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Biographical sketch of Paul Tarrant

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1. Formative years

Paul Tarrant was born on All Saints' Day (November 1) 1914 in Birmingham, Alabama, USA. His full name then was Paul Tarrant Brittain, but his mother died soon after his birth, so he was raised by his maternal grandparents, James and Margie Tarrant, hence the formal change later. Paul had a happy childhood, playing baseball, swimming in the nearby creeks, and caddying golf when he got the chance, including once at the Southern Amateur Championship. He attended Robinson Grammar School, four blocks from his home, and Woodlawn High School, three miles away. During his high school and college years, he worked in his Uncle Paul's drug store in downtown Birmingham; his job as a soda jerk was really the first indication of intellectual potential and foreshadowed what he was to become.

P. Tarrant

After high school, Paul went to nearby Howard College (now called Samford University) in East Lake, which was within walking distance from home. His Uncle Felix helped pay his tuition for the first year, and Paul put in 36 h per week (6:30–10:30 p.m.) at the drug store (and full time during the summers) while working on assistantships in the Physics and Chemistry Departments at Howard in order to cover his tuition fees and expenses. He graduated with a B.S. degree in chemistry in 1936 and entered Purdue University (Lafayette, Indiana) in the fall of that year.

2. At Purdue with McBee

During his last year at Howard, Paul met Viola Griffin, with whom he eloped in the summer of 1937, after 1 year on the Master's programme in chemistry at Purdue. Paul held an National Youth Administration (NYA) job at Purdue at first (paying a maximum of $22.50 monthly), but quickly also obtained a quarter-time assistantship; this gave him another $35 per month, for which he lectured a course in general chemistry. He chose Professor Earl T. McBee as his research director. McBee had not yet started doing fluorine chemistry, but while Paul was in his group a fellow student visited Professor Albert L. Henne at Ohio State University to learn about current research in organofluorine chemistry. That was the first time that Paul became aware of the field of fluorine chemistry, which was in its infancy. Purdue's chemistry department was closely linked to many companies, such as Commercial Solvents, Dow, and Westinghouse, with research fellowships being endowed by these companies and research areas thus being dictated by them. For example, McBee's group was engaged in research on chlorination and nitration methodology, which was of interest to Commercial Solvents. One of Paul's projects while working with McBee was to try to convert 1,3-dichlorobutane to methylcyclopropane with zinc, in a manner analogous to the conversion of 1,3-dichloropropane to the parent cyclopropane, which at that time had gained some notoriety as an anaesthetic. So Paul's initiation into research was in the area of chloro organics. Later, when considering where to do his Ph.D degree, he was attracted to fluorine chemistry because of his experience of chlorine chemistry.

3. At Duke with Bigelow

Paul obtained his M.S. degree from Purdue in 1938 and returned home to teach high school physics and chemistry for 2 years in Birmingham (Ramsey Technical High School), at a salary of $1135 per year. Realizing that it was going to be difficult to support a family on that salary, he decided to seek a Ph.D degree, and travelled out to Stanford University (California) in 1940. Stanford's chemistry programme was

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somewhat in disarray at that time, since the Head of the Department for 25 years had just retired and a number of professors were not on campus when Paul arrived. As a result he decided not to enroll, and instead took a job at the Shell Development Company in Emoryville (CA), where he stayed for 1 year, working on plasticizers for rubber; he was earning $170 a month at the time he quit. Having time for a more considered decision about graduate schools, he thought long and hard about where he wanted to study for a Ph.D degree. He considered applying to schools at Duke, Indiana, Texas, and North Carolina but discarded the Texas option because it didn't have much money for assistantships, and eliminated Indiana because he thought the town was too small for Viola to find a good secretarial job. Using the College Blue Book, he learned that Professor Lucius A. Bigelow's group at Duke University (Durham, North Carolina) was active in the field of synthesis of organic fluorine compounds, so he applied there and was awarded an assistantship in 1941. Paul made his decision to work in the field of fluorine chemistry because he thought that 'fluorine chemistry would be something like chlorine chemistry. Was that ever a wrong assumption!

When Bigelow was hired by Duke University in the late 1920s, he had been encouraged to do research in the field of fluorine chemistry by the Chairman of the Chemistry Department, Professor Paul M. Gross. Paul arrived at Duke holding a quarter-time assistantship, but after 2 months the department obtained a Navy contract that allowed him to move to a full-time research position at $150 a month while he worked on the development of inert fluorinated fluids needed for the Manhattan Project. Viola got a job next door in the Law School, and she and Paul would generally have lunch together in the Duke Gardens. It is interesting to note that all workers at Duke who were working with F_2 were required, as a preventative measure, to drink a quart (about a liter) of milk a day!

Bigelow's group generated their own F_2 and this made for some very interesting and exciting experiences during Paul's time at Duke. His work involved the direct fluorination of 'deactivated' aromatics, such as 4-chloro-1,3-bis(-trifluoromethyl)benzene, although he also did a considerable amount of work on Cl-for-F exchange reactions using SbF_3 in HF. His Ph.D Thesis was aptly entitled 'Fluorination of Organic Compounds,' and some of his results were published a few years later by Bigelow in papers entitled 'The Action of Elementary Fluorine upon Organic Compounds. XII. Vapor Phase Addition to Certain Deactivated or Condensed Aromatic Rings' [1] and 'Fluorine as a Halogen. Reaction with a Highly Deactivated Aromatic Nucleus' [2]. Paul graduated in 1944 and immediately went to work for the American Cyanamid Company in Stamford, Connecticut, at the recommendation of Professor Paul Gross, who was a consultant for the company. Through Gross, he knew that American Cyanamid was actively involved in the development of a fluorinated insecticide, fluoroacetamide.

4. At American Cyanamid

Paul's initial project at American Cyanamid was to develop a more practical route to fluoroacetamide. The earlier method, which introduced the fluorine via a diazonium route, had led to an explosion and the blinding of one researcher; so the company was looking for something better. Paul successfully developed an excellent Cl-for-F exchange process with chloroacetamide, using acetamide as the solvent. In the course of this work, he had reason to distil 1 Kg of ethyl fluoroacetate in an open-bay lab! The entomologists who conducted the experiments on the effectiveness of ethyl fluoroacetate as a fumigant put a little in a Petri dish, and when it had evaporated, it was found that every insect in the huge greenhouse had been killed. It wasn't long thereafter when a worker mysteriously died, and testing led to the death of a few rabbits, that the clearly demonstrated great toxicity of monofluoroacetic acid derivatives led to the termination of the entire project; only later was it learned that the Allies (WWII) were aware of the extreme toxicity of this class of compounds, and had plans to make some on a large scale. Paul then started working on polymers derived from CTFE (CF_2=CFCl) and the preparation of trifluoroacetic acid from CF_3CCl=CCl_2 via a method suggested by Max Gergel. Incidentally, the first paper with Paul's name on it derived from his work at American Cyanamid ('The Preparation of DDT using Hydrogen Fluoride as the Condensing Agent') [3].

American Cyanamid was a great place to work, and Paul indeed did work — 6 days a week from dawn to dark (in the winter). Saturday nights were generally spent playing cards (hearts) with Viola and her mother, Ava Griffin, who lived with them. In the end, however, Paul grew tired of the dreary Connecticut winters and sought to return to the South.

5. At UF

Paul applied for a few academic jobs at Ph.D granting institutions in the South and got an interview with the University of Florida (UF). Often in those days people were not brought in to be interviewed but generally seen at a central location, usually during an ACS meeting. Thus, Paul was interviewed by Professor Jack Hawkins in Atlantic City, early in 1946. He was hired, along with about 12 other new faculty (hired to handle the expected increase in student numbers due to the return of the veterans to school), but only Paul, George Butler and T. W. Stearns ended up staying for more than a couple of years. Its easy to understand why they might have not stayed, given the conditions that prevailed in Gainesville and at the University in 1946. For a start, there was no decent housing for Paul and Viola. After staying in a rooming house for 2 months, they were moved to university-owned housing that was located at Stengel Field (a small airport located at the site of the current Butler Plaza on

Archer Road), and for 6 weeks this accommodation had no hot water, and the wind, sand and mud (after rain) made life almost unbearable there. They survived somehow for a year, during which time Paul almost moved to Jefferson Chemical Company in Port Arthur, Texas. However, Paul and Vi decided that Port Arthur was even worse than Gainesville! To compound matters, the new faculty hires had been promised that there would be an extension built to the overcrowded Leigh Hall, which housed not only Chemistry but also the Pharmacy School. However, when the bids came in too high, the project was abandoned. Paul and George had been hired as Instructors, not Assistant Professors, and when they found out that they were the only Instructors on campus with Ph.D degrees, they protested to Dean Townes Leigh, who was also Chairman of the Department. The Departmental Policy and Procurement Committee, which was in charge of such decisions, refused to budge, but when Paul and George threatened to leave, Leigh gave in and they became Assistant Professors in 1947. This was not to be the only time that Paul and George had to stand up for themselves in those early years.

Before Paul and the others came to UF in 1946, the Chemistry faculty comprised organic chemists Leigh and Cash Pollard, physical chemist Jack Hawkins, water chemist A. P. Black, analytical chemist Fred Heath, and Vestus T. Jackson. It was an all-male school, except for the pharmacy and graduate programmes. Paul's teaching assignment during those early years included four sections of introductory organic chemistry for pharmacy students, which entailed two lectures and four discussion sessions per week. He also taught two 3 h labs and a beginning course in physical organic chemistry (two lectures per week) to graduate students. There were about 35 students in the chemistry graduate programme at that time.

Paul's first graduate students were John Young and Henry Brown, with Dale Warner to follow shortly thereafter. The work of these students led to Paul's first papers in organofluorine chemistry at UF, e.g. 'The Preparation of Some Derivatives of Chlorofluoroacetic Acid' [4]. In those days many of the graduate students came from one's own undergraduate programme, and when Paul and George arrived on campus, they found that most of these undergraduates had already committed to join the research groups of one of the older faculty. Pollard had been the first professor at UF to direct Ph.D students, and he aggressively defended his turf as senior organic professor. Thus, Paul had to work hard to hold his own and to build his research programme in those early years at UF.

Prior to WWII not many organic compounds containing fluorine were known; the aliphatic ones were most often reported by Frédéric Swarts, the Belgian chemist, and Al Henne at Ohio State, who hailed from Belgium. However, a great deal of interest in organic fluorine compounds derived from the Manhattan Project, and in about 1946 Hanford and others at DuPont published reports concerning reactions between fluoro-olefins and alcohols or amines. These results demonstrated that it was possible to prepare fluorinated aliphatic compounds without using HF, F_2 or SbF_3 and thus opened the field to more conventional organic chemists. Still, it was an adventure to make new fluorinated compounds, and even more so to identify them: one did not have the luxury of ^{19}F NMR!

The usual way to identify compounds in those days was by determination of molar refractions; of course one needed to have some idea of the molecular weight of a compound in order to calculate its 'theoretical' molar refraction. Paul gave me the following example of such difficulties: 'Once we treated $CF_2{=}CCl_2$ with phenylmagnesium bromide and obtained a product, $PhCF{=}CCl_2$, that we could not identify for several months (at the time, there was no precedent for such an addition-elimination reaction of a fluorochloroolefin). When we finally guessed the correct structure and thus its molar weight, and calculated its expected molecular refraction, it hit the measured value right on the button′. Another reason why fluorine chemistry was so exciting in the 40s and 50s was that quite unexpected results were often obtained. Paul went on to say: 'We blew up several autoclaves and had many reaction products plastered on ceilings. Chemistry was more fun in those days. Today, those who carry out reactions in NMR tubes and identify products via the myriad of modern techniques available are definitely missing something'.

Paul, Vi and Mrs. Griffin lived out at Stengel Field for a year, then moved to a duplex on the east side of town, at 1033 NE 8th Avenue. They lived in the duplex for 1 year, until construction of their home at 1723 NW 12th Road was completed. In the meantime Paul and Vi started their family, with daughter Linda being born in 1947.

The late 1940s were times of rapid growth and remarkable evolution with regard to the role of research at Universities in the United States, and young professors like Paul and George were to be the instruments of such change. In 1948, Lou Butz, a representative of the Office of Naval Research, came to the University to talk to people in the Chemistry Department about a new Federal initiative, that of funding research. (This small but successful initiative to fund university research, mostly in applied science, led shortly thereafter to the creation of the National Science Foundation.) Three chemistry faculty were awarded contracts by ONR as a result of the visit by Butz: Paul, George Butler, and Cash Pollard. Paul's was the first to come through, and in order to accept the $10,000 contract, he needed the signature of Dean Leigh, who told him that he'd first have to get it approved by the Dean of the Law School. Once the Dean had approved the contract, Leigh signed, which broke the ice with respect to all future research funding within the College of Arts and Sciences at the University of Florida. Paul's contract was to carry out research in the area of 'fluorine-containing olefin chemistry', and the contract lasted about 3 years. One published piece of work carried

out under this contract involved a synthesis of ethyl difluoroacetate [5]:

$$CHF_2CF_2Cl + KOH/EtOH \rightarrow [CF_2{=}CF_2] \rightarrow CHF_2CF_2OEt \rightarrow (\text{with } H_2SO_4)CHF_2CO_2H$$

Two years later, Quartermaster chemists from the Army's 'Arctic Rubber Program' came down to solicit help from Paul. They were interested in a much larger commitment from him — one which would include retaining him as a paid consultant. This was something new to UF. The authorities did not approve of any faculty member doing 'outside' work, and thus Paul's request was turned down, first by Dean Ralph Page and then by Vice President John Allen. Paul then went to see President J. Hillis Miller to discuss the matter. Miller, as is still the case with the majority of university presidents, found it hard to turn down any source of money; also, he understood the value of compromise and suggested that Paul go off State salary and 'do full-time research' for the duration of his contract. This Paul did for 3 years, until A. P. Black, the Chairman of the Department, insisted that he return to teaching. However, the Army contract rolled on for a total of 16 years. Initially they made monomers, including lots of fluorinated dienes, such as perfluoroisoprene, and studied their reactions. In the process, Paul's group carried out fundamental synthetic work, developing methodology based on free-radical reactions involving hydrocarbon or fluorinated alkenes, e.g. $CF_2BrCFClBr + CH_3CH{=}CH_2$ (at 80°C, benzoyl peroxide)$\rightarrow CF_2BrCFClCH_2CHBrCH_3\rightarrow$(with KOH in ethanol) $CF_2BrCFClCH{=}CHCH_3\rightarrow$(with Zn in isopropanol) $CF_2{=}CFCH{=}CHCH_3$ [6]. In 1956, Paul's group made 100 g of $CF_3N{=}O$ for the Army (using Hazeldine's photochemical $CF_3I + NO$ method) and he personally took it to Washington by air, carrying it in a sealed tube cooled in dry ice. What in the world would happen if he tried that today?

Paul's research programme evolved continuously, as reflected by the diversity of his publications during that 16-year Army contract period. As Paul said, 'We made a lot of compounds and educated a lot of students as a result of Army funding'. Among the 'students' he has mentioned to me were Al Lovelace, Bob Taylor, Marv Lilliquist, Mary Louise VanNatta and also a number of postdoctoral fellows, including Ron Richardson, Peter Johncock, David Sayers, Jim Heyes, Fred Drakesmith amd Don Lomas, all of whom were trained in fluorine chemistry at either Durham or Birmingham University in the UK.

The mid-50s through the early 60s were the heydays for Paul's research group. Although never more than eight strong, with four or five being supported by the Army, every day was fun, filled with exciting chemistry and stimulating discussions. At his farewell banquet at Wright-Patterson AFB in 1973, just before taking up his appointment as an Undersecretary of the Air Force, Al Lovelace commented, 'I have seen many other good research groups, but I don't believe that any one of them was as good as we were in those days at UF'. Al later went on to become Administrator of NASA.

The Army contract evolved into a subcontract with the Air Force, through TRW, and eventually to direct funding from Wright-Patterson AFB for another 3–4 years. Included among the work done with Air Force support were novel photochemical studies on cycloadditions of hexafluoroacetone to alkenes. During this time, Paul also had a Navy subcontract through MIT to make fluorinated fluids for gyroscopes, and a NASA subcontract through Cal Tech. In the early 70s he obtained an NSF grant, but by the end of that decade his funding had essentially dried up, consequently he did less research. He decided to retire in 1981 at age 66 so that he could give his full attention to teaching his second wife, Marian (a Yankee), how to become a rebel. His final paper was published in 1988 ('The Reaction of some 3- and 4-Fluorooxetanes with Acids' [7]).

Paul's professional achievements during his academic career at the University of Florida were considerable. He rose rapidly through the ranks at UF, attaining the rank of Professor in 1957. With a natural wit and an uncanny ability to defuse the most volatile of situations, he was the 'glue' of the Organic Division, serving as its Chairman for 15 years. In total, he directed the Ph.D degree work of 20 young men and women, as well as the M.S. studies of another 20; also he served as mentor to about 25 postdoctoral fellows, being more like a surrogate father than a boss. (Paul still maintains close contacts with most of his former students and postdocs). With 58 pioneering papers and 17 patents in the field of synthetic organofluorine chemistry to his credit, Paul certainly made significant contributions to the field of fluorine chemistry, and these have been acknowledged on numerous occasions. He received ACS recognition through the Florida Section Award in 1966, the Southern Chemist Award in 1963, and the Fluorine Division Award for Creative Work in Fluorine Chemistry in 1976.

Paul was very active in the Fluorine Division of the American Chemical Society (ACS), serving as secretary of the Fluorine Subsection of the Industrial Chemistry Section for several years, and, when the Fluorine Section was formed in 1960, becoming its first Chairman. He was instrumental in the continuation of the International Fluorine Symposia (the first was held in England at Birmingham University in 1959, with no plan then for a second), and helped to organize the second in the series at Estes Park, Colorado, in 1962. Estes Park proved so attractive and popular a site that the fourth meeting was also held there in 1968. By that time Paul had commenced his duties as Editor of Fluorine Chemistry Reviews, a Marcel Dekker series which ran to eight volumes during the period 1967–77.

6. PCR

During the early years at UF, Paul and George were always on the lookout for ways to make a little extra money.

After all, their starting salaries were only $3200 per year, and raises rarely amounted to more than $100 per year. They came up with a number of abortive moneymaking projects before hitting on the idea of starting what was to become PCR.

Their first project was to make some super-high-energy racing fuel for a guy who came around looking for suckers, but although they made 5 gals of nitropropane for this fellow, he never showed up to collect it and pay up (later, Paul and George found out that he was in jail for threatening an FBI agent). Next, they tried working with the Naval Stores Lab in Olustee where Professor Hawkins had been consulting. Olustee had discovered that the adduct derived by heating maleic anhydride with the resin acid abietic acid made a good paper size. So George and Paul obtained 5 gals of pine tree drippings (collected for its turpentine) containing abietic acid, made 20 lbs of the adduct, and sent it off to the paper company for evaluation and, hopefully, purchase. Nothing came of that, so George, Paul and four others each put up $100 in partnership with Stan Wemberley (a friend and Associate Dean at UF) to buy fibreglass to build boats. After much frustration and many wasted fibreglass-making weekends, Stan returned $80 to each contributor and they called it quits.

Finally, in 1952, George and Paul decided to make and sell chemicals, specifically trifluoroacetone and various allyl compounds. They incorporated under the name of Penninsular Chem Research (PCR) with the purpose of 'doing research and making chemicals'! In choosing a name, they first tried combining their own names in some manner, but neither 'Tarbut' nor 'Buttar' sounded quite right, nor did 'Pine-Tree Chemicals', which was another candidate. Once they started the company, the most important thing they did was put an advertisement in C and E News. In addition to allowing them to sell some trifluoroacetone, their advertisement quickly led to a contract with American Viscose to make spinning machine lubricants. This was followed by two research contracts from the Air Force, the first to make antioxidants, and the second to synthesize fluoroalkylsilanes.

The first location of PCR was a shell of a building on NW 5th Avenue and 10th Street, near the water tower. Paul and George spent nights and weekends for 3–4 months building wooden benches, putting plumbing in etc. In those days, Paul and George ran the company by the seats of their pants, quoting to do research and make chemicals without really knowing whether they were making money or not. In 1957 they found a way to make $CF_3CH_2CH_2SiCl_2CH_3$, a precursor to a novel siloxane elastomer that had good low-temperature properties. Dow Corning was interested in the process and wanted to buy the patent rights. Their first offer was $25,000. George and Paul said that they would think about it, left the meeting, went to their hotel room, and burst out laughing : with assets of only $8000, Dow Corning could have had the whole company for considerably less than $25K! As it was, George and Paul took $30,000 for the patent rights, and this money allowed them to buy land and start construction, in 1958, of a modern facility at the current site of the company — the Airport Industrial Park. In addition to a small office building, they built many open-air structures, for safety reasons.

PCR grew quickly and prospered to such an extent that during the summer of 1960 Air Products came down to talk about the purchase of the company for $400,000! Paul was working in California that summer with Charlie Haber at the Naval Rocket Laboratory when he received word from Lee Gordon, then President of PCR, that the deal had fallen through.

At that time PCR was heavily involved in the preparation of high-energy propellants (NF compounds) for Allegeny Ballistics, ARPA and the Air Force. Also, George had discovered an allyl compound that proved to be a good flocculating agent, and which PCR developed and patented. This agent, called 'CATFLOC' was also of interest to Calgon, a water treatment company. In fact, Calgon had already built a plant to manufacture the material when it found out that PCR, 'some small company in Florida', held a Canadian patent on the process it was preparing to use. Thus Calgon offered to buy PCR, and the negotiations which followed led to the sale of PCR to Calgon in 1966 for ~40,000 shares in that company. Interestingly, the share price rose from $35 to $65 during the course of the negotiations, which lasted for 6 months. The number of shares was determined on the golf course, with Paul having to shoot an eight on the last hole so as not to offend the Calgon representative.

After the Calgon purchase, George and Paul became 'consultants' to the company and so remained actively involved in the goings on. The Calgon executive in charge of PCR was Ralph Thompson, who, having seen a 1968 Life Magazine article in which 5-fluorouracil (5-FU) was being touted as a potential anti-cancer agent, challenged PCR to devise a good preparation of the drug. This Paul did, and as a result 5FU made millions for the company. Paul's method was quite simple: direct fluorination of uracil in water. To show how hit-and-miss Ralph was when using Life Magazine as a source of ideas, he later read that pest deer could be scared away by the odour of tiger dung and asked PCR to determine the identity of the 'active ingredient'. However, some halfhearted research did not discover the magic repellent.

Merck soon became interested in Calgon — not because of 5-FU, but because Calgon had acquired Pittsburg Activated Carbon Company, which made the charcoal-activated-carbon in cigarette filters. Merck acquired Calgon in 1968. George and Paul then became consultants for Merck, but Merck had not the slightest interested in PCR. In effect, they sat on the company, squelching business; there were to be no more government contracts or any business done with government agencies. Finally, in 1970, six junior executives at PCR decided they'd had enough of this backward movement and, with the backing of Dow, they purchased PCR

back from Merck. These entrepreneurs were W. Arnold Dinkens, Gene Stump, Paul Shuman, Dale Warner, John Cochran and Max Petzold.

The 5-FU patent was finally issued in 1976 [8], and in 1977 PCR Puerto Rico was established to facilitate the compound's manufacture. PCR was acquired by the SCM Corporation in 1978, and, as time went by, Paul and George became increasingly separated from the company's activities. In 1986, SCM was acquired by Hanson plc, and just 1 year later PCR was re-acquired by Management, the Demetree family and Reichhold Chemical. Shortly afterwards, Hydrozo of Lincoln, Nebraska, acquired PCR and established vinylsilane manufacturing facilities. In 1994, PCR merged with Thoro (an ICI Americas, Inc., company), establishing Harris Speciality Chemicals, Inc., but was soon (1997) acquired by Lancaster Synthesis Ltd. (owned by British Tar) — a major catalogue company sited in NW England.

7. Family life and retirement

During those hectic years of the late 50s and 60s, Paul's personal life had its ups and downs. The family grew with the arrival of Paula and Sandy in 1953 and 1957, respectively, but Viola became ill in the 60s and eventually died of cancer in early 1971. Paul's grief was overwhelming, but the following year he was fortunate to meet, fall in love with, and marry Marian Christie, a widow from Chicago with two teenage daughters. Two years later they built their home at 2211 NW 26th Terrace, Gainesville, where they lived happily together until Marian's untimely death in 1997.

After retirement from UF, Paul continued his consulting not only with PCR, but also with Geigy in Tarrytown, NY (15 years); and during the 1980s he made a number of trips to Japan with Marian to consult for Daikin Industries. Nowadays, he continues to travel frequently with family and friends to exotic destinations around the world. He enjoys fishing on the Gulf with Gene Stump, keeping up his lake place, and tinkering with his computer (tarrant@chem.ufl.edu), but most of all being with family and friends. Paul especially enjoys the frequent visits made by his former students and postdoctoral fellows; and every 2 years, on the weekend following the ACS Winter Fluorine Conference, he holds his traditional post-Conference party at his home in Gainesville.

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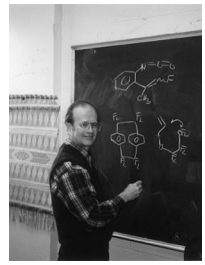

William Dolbier was born in New Jersey in 1939, and moved with his family to Haines City, Florida, in 1955, where he finished high school in 1957. He entered Stetson University as an engineering student, but his first experience with organic chemistry caused him to become a chemistry major, and he graduated with a B.S. degree in Chemistry in 1961. He immediately entered the Ph.D Programme at Cornell and joined the research group of Mel Goldstein, which set his future course as a physical organic chemist. Graduating with his Ph.D in 1965, he spent one and a half years expanding his physical organic horizons as a postdoc with Bill Doering at Yale University before accepting an Assistant Professor position at the University of Florida in the fall of 1966.

At UF he plied his trade as a physical organic chemist, using various kinetic and isotopic labelling techniques to study the mechanisms of cycloadditions and thermal rearrangements. Having an office adjacent to that of Paul Tarrant, he was inevitably exposed to the novel properties and reactivities of organofluorine compounds, and eventually he could resist the call no longer and began to apply his physical organic tools in the study of fluoro-organic reactivity. His initial ventures included work on the kinetic impact of fluorine substituents on cycloadditions of allenes and thermal rearrangements of cyclopropanes, which proved so exciting and productive that he never looked back and has devoted his talents almost exclusively to the field of fluorine chemistry ever since. In recent years, his primary research interest has been to determine the quantitative impact of fluorine substitution on radical reactivity. He also has been very active recently in the devlopment of commercial synthetic processes for the preparation of fluorinated [2.2]paracyclophanes for use by the semiconductor industry; this has led to a number of patents.

Bill moved through the ranks at the University of Florida, becoming Professor in 1975 and serving as Chairman of the Department from 1982 to 1987. He was an A.P. Sloan Fellow and a John Simon Guggenheim Fellow. Within the ACS Division of Fluorine Chemistry, he has served on the Executive Committee and as Chairman of the Division, and he acted as organizer of the 11th Winter Fluorine Conference. As of 1998, he'd published more than 175 papers, most of which involved synthetic and physical studies on fluoro-organic compounds. Currently he maintains a very active research group consisting of five Ph.D students and five postdoctoral fellows.

In 1995 as a result of a perceived gap in the market, Bill, along with Dr. Xiao Rong, established SynQuest Laboratories — small custom synthesis and contract research laboratory devoted to the preparation of small quantities of fluoro-organic speciality chemicals. Shortly thereafter, PCR put its catalogue business up for sale, with the apparent intention of getting out of the research chemicals catalogue business. In response to this, Dr. Rick DuBoisson, who at the time was Research Chemicals Business Manager at PCR, moved to SynQuest with the purpose of establishing a catalogue arm. SynQuest's first catalogue, containing more than 600 listings, was issued in the summer of 1996. Having now established international distributor relationships with Apollo Scientific Ltd. in England, and Hydrus Chemical, Inc., in Japan, SynQuest continues to expand. A second catalogue with more than 1600 listings was distributed in 1999 and can be accessed at www.synquestlabs.com. Meanwhile, neither Bill nor Xiao Rong are any longer associated with SynQuest.