In 1953, I was preparing my doctoral dissertation at the University of California, Davis when, surprisingly, I was invited to join the faculty. With a very tenuous grip on the bottom rung of the academic ladder, I recognized that my future prospects in academia would be infinitely brighter if I established a research area that was unique, and unrelated to the expertise of other faculty. My interests were in flavor chemistry, and I had been intrigued by some of Ray's early efforts in the newly invented field of gas chromatography. Keene Dimick, a chemist at the USDA Laboratories in Albany, CA, served as a mentor, and helped me construct my first gas chromatograph in 1954. Dr. Dimick later founded the Wilkins Instrument Company, which evolved into the Aerograph Company, and was later acquired by Varian on their entry into gas chromatography.

This was a Neanderthal period, and our self-constructed instruments were incredibly crude - massive flow volumes in the injectors - constructed from quarter inch tubing fittings and topped with a vaccine vial cap - and in the thermal conductivity detector, whose filaments were 10 cm lengths of 1/1000 inch platinum wire that had to be laboriously silver soldered into position, a task I usually undertook around five in the morning when nerves are steadiest.

Columns were constructed from 1/4" Cu tubing, packed with diatomaceous earth, detergent powders, or pulverized firebrick - from which I've inhaled enough asbestos-laden dust to convince me that the dangers of asbestos are greatly over-rated - and coated with copious amounts - up to 25% by weight - of any high boiling liquid we could lay hands on. Amazingly, we did accomplish separations, but I must confess that I shudder to look back on them now.

Golay's experiments with open tubular columns were published in 1956, and in the early 1960s, Roy Teranishi gave me one of the 0.03 inch (0.75 mm) 1000 ft (335 m) stainless steel open tubular columns he had developed for isolating individual fractions from flavor essences. Following Dr. Dimick's departure, Dr. Teranishi had been hired to oversee the GC efforts of the Albany lab; sadly, gas chromatography lost one of its pre-eminent scientists when Roy Teranishi died just three months ago. One must remember that these efforts all pre-dated the ready availability of mass spectrometric equipment; instead, Roy developed techniques for condensing and trapping fractions eluting from the GC column. These were then characterized on the basis of thin film infra red spectroscopy. This necessitated large capacity columns, hence the large diameter. Although crude by modern standards, they surpassed the performance of the packed column, and I was converted.

It was in the mid 1960s that an invitation to present some of our findings identifying the "character-impact-compounds" of the Bartlett (or Williams) pear (as methyl through hexyl esters of $\text{trans-2-cis-4-decadienoic acid}$)

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triggered my first peace-time venture into the European Theater. This, plus later sabbaticals in Austria, Switzerland and Germany, plus several shorter assignments in Poland, Bulgaria, and Yugoslavia provided contacts with a large number of European scientists who helped shape my life and to whom I will always be indebted.

During a 1965 sojourn, I became acquainted with Professor Kurt Grob, who made me a gift of one of his glass capillary columns. Some of the chromatographic results he showed me destroyed what self-complacency I possessed, and I was determined to master the production of these columns. I flew back to the University, cradling in my lap what I suspect was Dr. Hupe’s second machine for drawing glass capillary tubing.

Our university laboratory was soon attracting visiting scientists, post doctoral scholars, and more graduate students applicants than I could possibly supervise, from both foreign and domestic sources. It was the combined efforts of these individuals that resulted in a number of contributions to the field of gas chromatography, including improved methods of trapping and isolating GC fractions; a host of compound identifications, including that of saffrole (a suspected carcinogen) and myristicin (hallucogenic compound) in oil of black pepper; the use of porous polymers as trapping substrates for the subsequent analysis of vapor-borne volatiles, later expanded to what we now term "purge and trap" methodologies; an all-glass inlet splitter with an integrated mixing device; a high pressure device permitting the use of liquid carbon dioxide in Soxhlet extraction, providing "solvent free" essences for GC analysis; the graphite ferrule that revolutionized methods of interfacing open tubular columns to the injector and detector; a recycling GC system that developed slightly over 2,000,000 theoretical plates; the first stationary phase specifically tailored to maximize the relative retentions of all solute pairs in a given mixture; computer-generation of van Deemter curves that permitted theoretical assessments of variables such as column dimensions, including the thickness of the stationary phase film, and solute diffusivities in the stationary and mobile phases. It is important to recognize that this involved joint efforts to which graduate students, post doctoral scholars, and lab assistants all contributed. I owe much to many people.

As in many of our more prestigious institutions, UC’s Departments of Chemistry had no tolerance for applied chemistry, when the applications were conducted within those departments. Intra-departmental chemistry had to be "pure" organic chemistry, or "pure" physical chemistry. Because I was doing applied chemistry, it is fortunate that I was in a Department of Food Science. I carried an Experiment Station title of Chemist, was housed in the chemistry building, utilized on qualifying and thesis committees of graduate students in Chemistry, and tolerated because I was in fact assigned to an "Applied" department.

At the same time, the chairman of the Department of Food Science expressed a strong desire that I do something "more clearly related to the food processing industries". Today, even a young academician can usually ignore such pressures, but 40 years ago, that would have been most unwise. Ultimately yielding to his insistent "suggestions", I began an additional research project in the field of hard surface detergency.

By applying chemical kinetics to study the removal of thin layers of radio-labeled tristearin from glass and stainless steel surfaces by a circulating fluid, we found that the molecules comprising thin films on hard surfaces apparently existed only at one of two discrete energy levels, a high energy level that was more easily displaced, and a low energy level that was more firmly anchored. The more tightly bound film could be 20 to 30 molecular thicknesses, it wasn't a question of adsorption vs simple deposition. We were also able to establish that certain physical treatments
could encourage a transition from the low energy level to the high energy level, and vice versa. Serendipitously, this information proved invaluable in later efforts to produce stable coatings of stationary phases on glass capillary columns, and eventually led to the design of a coating machine capable of routine production of highly efficient glass capillary columns.

By this time, there were a number of "centers of excellence" in glass capillary columns, mostly in the UK, Germany, Belgium, the Netherlands, Czechoslovakia, and Hungary, with a few scattered in other places, including the US. Most of these were in universities or other institutions, and a very few were in industrial companies. Whole careers were being built on glass capillary columns, and the results generated from these activities dominated many of our scientific meetings.

In 1974, one of my completing doctoral students, Rob Wohleb, mulled over the fact that the many job offers he was receiving had a common thread: all were based on the supposition that he had acquired the skills for "routine production" of glass capillary columns. Recognizing the commercial potential, he suggested we form a commercial company manufacturing glass capillary columns. The University was already permitting me to accept a large number of outside consulting and teaching assignments on glass capillary gas chromatography, provided they were done on my own time. When I now explored with them the benefits and hazards inherent in a commercial endeavor, they agreed that I could play a minor role in the initial phases, subject to certain well defined restrictions: 1) no entanglement in the day-to-day operations; 2) no relationship between my university research and matters that could benefit the company; and 3) no remunerations from the company. I could -- on my own time -- continue to engage in "extended education", including extra-curricular teaching, and handling trouble-shooting queries.

Under these restrictions, J&W Scientific, Inc., began its existence in a Sacramento garage in 1974. The first year was slow, but as business increased, space limitations forced our move to larger facilities in Rancho Cordova. At its peak as a manufacturer of glass capillary columns, J&W was simultaneously operating six glass capillary drawing machines, plus several of the coating machines. Fortuitously, one of the latter developed a faulty thermocouple, causing the operator to greatly increase the temperature of the heated inlet tube. The unexpected longevity of the columns produced in this "error mode" led to the invention of bonded-phase glass capillary columns, and the "DB-" trademark. As detailed in Ettre's recent paper in LC/GC, many scientists were engaged in efforts toward bonded phases, but Leslie either missed or ignored the earlier paper by Rand Jenkins and Rob Wohleb at Professor Zlatkis' Expochem 1978 in Houston, reporting this work AND the commercial availability of bonded "DB-" columns.

In the early 1970s, shortly after Hewlett Packard acquired the F&M company, one of their representatives became interested in our efforts in glass capillary columns, and I was asked to present a seminar to their group in Avondale. Shortly thereafter, I was hired as a consultant, first on interfacing these columns to their instruments of that time, then on instrumental design features, thence to the instruction of an abbreviated "capillary section" of their GC courses, and ultimately to the instruction of an extensive series of courses dedicated to glass capillary gas chromatography. Fortunately, the University looked favorably on these teaching and consulting activities, accepting my arguments that such "real world" experiences bolstered my academic teaching expertise. Some 30 to 40 courses were conducted throughout the continental U.S. and Canada each year, continuing into the early 1980s when J&W and HP became competitors in column manufacture. It was during this period that HP hired Terry Rooney and Bob
Freeman and the three of us barnstormed the U.S. and Canada, forming a bond of camaraderie that lasts to this day. My history with the "old" HP is extensive, and I owe much to many individuals -- Jerry Johnson, Gary Beach, Ivan Crockett, Bill Hart, plus many, many more.

By the late 1970s, J&W was one of the leading manufacturers of glass capillary columns. A number of academic researchers and even a few industrial firms -- e.g., elements of the petroleum industries; sections of Dow, Eastman, and 3-M; Stauffer Chemical, and the Jockey Club of Hong Kong -- had made at least a partial conversion to open tubular columns. Most industrial chemists, however, continued to employ packed columns. While recognizing their advantages, they could not afford the luxury of "down time", and were wary of the fragility of these columns. Compounding the problem, GC methods of testing and control, as specified by the Food and Drug Administration, EPA, etc., were invariably based on packed column analyses. Even so, there was some growth in the market place, and J&W was grew with it. In May of 1979, Ray Dandenau introduced the fused silica column; it seemed obvious that with this new robust tubing, industry would begin the swing to open tubular columns. J&W quickly switched to fused silica columns.

But there was resistance to the use of this new material - - rarely mentioned today, and it came from those whose established reputations were in glass capillary columns. Many of the "Centers of Excellence" in glass capillary columns now found that fused silica tubing was not only less available, it was many times as expensive. Abruptly, a number of prestigious careers seemed threatened. Those of us presenting work using these new columns - - notably myself and Tom Stark of Hewlett Packard - - were not popular; indeed, some meetings actually refused to allow papers or discussions that hinged on fused silica columns. At one international meeting I was admonished from the Chair that "fused silica appeals only to Americans, and only because they are too clumsy to handle glass". Nonetheless, with the fragility problem solved, industry began a massive conversion to open tubular columns, and J&W began experiencing yearly growth rates of 25 to 50%.

But then we became interested in the possibilities of a UV-compatible fiber optic spectrophotometer. This was soon consuming a good portion of our cash flow, as we rapidly assembled four or five of these instruments for the 1984 Pittsburgh Conference. These employed small diameter open tubular LC columns of fused silica, with an integrated zero dead volume flow cell. The latter involved placing four 90° bends in the column to provide an out-of-line straight section ca. 3 cm long, to each end of which an optical fiber of fused silica was laser-welded.

Unfortunately, the chief architect of the project had failed to advise management that under exposure to high energy UV, fused silica undergoes "agglomeration" -- the trace levels of impurities that remain in even the highest quality fused silica assimilate, causing the transmission efficiency of the fiber to plummet. With a new unexposed cell, transmission was high, even at 170 nm; subjected to high intensity UV, it climbed to 190, then 210, and on into the visible -- UV transmission was blocked, and the cell -- whose construction costs were not trivial -- had to be replaced.

Just as we discovered this, a payroll was looming, and we had depleted our meager cash reserves. With my wife’s blessing, I withdrew the small nest egg we had been accumulating toward retirement (I was then 62 years of age) and used it to discharge our payroll obligations. Again with my wife’s concurrence, we mortgaged our home and all other assets (as did Dr. Wohleb) to obtain a line of credit that permitted us to continue operations -- which we now
restricted to the manufacture and sale of fused silica columns. Today, Erika - my most prized souvenir of World War II - can do no wrong. What she wants, she gets.

Today, as an older (and I hope wiser) individual, I would be appalled at the risk we took, but fortunately, it worked. Industry was making a massive shift into fused silica columns, and because the booming business required more of my time, I asked the University to change my appointment to 50% "phased retirement", an option that would reduce my University salary by 50%, activate a reduced level of retirement benefits, remove the restriction against my accepting remuneration from J&W, and (I reasoned), result in a 50% reduction of my responsibilities at the University, where I was still carrying one of the heaviest teaching loads in the department. Unfortunately, my workload at the University remained unchanged. In late 1987, I asked that my phased retirement be changed to 10% active, 90% retired. Again, my University workload remained the same: in 1989, I fully retired from the University and was named an Emeritus Professor.

During the above period, 1986, J&W was sold to Fisons, plc, a British pharmaceutical firm that was branching into different areas. Dr. Wohleb retired to spend more time with his family, and I was asked to continue as a Consultant. Some eight years later, Fisons began encountering financial difficulties, and in 1996 succumbed to a hostile take-over from Rhone-Polenc; during the final stages of that period, a group of investment bankers asked me to "figure-head" a "management buy-back" of the company, that was, from their standpoint, successful.

These were not wholly pleasant times; our investment bankers were interested not in long-term growth, but in yearly profits, and we became a "cash cow". Three to four years is about the time frame that such investors restructure portfolios, and in 1999, we were on the auction block. There were perhaps six "serious" bidders, and we were ultimately purchased by Agilent Technologies.

When, late in 1999, Agilent emerged as one of the strongest bidders for J&W, I had some reservations. J&W had an established reputation for excellence in free technical support. With six-to-eight experienced chemists assigned to our Technical Support Group, we were responding to approximately 1,200 queries every per month on a "no charge" basis, using a toll-free number. The expenses connected with such an operation are not trivial, but we were convinced that free, competent, and readily available technical support, as an integral part of a larger commitment to address and respond to the needs of each individual customer, was largely responsible for our high degree of customer loyalty and large market share. Customer-oriented dedication had permitted us to introduce tighter column specifications, ensuring the highest column-to-column reproducibilities, and a policy of "24 hour shipment or the next column is free". A few years earlier, Hewlett Packard had begun charging the customer for technical support, and my personal contacts with users in the field indicated a public perception that HP, secure in their position as the pre-eminent instrument manufacturer, no longer catered to the customer. Agilent, of course, was derived from HP; what was their attitude toward these customer support issues?

I raised these questions with the Agilent hierarchy during the negotiations, and was assured on several occasions that J&W had attracted Agilent’s attention largely because we "were obviously doing some things right": rather than change our methods, they planned to copy them; technical support would be expanded, and would continue as a free service. Our other manufacturing activities -- e.g., flow meters, gas scrubbers and purifiers, syringes -- would be transferred to other sites. But the Agilent (formerly HP) column production facilities would be moved to the J&W
site at Folsom, which as the sole GC column production facility, would manufacture both J&W and HP columns to their original specifications. The "J&W Scientific" name would continue, albeit under the Agilent banner, and the availability of both "J&W" and "HP" columns would continue.

It took some time to convince me, but I now believe in the sincerity of those assurances. As a result, I view the merger of J&W and Agilent as customer-beneficial: a wedding of the prime supplier of the best in GC instrumentation, with the leading manufacturer of open tubular GC columns. I've spent the last two weeks in Europe, working with a number of Agilent's field reps. Aside from a snow storm that closed the Dublin airport and forced me to cancel an invited lecture for the Chromatographic Society scheduled the following day, it was a pleasant experience. I was very favorably impressed by the rapport that these individuals had established with their customer base.

As detailed earlier, several decades have passed since I convinced HP chromatographers that for the vast majority of applications, packed columns were passé (this was not always easy!). In a sense, I've now returned to that fold, and at 79 years of age, I'm looking forward to continuing a strong inter-relationship with our customers in particular, and the chromatographic community in general.