, where my host was Professor K. Stephan and where I maintained contact with E. Kröner, the wellknown creator of the mathematical theory of dislocations. A year later, as a guest of Professor K. Gersten, I spent a semester at the Technical Universität of Bochum. Last, but not least, in 1990 I was invited by Professor D. Straub for a semester at the Universität der Bundeswehr in a suburb of Munich. I formed a very close professional and personal relationship with Professor Straub which lasts to this day.

During all those visiting assignments, I concentrated attention on the foundations of thermodynamics, with emphasis on its applications to problems of stress and strain in solid materials. The most basic difficulty can be traced to the absence of a generally accepted formalism for the description of irreversible processes. These must be conceived as sequences of nonequilibrium states distributed in space and varying with time, thus calling for the creation of a theory of *continuum thermodynamics* rather like that of *continuum mechanics.* In a nutshell, the problem is to formulate a credible version of the Second Law and of a physically acceptable concept of the *entropy of a nonequilibrium state.*

Personally, I concentrated on the theory of internal variables, in the spirit of P. W. Bridgman, grafted onto what many workers call the theory of local equilibrium but which I prefer to describe as the hypothesis of *local state* [43]. As time passed, I recognized that this version must be considered an approximation to an as yet undiscovered, more general theory. I still believe that this is an adequate approximation for the solution of many practical problems as witnessed by its indisputable success in fluid mechanics. I subscribe to the opinion that its adaptation to the study of processes in solid materials is an effort well worth undertaking.

Other workers have come to the same conclusion, namely, that a convincing formalism for the systematic analysis of nonequilibrium and irreversibility does not yet exist. Many have attempted to find an answer. However, the proposals advanced so far are mutually contradictory and none has recruited me as an acolyte. Contemporary thermodynamicists have formed themselves into warring camps which often engage in acrimonious disputes, rather like those between incompatible (but close) religious denominations. One such clash occurred in 1966 in Vienna, as I already mentioned.

13. ENERGY

The traumatic oil embargo of 1973 turned the country's concentrated attention on problems of energy. This development brought me back to my studies of applied thermodynamics in prewar Warsaw. An adequate, continuous and economical supply of "energy," that is, of electricity and fuel for heating and transport of all forms, is a *sine qua non* of our well-being, our civilization. Every aspect of this enormous complex of problems involves thermodynamics in its many-faceted applications.

I soon became interested in the practical applications of geothermal energy in close collaboration with R. DiDippo and H. E. Khalifa [44–48] and, from it, in problems of multiphase flows in close collaboration with Z. Bilicki [49–52]. First, the Energy Research and Development Administration (ERDA) and, later, its successor, the Department of Energy, provided ample financial support for a modest group of students and colleagues who constituted the Brown University Center for Energy Studies. The Center existed from 1976 until 1985. Apart from research on geothermal energy, the Center organized a series of monthly university lectures to which I was able to attract, as speakers, many of the most prominent specialists in all aspects of this vital problem. The geothermal work resulted in the publication by the Department of Energy of the *Sourcebook on the Production of Electricity from Geothermal Energy* [53], edited by myself with assistance from R. DiPippo, H. E. Khalifa, and D. J. Ryley. We also produced the book *Geothermal Energy as a Source of Electricity* (1980), with Professor R. DiPippo as its author [54].

Our efforts were severely curtailed and then stopped when the Federal Government began to subscribe to the view that "the energy problem will be solved by the free play of market forces." I think back to this period with a mixture of sadness and astonishment. On the basis of what I take as my understanding of the technical aspects of "energy" and my assessment of what the inherent characteristics make promising and what is a hopeless chasing of mirages, I believe that the country made many questionable choices. In the early days ERDA and DOE poured vast sums of money into such ventures as the design of solar-thermal, large-scale electric generators, installations to utilize the wave motion of the oceans, the photoelectric satellite, and many others whose failure could have been, and was, confidently predicted. Very often, wishful thinking is taken for fact by politicians. And yet, I sympathize with their plight: How can they tell whom to believe in a field full of contradictory "experts"?

This is not the place to outline what I might consider a rational energy policy. But, I cannot help but record that contemporary decisions all too often are based on emotional, irrational reactions. For example, how can one justify simultaneously curtailing research on nuclear reactors (which exist, but need to be improved) and supporting research on energy from fusion (which is still a dream)? Lest I be misunderstood, I think that research on both is needed.

To end this section on a more positive note, I recall that the State of Rhode Island appreciated my lecture series. On April 14, 1983, the Rhode Island Energy Coordinating Council unanimously resolved to present a citation to me. This citation was read by Governor J. J. Garrahy at an energy seminar at Brown University on the twenty-first of that month.

14. CLOSURE

In closing, I revert to the opening. I remind myself—and my well-wishers—that this is an unreviewed and unresearched stream of reminiscences. There are people who played a part in my life and career whose names I left out. I apologize that I have not mentioned them.

I end by confessing, unashamedly, that I felt much satisfaction when I was elected to the National Academy of Engineering, as a Foreign Member of Polish Academy of Sciences and as a Fellow of Imperial College.

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