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À la recherche des années perdues, or, my life is more interesting than formerly thought

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The Guest Editors, Professors Martin Ostoja-Starzewski and Vitali Nesterenko, have asked me to provide a biographical sketch, and I would like to preface it with warmest thanks to them for organizing this special issue of Acta Mechanica and a symposium which preceded it, held at the 2008 meeting of the Society of Engineering Science (SES) at the University of Illinois, Urbana-Champaign.

While I had envisaged a fittingly brief CV with highlights of my professional career, my colleague Vitali Nesterenko suggested I simply "start writing" without too much thought as to the outcome. I fear I have exceeded anything he could have imagined, and, at the risk of conjuring up Huxley's [6] unkind image of Proust in his bathtub, I ask the reader to indulge me in my recollection of various minutiae of my career. To me, these represent small but significant perturbations on this sometimes unstable trajectory we call life.

I begin with a tribute to my early mentors, especially Helen Hudgens Goddard, who was my elementary-school teacher in grades one through three and who also happened to be my mother. From a large, happy but financially impoverished family, Helen managed against all odds to obtain a college education, subsequently devoting 35 years to elementary-school teaching in the Elvira School District of Johnson County, Illinois. Being somewhat special in her own family, "Miss Helen" pushed me and several generations of other pupils to develop potentials that were frequently smaller than she may have imagined. Secondly, I should express deep appreciation to Toby Hightower, my high school science teacher, for introducing me to the wonders of physics and chemistry, despite my numerous teenage foibles and distractions. It was a special treat to have Toby join in the festivities surrounding the SES symposium, where he made a most memorable contribution to a perhaps richly deserved "roasting".

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Following graduation from high school in Vienna, Illinois, in 1953, I was admitted to the University of Illinois (U of I), Urbana-Champaign and awarded a Johnson County Freshman Scholarship, whose effect on my self-esteem was as least as great as the contribution to my finances. For a considerable time, thereafter, I made ends meet with the usual help from home, meal jobs during the academic year, and factory work during summer breaks. Immediately after matriculation, I made a fateful decision to switch from art to engineering, and, since Chemical Engineering (Ch.E.) was affiliated with Chemistry in the College of Liberal Arts and Sciences, it seemed a logical choice for a major, especially since it involved an extensive curriculum in chemistry. This later led to summer engineering employment, with the Mallinckrodt Chemical Company, St. Louis, Missouri, in 1956, and the Columbia Southern Chemical Company, Barberton, Ohio, in 1957.

As a few of the important influences in my undergraduate education, I should mention an early required Ch.E. course taught by a neophyte assistant professor from Princeton, Thomas Hanratty, as well as an elective one-year quantum chemistry course offered by Martin Karplus, a postdoc fresh out of Linus Pauling's research group at Caltech. I was duly impressed by Tom Hanratty's valiant efforts to reconstruct all the basic equations from first principles, and no less by the ability of Martin Karplus to cover several blackboards with molecular-orbital calculations, only rarely glancing at his notes. I still recall Tom Hanratty's observation to our class that "Those who know *how*, wind up working for those who know *why*." Both these gentlemen went on to stellar accomplishments in their fields, and Professor Karplus recently directed me to a highly entertaining account of his career [8]. I also attended several graduate seminars in Chemical Engineering during my junior and senior years and still recall an elegant lecture, without notes or slides, on the thermodynamics of pipe flow by a brilliant young faculty member from the University of Wisconsin, R. Byron (Bob) Bird. I later enjoyed several interactions with Bob on the rheology of nonNewtonian fluids, and received some possibly unwarranted stimulation from his optimistic assessment of some of my early work on the subject [1].

Among the more noteworthy of the U of I experiences was my residence during my junior and senior years in the Zeta-Chapter house of the Alpha Chi Sigma Chemistry Fraternity. The privileged few undergraduates in the "Axe House" were surrounded by a majority of relatively sophisticated graduate students in chemistry and chemical engineering. They contributed immeasurably to our extra-curricular education, through an extensive collection of classical music on 33 1/3 rpm records, an introduction to various distillates from the U of I chemistry laboratories, and chaperoned visits to local honky-tonks. On a higher academic plane, we enjoyed regular dinner visits and speeches by distinguished chemists, from the U of I and elsewhere, perhaps most memorably, Linus Pauling from Caltech on the heels of his first Nobel Prize. This environment fostered a level of academic discourse unusual in most undergraduate milieu. I recall struggling through Schrödinger's derivation of his famous equation in a paper lent to me by a fraternity brother, and, with somewhat more gratification, helping another brother calculate the plastic confining pressure in the Bridgman anvil, a device employed in his research with Professor ("High-pressure" Harry) Drickamer. The same Professor Drickamer was once quoted as saying that the undergraduate chemical engineering curriculum in those days contained so much material that we could forget ninety percent of it and still be well educated. As observed later by another colleague, it was not clear which 90% we were allowed to forget.[1]

Despite an academic slump in my junior year, the Chemical Engineering Department was kind enough to award me a Kennecott Copper Scholarship in my senior year, and I managed to finish at the top my chemical engineering class in 1957. I was encouraged to apply to five leading graduate programs in chemical engineering. Since I was already toying with the idea of a possible career switch to physical chemistry, I decided on the University of California at Berkeley (UCB), mainly because of its stellar reputation in the field, but also because of classmates at the U of I. David Kearns,[2] a 1956 Ch.E. graduate had already gone there to work with Melvin Calvin on lasers (even before they became "solutions looking for a problem"), and a close friend and B.S. chemistry student from the Axe House, Glen Gordon[3] went to Berkeley to study nuclear chemistry with Glenn T. Seaborg.

With my erstwhile high-school sweetheart (one of several) as new bride (the one and only) and eventual bedrock of my life, Shirley Keltner, we settled in Berkeley in 1957, for my graduate studies in Chemical Engineering, supported by a Dow Graduate Fellowship in the first year and subsequently by a National Science Foundation Graduate Fellowship for the balance of my stay. The Berkeley Chemical Engineering Faculty

[1] Since undergraduate engineering curricula have shrunk from some one hundred fifty semester-hours in those days to less than one hundred thirty today, the question may be regarded as proportionally less important.

[2] Closing a 40-year circle, I discovered that David was on the Chemistry Faculty at UCSD sometime after I moved there in 1991.

[3] Glen's promising career in environmental chemistry at M.I.T. was later tragically curtailed by his untimely passing.

was in a vigorous embryonic phase of growth, with the recruitment of rising stars such as thermodynamicist John Prausnitz, from Princeton, and applied mathematician Andreas Acrivos, from Minnesota.

Chemistry and Chemical Engineering at UCB were more closely integrated than it perhaps is the case today, with weekly joint evening seminars involving the combined faculties and graduate student bodies. I remember that, as part of our graduate requirements, two graduate students would occasionally speak at the beginning for about a half-hour each on a subject of their choosing, followed by questions and, then, by a longer seminar presentation by a senior researcher or professor. For my required seminar, I gave an account of a paper by N. R. Amundson (former Ph.D. advisor of Acrivos and a giant in chemical engineering) et al. on chemical reactor stability, and I established something of a reputation for myself by declaring as "straightforward" the answer to a question posed by a senior member of the audience. I later found out that the ensuing peals of laughter came from those who recognized this gentleman to be William Giauque, winner of a 1949 Nobel Prize for low-temperature physics. As far as I can tell, Professor Giauque forgot or ignored the incident and later allowed me to audit some lectures in his graduate course on thermodynamics, which as I recall consisted of his speaking casually from memory, holding at his side but never consulting a dog-eared copy of Lewis and Randall stuffed with hand-written notes.

Still intrigued but more than a little mystified by quantum mechanics, I audited for a time a graduate physics course on the subject. After one particular lecture that dealt with tunneling, I asked the professor for a physical explanation of how a low-energy particle could get over an high-energy barrier. His pained response convinced me that I should retreat into the arms of classical physics.[4] My decision was subsequently reconfirmed by discussions of quantum field theory over coffee with a brilliant physical chemistry student, Oktay Sinanoğlu, who I seem to recall went directly from his Ph.D. studies at Berkeley to an associate professorship at Yale.

Early on, I took a course in applied mathematics for chemical engineers from Andy Acrivos, who gave elegant lectures without notes, and who was locally known for a frequent "Isn't it obvious?" response to questions he considered as lacking in merit. After working out a singular integral on one of his examinations, I was deemed qualified to do research with him, and thus became one of the fortunate nine or so research students he supervised at UC Berkeley before he moved on to a brilliant career in fluid mechanics, first at Stanford University and, finally, as Einstein Professor and Director of the Levich Institute, in the City College of New York. One classmate, his last Ph.D. student at Berkeley was Andrew Grove, who went from experimental fluid mechanics at Berkeley to become one of the creators of Silicon Valley.

While my Ph.D. thesis dealt with the mathematical modeling of chemical reaction in laminar boundary layers, my later interest in heterogeneous media was sparked by a question put to me by an eminent electrochemical engineer, Professor Charles Tobias, as to the status of a certain approximate theory for the electrical conductivity of particle dispersions.

After a bit of research on the subject sufficient to satisfy Professor Tobias, I decided to make it the basis of the major of two "Propositions", distinct from my field of Ph.D. research, whose oral defense constituted the Doctoral Qualifying Examination (DQE) in Chemical Engineering. It was evident from reading Maxwell [10] (cf. also Kellogg [9]), the interface between media having different permeability can be treated as the locus (single layer) of sources induced by the normal potential gradient. I employed this idea to develop a perturbation theory for the interactions between inclusions in an unbounded matrix, with the goal of improving the classical dilute theory of Maxwell. My eventual presentation of the theory left much to be desired but was deemed passable (barely, I suppose) for my DQE. Putting the matter aside, I was edified some years later by a much more elegant formulation of a similar theory by Gubernatis and Krumhansel [5], while, in the meantime, David Jeffery [7] had employed Batchelor's "renormalization" to work out the second-order correction for random dispersions of spheres, exactly a century after Maxwell's original 1873 work.

Apart from my interactions with the many outstanding professors and students at UC Berkeley, I shall always treasure the time I was granted to ruminate in the great libraries at Berkeley, where I made passing acquaintances with the likes of Hamilton, Kelvin, Lagrange and Maxwell.[5] I am certainly not the first of my generation to lament the gradual erosion of such privileges by the self-inflicted pressures of contemporary university research.

Influenced by similar postdoctoral plans of two fellow Ch.E. students and impressed by certain of the top-notch French graduate students at Berkeley, I decided to undertake postdoctoral studies in France, both

[4] Many years later, I recounted this story to a well-known physicist, Dr. Oreste Piccioni, working on the EPR experiment in a laboratory near my first office at UCSD. Without hesitation, Piccioni asked "What made you think the barrier is always there?", which may have been his version of quantum field theory.

[5] I was reminded of this in the mid-nineties by a remark from G.K. Batchelor that retirement allowed him to achieve once more the exalted state of research student.

for cultural and scientific reasons. This came as something of a surprise to Andy Acrivos, who had advised me to go to the University of Cambridge in the U.K. At any rate, one particularly bright French M.S. student, André Barre, the product of one the grandes écoles and fluent in several European languages, taught me the rudiments of French in 1960 and 1961, and, on completing my Ph.D. thesis in 1961, I was awarded a North Atlantic Treaty Organization Postdoctoral Fellowship to work with E. A. Brun, Professor at the Sorbonne and Director of the Laboratoire d'Aérothermique du C.N.R.S. in Meudon, France, on the outskirts of Paris. As something of an experimental extension of my Ph.D. thesis, I undertook an overly ambitious experimental study of the ortho-para hydrogen conversion on a nickel catalyst in a laminar boundary layer flow. This required the building of a small, hermetically sealed wind tunnel to circulate pure hydrogen, and, owing to the shortage of space in Meudon, Professor Brun secured the necessary shop and laboratory facilities in the Institut Français du Pétrole (IFP) in Rueil Malmaison, where he was a scientific consultant.

During our stay in Paris, we were visited by Andy Grove and his wife, Eva, on a trip back to Hungary after completion of Andy's Ph.D. I once thought he might be seeking guidance from a trusted senior colleague but am relieved that he promptly ignored any advice I may have offered.[6]

After spending more than a year designing and supervising construction of my wind tunnel in an extremely pleasant environment, I was ultimately prevented by safety regulations at the IFP from carrying out experiments with hydrogen. Struggling to recover from this setback, I undertook a surrogate study of the analogous heat transfer problem [4], which I understand was eventually brought to fruition by a series of Brun's doctoral students in his Meudon laboratory. Thus, all was not lost, especially since la ville de lumière has become something of second home.

At the end of our financial resources and with our first child born in a French hospital in the early summer of 1963, we were fortunate to have Andy Acrivos signal my immediate availability for a faculty position to Professor Stuart Churchill, Chairman of the University of Michigan (U of M) Department of Chemical Metallurgical and Materials Engineering (CMM) in Ann Arbor. In lieu of the usual candidate's visit and seminar, I was interviewed in 1963 by a CMM petroleum engineering professor, Rasin Tek, during one of his routine visits to the IFP. After joining the CMM Faculty, I gave a highly mathematical seminar talk on my Ph.D. and postdoctoral research, following which another faculty colleague allowed that he would not have recommended hiring me on that basis. I later redeemed myself, at least in his eyes, by working out a problem he posed on the thermodynamics of a device to extract work from atmospheric temperature fluctuations.[7]

The U of M years and the life in Ann Arbor were replete with intellectual and personal rewards. Our family grew to seven and we established lifelong friends and professional ties. While launching my own research on the rheology of nonNewtonian fluids, I was privileged to work with the distinguished professor Donald L. Katz, on a massive study of the Dow Chemical Company's in situ shale oil recovery process, with colleagues H. Scott Fogler, on ultrasonic hydrodynamics and cavitation, and Jerome Schultz, on biological transport phenomena and blood–biomaterial interactions.

During these formative years, I was heavily influenced like countless others by the works of J.G. Oldroyd on rheology and later by the writings of Truesdell, Coleman and Noll on the foundations of continuum mechanics and thermodynamics. I am deeply grateful for subsequent technical and social contacts with Bernard Coleman, despite his occasional reminders that I may not have read some of his work closely enough, and I was delighted that he could participate in the SES symposium. The contribution of another distinguished colleague K.R. Rajagopal to this journal issue suggests that the last word has not been said on the thermodynamics of plasticity (or, should I say, "the plasticity of thermodynamics"?).

As other fondly remembered colleagues at the U of M, I should mention the eminent fluid mechanician Chia-Shun Yih, who took several junior colleagues under his wing and brought us into contact with luminaries such as G.I. Taylor, S. Chandrasekhar and Clifford Truesdell, whom he invited to Ann Arbor as speakers in conferences or seminars. I recall the further distinction of serving on the doctoral dissertation committees of colleagues such as the late Dudley Saville, of Princeton University, and Charles Vest, of M.I.T. As their names attest, the University of Michigan attracted excellent graduate students, and some of my most rewarding research was done with Ph.D. students such as Chester Miller and Charles Weinberger. Part of my joint work with them on suspension rheology caught the attention of George Batchelor of the Department of Applied Mathematics and Theoretical Physics (DAMTP) in the University of Cambridge, and led

[6] My merits as senior advisor became all the more evident some years later when I heard a rumor that, as guests in the Manhattan apartment of Andy Acrivos, only Andrew Grove and George Batchelor had slept in a proper bed, whereas most of us got the living-room couch.

[7] A thermodynamically admissible macroscopic analog of the Maxwell-daemonic "Brownian ratchet": http://en.wikipedia.org/wiki/Brownian_ratchet.

eventually to an invitation for me to visit. I subsequently spent a sabbatical leave in the DAMTP in 1970-71 as N.S.F. Senior Postdoctoral Fellow, interacting with George and associates, including his research student E.J. Hinch, on microhydrodynamics. Among the many memorable Cambridge experiences, George took me to meet G.I. Taylor in the old Cavendish Laboratory, where we found Sir Geoffrey perched high on a ladder dropping small wire cages into a tall glass column of viscous liquid, in order to study the low-Re slender-body interactions.

A lively disagreement with George Batchelor over the substance of the well-known Frankel-Acrivos [2] model for the viscosity of dense suspensions led me to suggest to him that we simply "remove the fluid altogether", leaving only particle–particle contact. In pursuing that idea some years later [3], I rediscovered a formula unknown to me but routinely employed nowadays for the stress tensor in a granular medium.[8] Armed only with this "hammer", granular mechanics appeared to me as the proverbial "nail", and I later plunged into the field, armed mainly with the fulsome optimism of the uninitiated.

The chance to return to California and the promise of achieving a small but excellent chemical engineering department induced me to join the University of Southern California (USC) in 1976. As the nucleus of a reinvigorated department, I was instrumental in the hiring of four new faculty members from the Ph.D. programs of top-ranked departments of chemical engineering. It gives me considerable satisfaction to note that two of those persons are now chair holders at USC and another has recently been appointed dean of engineering. I am delighted that one of them, Professor Muhammad Sahimi, was a participant in the SES symposium and a contributor to this special journal issue.

At USC I began work in earnest on the mechanics of heterogeneous and granular media, where I owe much to Ph.D. students in chemical engineering such as Yung-Hsiang and Li-Chau Huang (a husband–wife team) and Yaser Bashir, as well as to collaborations and joint research grants with younger USC faculty, including Muhammad Sahimi. Near the end of my stay at USC I decided, despite a lapse in my research funding on granular mechanics,[9] to devote much of summer of 1989 to the problem of anomalous pressure-scaling of elastic wave speeds in granular media. This resulted in a frequently quoted paper, the subject of a perhaps overly generous assessment in an article by James Berryman in this journal issue.

My next and probably last academic move came in 1991, when I joined the Department of Applied Mechanics and Engineering Sciences (AMES) in the University of California, San Diego (UCSD). The promise was irresistible of spending my later years in the extremely pleasant area of La Jolla, in a department that had been or still was academic home to the figures in mechanics such as William Prager, Eric Reissner, S.S. Penner and John Miles, not to mention other respected faculty members. Unfortunately, this kind of large interdisciplinary "engineering-science" group seems no longer viable (especially in large state universities), and AMES has subsequently given way to several, more traditional engineering departments. Despite this concession to current academic realities, my present academic home, the Mechanical and Aerospace Engineering Department, retains a considerable scientific and technical breadth that reflects in part the legacy of its former incarnation as AMES.

I close here with the observation that, while certain "social sciences" may not be true sciences, science is truly "social". Beyond the inspiration, guidance and friendship of great individuals, some of whom in my case are acknowledged above, the least and the greatest of us are ultimately dependent on recognition by our colleagues. I, therefore, am humbly grateful for the kind gestures of Martin, Vitali and all the participants in the SES symposium and the contributors to this volume.

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[8] As pointed out to me much later by my doctoral student Y.-H. Huang, a small algebraic error in that paper accounts for my reported disagreement with a (correct) numerical coefficient given in [2].

[9] Suggesting that it is occasionally better to think about research rather than promising to do so in a research proposal.

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