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Colloquia: ICTT2009

Introduction to the Proceedings of the 21st International Conference on Transport Theory

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For unknown reasons, I was invited to the 21st International Conference on Transport Theory (ICTT-21) held in Torino, Italy, July 12-17, 2009, at the Politecnico di Torino, one of Italy's premier technological universities. Technical Universities have figured prominently in the history of ICTT. The first ever conference was held in 1969 on the campus of Virginia Tech as were six of the subsequent meetings. (At one time, on fact, before the term "ICTT" had been coined, these gatherings were referred to as the "Blacksburg meetings," Blacksburg being the town in Virginia where VA Tech is located.) Subsequent meetings were held on the campuses of technical universities in Lubbock, Texas; Atlanta, Georgia; Goteborg, Sweden and London, England.

The meeting was dedicated to Prof. Carlo Cercignani, Politecnico di Milano, in the occasion of his seventieth birthday. Carlo gave a well-appreciated and stimulating speech (Transport theory for MEMS and NEMS) at the opening session on Monday. Unfortunately, we have recently learned that Carlo has passed away. It is really a great loss for our community, where he has been active for a long time and has given so many significant contributions. We will all miss him.

The ninth meeting in the series, organized by Vinicio Boffi in 1985, was held in Montecatini Terme, Italy, a lovely spa town halfway between Firenze and Pisa. This was the first meeting held outside the US, and allowed the letter "I" to be added to the name of the conference. Since then there have been 12 ICTT's, four in the US (La Jolla; Blacksburg, again—a celebration of the 65th birthday of the late Kenneth Case and my own 60th; Albuquerque; and Atlanta); and, besides the meeting last year in Torino, another in Italy (Riccione) as well as meetings in Beijing; Göteborg; London; Rio de Janeiro; Budapest; and Obninsk. The next meeting, by the way, will move back to the US, to Oregon, as decided at a rump session in Torino.

The idea for the first meeting in the ICTT sequence originated at the Raleigh, NC airport where Chuck Siewert, Bob Erdman and I were enjoying a cup of coffee while waiting for Erdman's plane to leave. After deciding to hold a meeting, and to hold it at VA Tech to which I had recently emigrated after 10 years at the University of Michigan, I was relegated to the task of fund-raising. I managed to talk the Atomic Energy Commission (now a branch of the US Dept. of Energy) into providing enough funds for about 25 participants to gather for a week in Blacksburg. Each participant was granted a full hour, including discussion, and despite the fact that I chaired every session the meeting was such a resounding success that we decided to hold another, in two years.

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This meeting was at Los Alamos, and the topics, chosen by the local organizers (mostly the late Bill Hendry) were not strictly neutron transport theory, which had been the focus of the first conference, but broadened significantly into quasi-related areas such as kinetic theory, radiative transfer and the like. This format evolved into the conceptual framework of the meetings: scientists, mathematicians and engineers who used similar mathematical tools to solve disparate physical problems should profit by exposure to each others' work. (At about this time, I established the journal Transport Theory and Statistical Physics, or TTSP as it came to be known, with a similar mandate.) The subsequent history of the meetings is found above, in abbreviated form. (I was coerced into writing this report while enjoying my annual visit to Firenze, so I do not have any documents available, and am relying primarily on my memory. I do wish to thank Gianni Frosali, however, who made his office at the Università di Firenze as well as his computer, his documents and his intellect available for my use. Without his gracious assistance, this report would never have been written.)

Now let me say a few words about ICTT-21. I have not been active in transport for the past several years, which is why I used the term "unknown reasons" in the first line of this essay. In addition to attending the conference, I was asked to give the final talk, which is usually a summary of the most important papers there presented and a rehash of some of the most significant discussion. But such a task was entirely beyond my area of expertise, so I compromised with a few general remarks, and spent the remainder of my allotted time reprising my own career in transport theory which I will reproduce in part below. Since I am among the world's oldest living transport theorist—I turned 80 a few days before the ICTT and my career in transport theory spans some 60 years—I felt that this might give an historical perspective to the audience that they could perhaps attain in no other fashion.

I did attend most of the sessions in Torino, listened and did try to understand the talks as well as I could, and I came away with a few impressions. First, there were 92 participants listed in the program, from 17 countries compared to some 25 from three countries (US, Yugoslavia ad Turkey) for the first. So the conference has grown, prospered and become truly international. Second, I was surprised to see what a comeback neutron transport had made. In the period, say ten to fifteen years in the past, mathematicians had to a large extent dominated the meetings, with particular interest in existence theory for various kinetic equations, especially the nonlinear Boltzmann equation, but other types of kinetic equations as well. These topics seem to have largely been relegated to the background now, and there is much more interest in numerical studies of reactors and similar objects (*e.g.*, accelerator-driven fission systems) with a lot of emphasis on Monte Carlo calculations. At the 1969 meeting Monte Carlo was in its infancy, was mostly classified and certainly was not discussed at the meeting. Today, with the advances in computing speed, it has become a routine calculational tool.

This is not to imply that there was not a broad spectrum of topics covered; there certainly was the usual complement of papers in kinetic theory, plasma physics, etc. (Although I do not recall a single paper on existence theory for the nonlinear Boltzmann equation. Do mathematicians today consider that the work of Di Perna and Lions has answered that question definitively? I beg to differ.) But I did feel that neutrons were making a comeback. One reason was the varied uses now being made of them, such as ecological applications (destroying waste) and applications to non-proliferation and safeguards (discussed in a brilliant presentation by Sara Pozzi of the University of Michigan, a transplanted Italiana). I might mention another paper by M. Trovato of Catania, on a second-quantized version of the Wigner representation of quantum mechanics. This is a problem I had studied myself, in the 1960's, and I was happy to see that someone was making strides to bring my old work up to date. (By the way, the usual spate of papers on the use of the Wigner representation to compute electric currents in semiconductors seemed to have gone the way of all flesh—I do not recall a single presentation on that subject, although I must admit I did not scan all the posters as well as I perhaps should have.)

So much for the technical sessions and the posters. I have already apologized and explained why I could not really do a competent job of reviewing the conference from a scientific point of view. So let me turn to the full and exciting social program. Sunday evening there was a welcome reception, which included a supper. Then Tuesday night everybody was entertained at a reception at the Valentino Castle, while Wednesday afternoon a cultural visit to the Venaria Reale Royal Residence and Gardens was followed by the conference dinner at a local restaurant. In between there were twice-daily coffee breaks at which enough delicacies were served to sate the most rabid sweet tooth. The local organizing committee, which consisted of Pietro Asinari, Sandra Dulla, Roberto Monaco, Miriam Pandolfi and Piero Ravetto, all of Torino, and Giovanni Frosali of Firenze are to be complimented for the extraordinary job they did in organization of both technical and social programs, especially in these economically challenging times, when raising money is so difficult.

Now as promised, a summary of my own career in transport theory. I entered graduate school at Duke University in the fall of 1949, and since I had done my undergraduate work at a technical university (now known as Carnegie-Mellon) I was about a year ahead of the other students who entered at the same time—they had mostly graduated form liberal arts universities. So I was able to skip most of the first-year courses, and was in fact accepted by a thesis adviser, Eugene Greuling, during my first year. Greuling who, along with a plurality of the physics faculty, had only recently returned to his teaching job after spending several years at Los Alamos helping to construct weapons of mass destruction, was asked to spend the summer of 1950 at the Oak Ridge National Laboratory with a task force studying the feasibility of building a nuclear-powered airplane. (The logic behind this was that a simple calculation could prove that no airplane powered with petroleum products such as gasoline or kerosene could fly from the US to Moscow and return; as more fuel was added to the airplane to allow it to fly the added distance the extra weight of the added fuel required more fuel, and so on *ad infinitum*.) Anyway, after who knows how much money was wasted on this absurd project, someone came up with the idea of in-air refueling, rendering the whole concept moot. But it did have one salubrious effect; I spent the summer learning reactor theory from Dr. Greuling.

When I received my Ph. D. a couple of years later, I was hired by General Electric to help design reactors to power submarines. (This turned out not to be an absurd project at all; diesel-powered submarines had to surface regularly to replenish their supply of oxygen, where they became prey to enemy destroyers and aircraft. Nuclear-powered craft could stay submerged much longer.) At GE I had the good fortune to be assigned to work under Henry Hurwitz, a brilliant physicist who gave me the problem of solving the energy-dependent Fourier-transformed Boltzmann equation for a fission source of neutrons. The rationale for this was that Hurwitz and his coworker Richard Ehrlich had just invented the multigroup method for calculating reactors ("multi" was a misnomer; generally two groups were used). Hurwitz wanted to compute the energy spectrum of the fast neutrons (the spectrum of slow neutrons was assumed to be Maxwellian at an appropriate temperature). This spectrum could then be used to generate group cross-sections by averaging the raw cross-section data over it. One important checkpoint was that the second spatial moment of the distribution of slow neutrons (the so-called "age") emerged exactly from the P1 approximation that we were forced to use (actually, we had developed an improvement on P_1, called B_1—named for Hans Bethe, who had suggested it). So if the age came out correctly from the calculation (it had been measured experimentally to be $30\,\mathrm{cm}^2$) we felt we could trust the spectrum to be at least qualitatively correct. Unfortunately, my calculation for the age gave the wrong result, namely $25\,\mathrm{cm}^2$. I might add that to do the calculation I had to learn to program a computer, the IBM CPC ("card programmed computer", with 30 words of internal memory) in machine language, the only programming language which existed at the time. Intermediate steps in the calculation had to be retrieved on punched cards and then reinserted at the appropriate point; the entire calculation took about one hour of computer time. Hurwitz and I tried every trick in the book to figure out why the calculation gave the incorrect value of the age. One theory was that anisotropic scattering by the oxygen was responsible, so we developed a method, later widely adopted by experimental nuclear physicists, for treating anisotropic scattering. Nothing worked. It took five years for Hebert Goldstein and myself to figure out that the experiments were wrong: $25\,\mathrm{cm}^2$ was the correct answer after all. Our work was the basis for the computer code MUFT (Multigroup Fourier Transform) which became a standard in reactor design and helped established my own reputation in reactor physics and transport theory. It was primarily for this work that I was awarded the E.O. Lawrence medal by the US Government in 1972, a medal also awarded to my mentor Hurwitz some years earlier and to my mentee Mitchell Feigenbaum (see below) in 1982.

After five years at GE I became an associate professor of nuclear engineering at the University of Michigan, where I soon fell under the influence of Kenneth Case, a professor in the physics department. (Case was already well-known to transport theorists due to his monograph with de Hoffman and Placzek.) The second year I was at Michigan Case gave a course in transport theory which I audited; the notes I took evolved into our book "Linear Transport Theory." A large number of nuclear engineering students signed up for the course, and many of them took up transport theory as their field of Ph.D. research, some of them under my direction, some under the aegis of Prof. Richard Osborn, of whom Sidney Yip and the late George Mitsis, as well as the three Turks, Ziya Akcasu, Erçument Özizmir and Kaya Imre are among the better-known names. The latter two worked in plasma physics which came to be defined as a viable branch of transport theory mostly due to Case's seminal paper which introduced singular eigenfunctions there before they were used in neutron transport. (In the case of plasmas, it is not obvious what is being transported, but that has not kept plasmas from being a significant topic in ICTT through the years.)

Other students worked under the direction of new faculty members hired into the Michigan nuclear engineering department, especially George Summerfield and Cases's student Fred Shure (both now deceased). Yip and Summerfield both participated in ICTT. Other Michigan students who attended ICTT include Norman McCormick, Charles Siewert and Janus Mika (a student of Case's). Many other well-known workers in transport theory came through Michigan, either as students (*inter alia* the late Joel Ferziger; Max Mendelson; Michael Natelson; Dale Metcalf; Basil Nicolaenko; Surrenda Purohit; Charles Stevens; Marcos Rosenbaum; Richard Nicholson; Paul Plummer; or, as visiting faculty, Roman Zelazny and, above all, Ivan Kuščer. In later years Michigan faculty who have participated in ICTT include Edward Larsen (a frequent collaborator of my own); Bill Martin; Mike Williams; Sara Pozzi, mentioned above; and probably others whom I have either forgotten or am trying to forget.

Kuščer came to Michigan to work with Case in the early 1960's, but Case had forgotten to notify him that he would be at MIT, on sabbatical. So one morning I got a frantic call from a secretary in the physics department, asking me if I could possibly host Kuščer, something I was delighted to do since I already knew his work, primarily on transport of light in seawater, a topic inspired by his love for diving. (He brought his own funding, in the form of an international fellowship, another reason I welcomed him with open arms.) He was accompanied to Ann Arbor by his wife Martina, who died fairly recently, and his two sons. Martina with her vivacious and sparkling personality soon became the life of the party at all the nuclear engineering gatherings, after she learned to speak English, a task that took her about a month. Ivan himself was one of the most talented linguists I have ever met—he spoke almost every standard European language, fluently.

Ivan and I established a wonderfully fruitful collaboration as well as a warm friendship that lasted until his death almost 40 years later. Of course, Kuščer became one of the most celebrated transport theorists of all time; he was chosen to speak about neutron transport theory at the symposium in Vienna in 1972 celebrating the 100th anniversary of the Boltzmann equation. He also contributed mightily at Michigan, publishing with faculty including myself and George Summerfield, inspiring others (Fred Shure) and directing graduate students (he was Norman McCormick's Ph.D. adviser). For example, he, Summerfield and McCormick discovered half-range orthogonality of Case singular eigenfunctions. Although Kuščer was only in Ann Arbor for a single year, it is a truism that his influence was crucial in turning the University of Michigan department of nuclear engineering into the world's preeminent center of transport theory research. Partial recognition of the department's status came from the IAEA, which held an international conference on the University of Michigan campus, in response to my invitation. I chaired the conference and gave the summary talk at the end (with ample assistance from Ivan).

Actually, not all of my activities at Michigan involved transport theory. The university had a swimming pool reactor available for experimental research, although it was underutilized. During my first year in Ann Arbor I was successful in obtaining grants from the AEC and the NSF for research in condensed matter physics using neutrons from the reactor in scattering experiments. The nuclear engineering department hired two experimental physicists, the late Dieter Vincent from Argonne National Laboratory and the late John King, who had been a colleague of mine at General Electric to take over the experimental portion of the grants while I continued to work on the theory. Dieter supervised the construction of a slow chopper and John built a triple axis spectrometer, both dedicated to the study of matter, and both successful both in research and in training students for a period of many years. Among the students going through these programs I recall especially Jack Carpenter and Sam Werner, although there were many more. John, Dieter and I visited Chalk River Laboratories in Canada to learn about similar programs they were operating, and were able successfully to pick the brains of the late Bert Brockhouse who had built the world's first triple axis spectrometer for which he later won the Nobel Prize in physics.

In 1964-65 I took a sabbatical from Michigan, spending a year at the Orta Doğu Technical University in Ankara, Turkey where the late Erdal İnönü was dean. Charles Siewert, who was busy writing his Ph.D. dissertation at this time, accompanied me to Ankara, and successfully completed his doctoral work there. His topic was a singular-eigenfunction solution of the equations of radiative transfer in stellar atmospheres, a major theoretical contribution being the discovery of the degeneracy of the continuum modes. This led him to a distinguished career in problems involving both radiative transfer and gas dynamics; he was really responsible for making these important areas for ICTT. For most or his scientific career he was professor of mathematics at North Carolina State University.

During the time in Turkey I also hooked up with Michigan graduates Özizmir and Imre (who were working in İstanbul) as well as my former student Marcos Rosenbaum, who worked with us at a distance, from Mexico. We wrote one of the early papers on the Wigner formulation of quantum mechanics, a topic which was to dominate the last ten years of my scientific career as well as becoming a major part of ICTT during the late 1990's and 2000's. The work in Turkey was on the linear Wigner problem; my later work was primarily on the nonlinear Wigner equation. Another high point of the year in Turkey, aside from the birth of my first son, was the international, two-week long symposium on conference theory organized by İnönü and myself. (Case, Ferziger and Siewert were among the speakers, so Michigan was well-represented.) Erdal and I worked together for a number of years, attempting to apply group-theoretical methods (his specialty, having been a Wigner student) to the transport equation, but not much came of our efforts.

As I have mentioned above, my early work in transport theory was involved with the energy-dependent but spatially homogeneous (or Fourier-transformed) transport equation. My work at Michigan, inspired by Case, centered on the one-speed but spatially dependent equation, a topic also favored by my group of disciples enumerated above. The idea was that these exact solutions might serve as benchmarks for numerical solutions of practical reactor problems, a field in which one of the participants of the Torino ICTT, Barry Ganapol, has become an expert. In fact he has recently published a treatise on the subject. Ganapol spent some time working with me at Virginia Tech, to whence I departed from Michigan in 1968, and he has been a faithful participant in ICTT since his first meeting in 1977. I continued my work in one-speed transport theory in Blacksburg, and trained a few students who did some commendable work in transport theory, most notably Pekka Silvennoinen, Bart Willis, Selim Sancaktar and Mike Arthur. (Sancaktar actually worked on multigroup transport. Along with Robert Bowden of the VA Tech physics department, we solved the multigroup transport equation, by a method analogous to that used for the one-speed equation. This had been an open problem for a number of years, and our success was so unexpected that Kuščer and Case refused to believe it. In fact, Hans Kaper, the referee for the Journal of Mathematical Physics, rejected the paper, but it was accepted on appeal.)

Because Va Tech did not at first attract the same caliber of graduate student as did Michigan, much of my work was carried out with other faculty (William Greenberg; Robert Bowden; and Cor van der Mee). Because Greenberg and van der Mee were pure mathematicians, they influenced me to move in that direction, for example towards abstract transport theory where the scattering term in the one-speed equation was replaced by an operator with certain symmetry properties (*e.g.*, essential self-adjointness) and the properties of the solutions (including well-posedness) were studied in abstract measure spaces. Greenberg and van der Mee (along with Vladimir Protopopescu) went so far as to write a book on the subject, in direct competition with a book by Hans Kaper (mentioned above, a Dutch mathematician who had emigrated to the US. He was also a sometime participant in ICTT—but not recently—and incidentally also wrote a book with Joel Ferziger, mentioned above as one of the students at Michigan.)

Sometime in the early 70's I spent two months in Ulm, Germany (then one of the world's most beautiful cities with the highest church steeple in the world). My host there was Theo Nonnenmacher; he and I wrote a paper on, as I recall, ion transport. Around then I also spent two months in Pakistan, having been sent by the United Nations (diplomatic passport included) to lecture on reactor physics which, I must admit, was really my first love.

In 1974-75 I took my first sabbatical leave from VA Tech, on which I was supported by a Guggenheim fellowship. I spent the first several months of my leave at Rockefeller University, ostensibly working with Kenneth Case although I spent more time at the Courant Institute working with Edward Larsen who was a junior faculty member there. At Rockefeller I had the pleasure of hobnobbing with George Uhlenbeck and Eugene Wigner. In particular Wigner and I had lunch together every day—a sandwich procured from a local delicatessen—during which he told me about his early days as an engineer in Hungary, and how he converted to physics. How stupid it was of me not to tape his stories and later write them out for the enjoyment of the physics community! There was one incident, however, that I remember vividly: Wigner had been invited to make a speech at a meeting of the American Nuclear Society. He wrote the speech out carefully, as was his wont, and then gave it to me asking for comments before he presented it. It began "Thank you very much for the kind introduction in which, I am afraid, my contributions have been greatly exaggerated." Modesty before the fact!

In March I left Rockefeller to spend the rest of the academic year visiting Aldo Belleni-Morante in Firenze. (Aldo, who had planned to come to the Torino ICTT died a few weeks before it began.) This was my first introduction to Italian mathematics, and I met several mathematicians such as Vinicio Boffi who were to play an important part in my future career. I began to spend more and more time in Italy not only in Firenze both at the Istituto Ulisse Dini (the pure maths department where I was hosted by Profs. Primicerio, later mayor of Firenze, and Fasano) and the applied maths department where after Aldo, Gianni Frosali became my host. Through the years I visited also both Universities in Milano, working with Carlo Cercignani at the Politecnico and Stefano Paveri-Fontana at the University. Stefano and I collaborated for many years, and he spent some time as a visiting professor of physics at Virginia Tech. I also visited Rome, Ancona, Naples, Frascati, Bologna, Cosenza, Torino, Ispra, Padua, and, especially, Catania where I spent a great deal of time and actually lectured in Italian for the first time (on mathematical methods in quantum mechanics). My host there was the late Marcello Anile. He and I helped organize a very successful two-week meeting on chaos in Sicily. The meeting took place in a hotel on the beach (near the town of Noto) and was especially exciting to the American students who were invited to attend since the hotel and the surrounding area were crawling with bare-breasted women. (Americans were then, as now, accustomed to only infants' being permitted to see women's breasts.)

My second sabbatical from VA Tech was spent entirely in Firenze. My daughter Katie attended school at Scuola Media Carducci and I met Gianni Frosali, his wife Gloria and his daughter Francesca for the first time. Since then we have become the closest of friends and we meet at least once a year since I still travel to Firenze and visit the Dipartmento di Matematica Applicata, where Gianni works, even in my retirement. In fact, a portion of this essay was written there, the rest in my apartment in Via Santa Margherita (near the Dome).

Greenberg was the organizer and host of the 1989 ICTT in Blacksburg, honoring Kenneth Case and myself on our 65th and 60th birthdays, respectively. (Thank you, Bill.) The banquet speaker was Mitchell Feigenbaum, a former post-doctoral student of mine who went on to great things as "Mr. Chaos." But it is a little-known fact that Mitchell attended the 1973 ICTT, and actually gave a talk (on the eigenvalues of the nonisotropic-scattering transport operator). At the 1989 meeting, one of the high points was a song, written by Carlo Cercignagni and sung by the entire Italian contingent at the meeting, accompanied on the piano by the multi-talented, late Jerry Pomraning. Jerry, who was a long-time participant in ICTT, died just before the 1999 meeting in Atlanta, and was honored at the banquet there. He and I wrote only a single paper together, in collaboration with Helène Frisch, but I worked with Helène on a number of other projects in the 1990's, mainly as a visitor at the Observatoire de la Côte d'Azur where she worked.

Greenberg influenced me throughout my career at VA Tech; he turned me into a mathematical physicist by teaching me functional analysis, a task begun by my student Basil Nikolaenko at Michigan. We began to work in pure mathematics, beyond the abstract transport theory mentioned above. For example, along with Carlo Cercignani we produced an existence proof for the nonlinear Boltzmann equation with the spatial dependence confined to a discrete lattice (an idea suggested by our weekly seminar in constructive field theory). In conjunction with Jurgen Voigt we even showed that the limit as the lattice spacing tended to zero existed, but in a rather weak topology. Our work, however, never attracted the attention commanded by Di Perna and Lions.

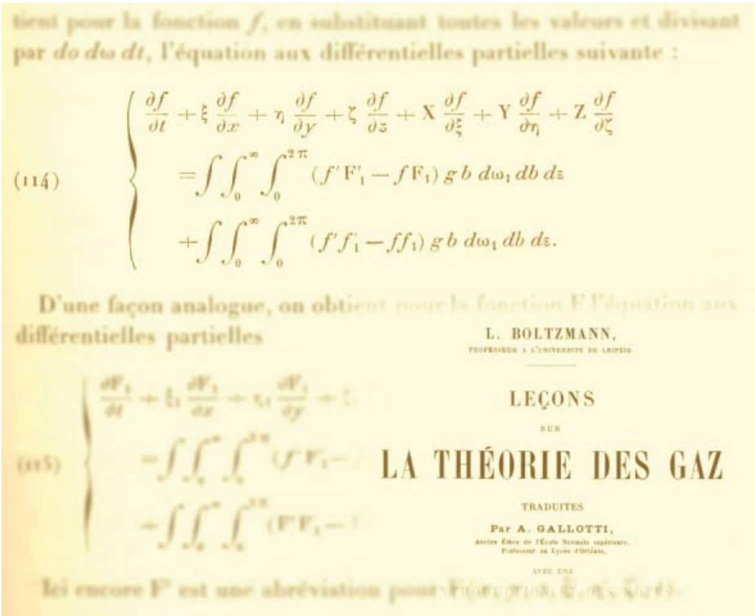
I was invited by Reinhard Illner to spend a semester, in 1990, at the mathematics department of the University of Victoria, in Canada. (Thus began my third and last sabbatical from VA Tech.) There I began to work with Reinhard and a visiting professor from Cologne and Freiburg, Germany, Horst Lange, on the problem of existence and uniqueness of solutions to the nonlinear equation(s) describing the time evolution of the Wigner distribution function. This is an area of transport theory, since it involves the transport of electrons, but it is quantum mechanical, requiring solution of the Schrödinger (or, equivalently, Wigner) equation because electrons can, and do, leak through potential barriers. I did a lot of work in this field, originally with Illner and Lange, Anton Arnold of Vienna, my Ph.D. student Bruce Toomire and later extensively with the group of applied mathematicians in Firenze led by Prof. Frosali, as mentioned above one of the organizers of the Torino ICTT. The second semester of the sabbatical is when I began the work with Frosali and his student Luigi Barletti. It is also when I began my career as a professional musician (see byline above) as I served as choir director at the American church, St. James, in Firenze.

So my career spanned many different areas of transport theory, and really little else, although I did write a Ph.D. thesis in quantum field theory, a topic I continued to follow with extreme interest, but made no contributions to. Ray Streater, one of the giants of the field, did spend two years visiting the physics department at VA Tech and worked closely with one of our students, Lamberto Rondoni, now at Torino.

The mathematical physics program at Virginia Tech was in full swing. In fact, a formal program in mathematical physics, a joint effort between the mathematics and physics departments, was set up. This involved not only a formal degree program of which I was the chair but also a research institute, The Center for Transport Theory and Mathematical Physics, which I also led. Many outstanding students enrolled, with the majority working in some area of field theory or in equilibrium statistical mechanics. Exceptions included the Italian Lucio Demeio who did an important piece of work on instabilities in nonlinear Vlasov plasmas and Jaczek Palewczak, a Polish student of Greenberg's, who worked in kinetic theory. Edmund Rusjan, a student of Kuščer's from Slovenia, was another exception; half of his thesis involved the analysis of an experiment dealing with the transport of muons. Besides Greenberg, George Hagedorn and Martin Klaus in the mathematics department and Joseph Slawny and Robert Bowden in physics were important in this effort. (Slawny and I did produce what I considered an important paper in transport theory, essentially vindicating Case's cavalier treatment of the continuous spectrum of the transport operator, but nobody seemed to care.) Greenberg's recent work might be considered a form of transport theory, since he is interested in well-posedness of various kinetic equations, but he now attend symposia devoted to differential equations rather than ICTT's.

I retired from VA Tech on Jannuary 1, 1996, but continued to work in the physics department for another five years, spending part of that time in Firenze working with Gianni Frosali and his group and part in Freiburg, Germany working with Horst Lange and Anton Arnold. Then I gradually stopped my scientific work, as I was getting more and more calls for my supertitles (go to `pzweifel.com` to learn more) and teaching a course "Introduction to Opera" in the VA Tech music department. My health is not all it used to be, and I very much doubt that I will be attending any of the future ICTT's.

In conclusion, my heartfelt thanks go out to my wife Kathy, who has attended 15 of the ICTT's, and has been of great help in preparing these reminiscences.



tient pour la fonction f, en substituant toutes les valeurs et divisant par $do\, d\omega\, dt$, l'équation aux différentielles partielles suivante :

$$(114)\quad \begin{cases} \dfrac{\partial f}{\partial t} + \xi \dfrac{\partial f}{\partial x} + \eta \dfrac{\partial f}{\partial y} + \zeta \dfrac{\partial f}{\partial z} + X \dfrac{\partial f}{\partial \xi} + Y \dfrac{\partial f}{\partial \eta} + Z \dfrac{\partial f}{\partial \zeta} \\ = \displaystyle\int \int_0^\infty \int_0^{2\pi} (f' F'_1 - f F_1)\, g\, b\, d\omega_1\, db\, d\varepsilon \\ + \displaystyle\int \int_0^\infty \int_0^{2\pi} (f' f'_1 - f f_1)\, g\, b\, d\omega_1\, db\, d\varepsilon. \end{cases}$$

D'une façon analogue, on obtient pour la fonction F l'équation aux différentielles partielles

L. BOLTZMANN,

LEÇONS

SUR

LA THÉORIE DES GAZ

TRADUITES

Par A. GALLOTTI,

The transport equation from L. Boltzmann, "*Leçons sur la théorie des gaz*", translation from German by A. Gallotti and H. Bénard, introduction and notes by Maurice Brillouin, Paris, Gauthier-Villars, Part I, 1902 and Part II, 1905. Reprinted by Editions Jacques Gabay, Paris ©1987. (Reproduced with kind permission.)