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My Autobiography: From High School Dropout to Global Scientist

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The Shadow of the Stutter

I was born on November 15, 1946, in Santa Ana, California, while my family was living in nearby San Juan Capistrano. My father worked as a telegraph operator for the Santa Fe Railroad—a career he maintained for his entire working life, eventually retiring as a freight station manager. My earliest memory of Capistrano has nothing to do with trains; instead, I remember climbing the fruit trees near our house before we relocated to Norwalk, near Los Angeles.

My development was completely typical until I was nearly five years old. Then, almost overnight, I began to stutter. Within two days, I couldn't utter two words without a severe struggle. I distinctly remember my first desperate attempt to cope: I would stand a few inches from a wall and throw my back against it, sliding down into a seated position. The physical shock seemed to give me the momentary momentum needed to force out one or two extra words.

This speech impediment completely disrupted my ability to communicate with everyone around me—friends, family, and teachers alike. At the suggestion of a child psychologist, my father, who was a strict disciplinarian, stopped using spankings as a punishment for me and my brother, Vance. I still remember the day he announced that decision. While the stutter remained, the constant fear of punishment thankfully disappeared. Still, the severe stuttering persisted with agonizing force until I was sixteen, and it didn't fully fade until I was twenty-eight.

The devastating social impact of stuttering

When the stuttering first took hold, my parents and the school did everything they could to find a solution. I was referred to a child psychologist, and I clearly recall attending a few sessions with him. He suspected my speech issues might have been triggered by a sudden trauma. Because I was terrified of being spanked for misbehaving, he believed home environment stress played a role, which prompted my father to alter his approach to discipline. Trauma caused by nearly fatal kidney failure shortly before the onset of stuttering may also have contributed.

Only recently have I looked online to compare my childhood experiences with those of others who stutter, and I recognize many of the exact same patterns. We are frequently rejected by our peers, targeted by bullies, and left feeling constantly embarrassed. During those years, I had only one friend: Danny, a Mexican American boy who lived nearby. Our friendship began before the stutter took over and endured long after. Outside of Danny, I didn't make a single new friend for more than six years. I was simply too terrified to speak to anyone. I never spoke up in the classroom, nor did I ever talk to my teachers in private. Paralyzed by anxiety, I tuned out during lessons and failed every single year from first to sixth grade. That was the true cost of my stutter.

While many people I know can easily list the names of their grammar schoolteachers, I can't remember even one of mine. Even today, in 2026, science still lacks a definitive understanding of why some children suddenly develop a stutter while others do not, or why it occasionally vanishes later in life, just as it did for me.

A System in Crisis

The stutter completely derailed both my social integration and my basic education. Paralyzed by the fear of being mocked, I stopped participating entirely, which led to me failing every grade. Yet, the California school system—overwhelmed by a doubling student population between 1950 and 1960—simply pushed me through to the next grade anyway. The schools lacked the resources to hold students back or to provide the specialized remedial support I so desperately needed. In hindsight, I have often wondered whether I may have also been on the autism spectrum. While my language comprehension was perfectly intact—I could spend hours completely absorbed in the scientific demonstrations on *Mr. Wizard* and other television programs—I actively avoided my peers to escape their cruelty. I preferred the company of plants, perhaps because they offered life and beauty without judgment. By the time I reached the seventh grade, my reading and writing skills were at a third-grade level, and my math skills were even further behind. My guidance counselor looked at my academic record and bluntly told me that I could leave school at sixteen to look for manual labor.

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The Turning Point: The Sunset Garden Book

My transformation began unexpectedly with a seventh-grade book report assignment. We were required to read a book and write a one-page summary on it. In complete despair, I went to the school librarian and begged for anything that had pictures. She handed me *The Sunset Garden Book*. As I studied the photographs and struggled through the captions, I stumbled upon the concept of plant breeding for the very first time. That book report earned me a C-, which was the highest grade I had received in six years.

Ignited by this sudden passion, I went to the city library and asked for more books on plant breeding, specifically requesting ones with plenty of illustrations. The librarian led me deep into the back stacks to an eight-volume collection written by Luther Burbank. By slowly and systematically working my way through those volumes, I essentially taught myself how to read. When I later went back to my school counselor to ask how I could become a plant breeder like Burbank, she replied, "Well, Mel, you would have to go to college, and that is going to be very hard for you."

From that day forward, I resolved to pay absolute attention in class. Climbing out of a six-year hole of academic failure was incredibly steep, but the fight had finally begun.

High School

At Chaffey High School, students were strictly tracked into three academic tiers for core subjects: dummy, average, and advanced. Despite my newfound focus in junior high, my transcript was still barely maintaining a C average, so I was automatically placed into the dummy level for all my freshman classes. However, before the first term was even over, my performance got me transferred into the average track. By the end of the second term, I had moved into advanced classes. By the time I graduated, I held the second highest GPA in a graduating class of 1,000 students.

Along the way, my academic interests naturally expanded from localized plant breeding to broader botany, and then into the hard physical sciences as I discovered a strong aptitude for mathematics, chemistry, and physics. I began to worry about whether I would have to choose between plant science and physical science. Fortunately, I came of age during the Sputnik era, a time when the U.S. government was heavily investing in inspiring students toward STEM fields by funding summer educational programs at major universities.

I enrolled in one of these National Science Foundation (NSF) programs at Arizona State University in Tempe. There, I took formal courses in plant physiology and spent afternoons doing hands-on laboratory work in taxonomy. The lectures absolutely electrified me, particularly when learning about bio-electricity in vegetation and the potential role of electrical forces—like electro-osmosis—in moving water and sugar through plant tissues. It was a massive revelation. I realized I didn't have to choose between biology and physics after all; I could study plants while using the laws of physics to understand exactly how they move mass, transport energy, and interact with the world around them.

Pomona College Changed My Life, 1964–68

In 1963, Pomona College launched an NIH-funded program designed to encourage undergraduates to pursue careers in molecular biology and biophysics. The program lasted eight years, admitting 20 students annually. During the first summer, before our freshman year, we spent two weeks taking a crash course in statistics and learning how to use the library to research scientific literature. We were then expected to write three review papers, one every two weeks. In each two-week cycle, we were stimulated by two days of lectures from senior regional scientists describing their research. Our papers were reviewed one-on-one by both a science professor and an English professor. In later summers, we were encouraged to find a lab anywhere in the world to collaborate on research. The NIH paid our travel costs and salary for three summers. In addition, because I received an undergraduate research grant from Sigma Xi, I was able to buy equipment and conduct part-time research for course credit during my senior year.

During my first two summers, I traveled to a small university in New Brunswick, Canada—Mount Allison University—to work with Professor David Fensom. He was the only person in North America I could find who was studying electro-osmosis in plants, the strange phenomenon I had first encountered in Arizona State. I was very fortunate to work with David. He appreciated my abilities and paid me a Canadian salary on top of my NIH stipend, even though I told him the NIH did not require any matching supplement. He also made sure I could attend meetings of the Canadian Society of Plant Physiologists (CSPP) to present results from his lab.

At my second CSPP meeting, the famous Harvard tree physiologist Martin Zimmermann attended my talk and later spoke with David about inviting me to work in his lab for my final NIH summer. David strongly encouraged me to accept. During the summer of 1967, I began work at the Harvard Forest laboratory, where I was paid double salary. I returned to Harvard the next summer, right after graduating from Pomona, and earned an even higher salary.

One of the greatest favors Martin Zimmermann did for me was inviting me to contribute a chapter to his famous book, *Trees: Structure and Function*, by M.H. Zimmermann and C.L. Brown, with a chapter by M.T. Tyree. Whenever I visited scientists in Europe over the next few years, that book was often on their shelves. They would pull it down and say, "I know you. I thought you would be much older." I would reply, "I shaved off my gray beard just a few days ago." The book brought together what was then known about the circulatory system of trees. Zimmermann covered physiology, Brown explained the structure of the circulatory system, and my chapter showed how irreversible thermodynamics could help us better understand it. As an interesting historical note, William

Harvey published the first full book on the circulatory system of the human body back in 1628, yet the comparable system in plants was not fully understood until some 300 years later.

At Pomona, I majored in physical chemistry with the intention of applying physics and physical chemistry to understand how plants transport the materials needed for life at every scale—from membranes to cells to the whole plant. My three summers of research, together with the Sigma Xi grant, greatly expanded my knowledge. I collected enough data to publish three papers, two of them as sole author. That record helped me win a Fulbright to work with Jack Dainty in the United Kingdom.

I first met Jack at UCLA, where he was spending a sabbatical year away from the UK. He happened to be the reviewer for a paper I had submitted to the *Canadian Journal of Botany* on electro-osmosis as explained through the thermodynamics of irreversible processes. He remarked that I showed a better understanding of thermodynamics than many postdocs in his own institute. On the strength of that assessment, he offered me early admission to his Department of Biophysics at the University of East Anglia (UEA) in Norwich, UK.

Jack's early admission was a crucial step toward receiving early notification of my Fulbright award. The Fulbright program granted early awards to only about 5 percent of applicants, but first I had to get past an interview committee at Pomona College. The committee included professors from science, history, the arts, philosophy, and other fields. What were my chances? On paper, not good. I had devoted so much attention to research that I hadn't worked as hard on my regular coursework. My GPA was around 3.1 rather than the more typical 4.0, and my graduate school entrance exam score was in the lower 40 percent.

What I did have, however, was an unusual record of independent scientific publication. During the interview, I spoke about my research and deeply impressed the science professors. Then the history, philosophy, and literature professors began asking questions. To the philosophy professor, I mentioned that I had taken an independent study course at Pomona on the philosophical development of the concept of causation, and on how the Heisenberg uncertainty principle seemed to challenge it. My term paper covered the famous debate between Einstein and Heisenberg over those ideas. To the historians, I explained that I had loved reading ever since high school, starting with American history, moving to British history, the history of science, and then novels. The literature professor asked me for some titles, so I named several major works and then casually added, "Oh, and last summer I read *The Vicar of Wakefield*." I didn't mention that I had picked it up for free at a junk sale in New Brunswick. Later, I learned that the Pomona committee nominated me for their single early Fulbright acceptance slot. They selected me because the science professors were impressed, and because the literature professor noted, "*The Vicar of Wakefield* is an important minor classic, but not even my literature majors have read it.".

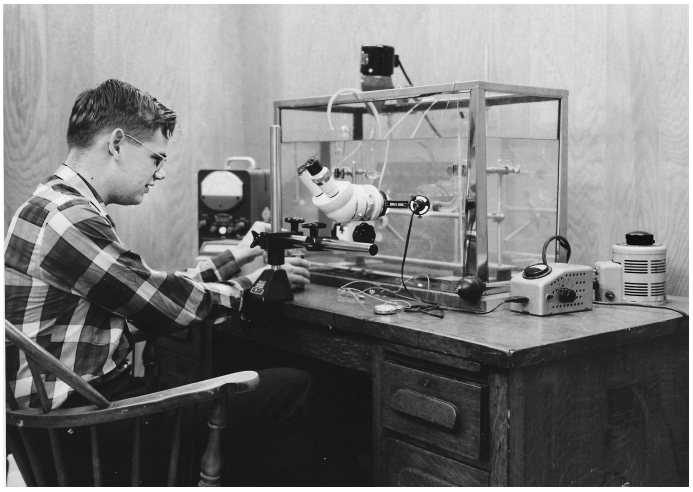


Figure 1. Mel Tyree performing electro-osmosis measurements in 1967.

Off to the UK

I joined Jack's lab in the Department of Biophysics at the University of East Anglia, where he was department chair. Although I began the two-year MSc program, I left after just one year. Early on, Jack told me that he planned to move permanently to UCLA. He invited me to come with him, but that would have meant starting over in a new MSc program with more coursework, and it also raised tricky Vietnam draft consequences.

Still, several good things came from Jack's decision to move. First, he suggested that I stop taking courses and focus entirely on publishing my earlier research, and he encouraged me to continue a new literature-review and theory project that I had begun in my spare time. Second, he nominated me to replace him on a Scripps Institution of Oceanography project in the summer of 1969. Third, he nominated me for a full scholarship to pursue a PhD at Cambridge University in Professor Enid MacRobbie's lab. Instead of taking classes at UEA, I spent my time writing two papers and one review article about the biophysics of solute flow through the plasmodesmata connecting adjacent plant cells (Tyree, 1970). That review was frequently cited and founded a major theme of research that I followed for many years from 1972 to 1978.

The Vietnam War and the Draft

Starting in 1968, the draft became a massive problem for me and all my university friends. Deferments were no longer being given to most graduate students, except those in medical school. Medical students received draft deferments until graduation but then had to serve in the armed services medical corps—which was less hazardous than carrying a gun on the front lines, but still dangerous. In 1968, most local draft boards were denying permission for men to go abroad to study, fearing that if students were overseas when drafted, they would evade service.

In my case, I applied to the University of East Anglia, Harvard University's Department of Biophysics, and two other places in the United States. I received an admission letter printed on Harvard letterhead that read "Department

of Medicine, Harvard University" at the top, along with my notice of a Fulbright award and my admission to UEA. I wrote to my local draft board and asked for permission to go to the UK to use my Fulbright to study at UEA, which was my first choice. However, I strategically noted that if they denied me permission, I would accept my admission to Harvard instead, enclosing all the documents as proof. A week later, I received my 2-S student deferment. Technically, I wasn't entitled to a medical deferment because I hadn't been admitted to Harvard to study medicine, but the admission letter's header was neutral as to the specific reason for my admission. My draft board made an understandable mistake, and I certainly didn't point it out. This deferment had to be renewed every year through my university. I never had to do a thing; for some reason, it was renewed year after year. I never asked why I was still considered eligible after moving to Cambridge, since I was clearly not studying medicine.

In contrast, my Pomona friends who went to Harvard to study molecular biology spent huge amounts of time trying to fail their physical exams so they would be declared ineligible for the draft. They researched what medications might induce heart arrhythmia or other serious temporary medical conditions that would cause them to fail the examination. This would give them a two- or three-month reprieve before being called up for another physical. One way or another, many managed to escape the draft legally.

A Productive Summer of 1969

The Scripps Institution trip was great fun. I joined a research vessel usually used to support Antarctic work, but during the winter, the *RV Hero* was made available for research farther north. I flew to Santiago, Chile, to board the boat for a nine-week trip along the southwest coast of Chile, all the way down to Tierra del Fuego.

The purpose of the expedition was to study freezing and thawing in evergreen beech trees (*Nothofagus betuloides*). The core question was how the leaves remained alive after repeated freeze-thaw cycles. The hypothesis was that the living cells did not freeze, but that water in the xylem—the woody conducting tissue—did freeze and thaw as the temperature fluctuated. The amount of freezing water could be measured with a bomb calorimeter by determining the latent heat released during freezing (about 80 calories per gram of water). By measuring the mass of water in a stem, the temperature, the heat released, and the rate of release, one could infer both the quantity and location of the water that had frozen. I was asked to develop the theory and mathematical equations needed to interpret the collected data.

It turned out that I completed the freezing-and-thawing model in about two weeks, so I asked for permission to help collect data to test the model directly. The answer was, "No, we already have all the experimental help we need. But we have another instrument you can work with. I call it the pressure bomb." The speaker was Ted Hammel. Hearing him speak, I instantly remembered the lectures given to us in the summer of 1964 by Pete Scholander from Scripps. Ted had actually invented the pressure bomb and understood exactly what it would mean for modern plant physiology, even though he was a quiet, reserved duck physiologist serving as a postdoc with Scholander. Once Scholander grasped its significance, he traveled around the world lecturing about it and, at least in the eyes of many listeners, received most of the credit for the device. At Pomona, Scholander had given one lecture about the pressure bomb and others on his work in animal and human physiology.

Ted wanted me to develop a model to describe the equilibrium balance pressure, as a function of (the water volume pushed out of a shoot by the pressure bomb). He wanted a quantitative model showing what this relationship meant, given that the millions of cells in a leaf can differ in both mechanical properties and solute concentration. Although many papers had already been published about the pressure bomb, no one had yet worked out the theory in a rigorous way. Ted asked me to try.

Two weeks later, I had a model that provided the theoretical answer. During the final five weeks of the trip, I carried out experiments with the pressure bomb so that Ted and I could publish a paper together. Ted generously suggested that I publish it as sole author, but I insisted on including his name. I drafted the paper after arriving in Cambridge. As it turned out, this was the only paper I got from the *RV Hero* adventure. The freezing-and-thawing work on beech was never published, so I was incredibly fortunate to have been redirected. By contrast, the pressure-bomb paper became one of the most highly cited publications (Tyree and Hammel, 1972) of my career and inspired many more years of research after I left Cambridge.

What Did I Do at Cambridge?

In one sense, I did quite a lot at Cambridge, because I wrote and submitted papers leftover from my Harvard summers and the *RV Hero* adventure. From 1968 onward, I wrote six papers, one book chapter, and one review article before even starting my Cambridge dissertation research. However, I could not count any of that prior work toward my dissertation.

Nevertheless, Enid MacRobbie and I both recognized that it was vital to get those papers out. Enid gave me an office on the top floor of the Department of Botany so that I could write without interruption. She also did me a massive favor by arranging for me to receive two scholarships—one from Churchill College and one from the University. This gave me enough income to cover tuition, room and board, and even enough to get married and support a wife.

During my first year, while I was writing those papers, I developed a research topic, set up a lab, and began some preliminary experiments for my Cambridge dissertation, though I didn't work on that specific research full-time until the second year. The research concerned ion exchange and the transport properties of plant cell walls. Progress was slow, and the results weren't especially

exciting, but they were adequate for a PhD after three years of enrollment.

Fortunately for me, I was also beginning to receive unsolicited offers for permanent jobs at a time when such positions were becoming incredibly scarce. Both offers were for tenure-stream positions that would begin in 1971, a full year before I was scheduled to submit my dissertation. However, Cambridge University rules allowed me to leave and finish writing my dissertation after only two years of physical residency, so I was able to take the offers seriously.

The first offer came from the Department of Chemistry at the University of Aberdeen for a position in biophysical chemistry. That seemed like a reasonable fit, and the chairman mentioned they were hiring two biophysical chemists: one to work on plants and one on animals. In addition, Professor Weatherley in the Botany Department at Aberdeen told me that I could easily transfer over to his department later if I didn't like the situation in Chemistry.

The second offer came from Jack Dainty, who was moving again—this time to become Chair of the Botany Department at the University of Toronto. I chose Dainty because I could see many advantages, not least of which was the intellectual environment he could provide. He also offered to turn over to me a research grant he had been awarded for his own use, expecting to be far too busy running the department during his first few years.

So, I began my career at the University of Toronto's Department of Botany, where Jack had moved that same year to become chairman. We were both in the unusual position of being the first people ever hired by that department without ever having taken a formal course in botany, ecology, or plant physiology. I started there in August 1971. By July 1977, I would become the youngest full professor of science ever appointed at the University of Toronto.

My Years at the University of Toronto

The Botany Department at the University of Toronto was primarily a graduate department. In any given year, it had more registered MSc and PhD students than BSc undergraduates, making it an exceptional environment for nearly full-time research.

Unfortunately, I ran into serious problems with my California draft board in 1972. I was called in for a physical examination as a prelude to a possible induction into the army. I deeply opposed the war, as did millions of draft-age young adults and tens of millions of older Americans who had grown entirely fed up with it. By then, I held permanent residency in Canada and a tenure-stream job, meaning I could have quietly broken the law, avoided the draft, and kept my citizenship. But I felt a deep sense of hypocrisy doing that. My view was that if I was unwilling to obey U.S. law regarding the military draft, I shouldn't feel entitled to keep the privileges of U.S. citizenship.

So, I walked into the U.S. consulate in Toronto, explained my reasoning, and formally renounced my citizenship. The consular officials bluntly told me that this act would not change my legal obligations under the draft law. I told them I understood that completely, but re-emphasized why I could not break the law and still feel right about remaining an American citizen—even though I knew that perhaps half a million young Americans in Canada had done exactly that: evaded the draft while retaining their passports.

This high-principled move naturally created an immediate practical problem: how was I supposed to return to the UK to defend my Cambridge dissertation without a passport, now that I was completely stateless? Naturally, I researched the question thoroughly before taking such a rash step. I discovered that individuals born in the United States could theoretically travel from Canada to the UK using only a birth certificate combined with official proof of their right to return to Canada (landed immigrant status). It would require a mountain of paperwork, but I figured I would be fine. I was fully prepared to remain confined to Canada for four more years, if necessary, until I became eligible to apply for Canadian citizenship.

Canada did technically offer Certificates of Identity for stateless individuals to ease international travel. However, when I inquired in Ottawa about obtaining one, I was told in no uncertain terms that, as a presumed "draft dodger," I would not be allowed to apply.

After all this immense stress, the Vietnam War was effectively called off before I was ever actually drafted into the army. Even though I had refused to appear for my physical examination, the U.S. government never prosecuted me. I completed my thesis and submitted it to Cambridge, accompanied by a detailed account of how and why I had become a stateless person, and why I should still be permitted to enter the UK for my viva defense. I asked them to send me an official letter with the scheduled date, which I intended to take to the British consulate in Toronto to make final arrangements, telling Cambridge I was about 90 percent sure my travel would be authorized. They wrote back immediately and told me mine was the most exciting case they had dealt with in years. The entire department administration had apparently been running around trying to figure out what to do, thoroughly enjoying the bureaucratic puzzle. Their final decision was remarkable: rather than forcing me to navigate international border security, they insisted on sending the Cambridge examination committee directly to Toronto. I could hardly believe my luck.

Immediately after I passed my dissertation defense, the University of Toronto promoted me from Lecturer to Assistant Professor, with a pay raise effective November 1972. Utilizing the research funding Jack Dainty had turned over to me, I was able to publish four to six papers a year. By July 1975, I had risen to tenured Associate Professor.

The following year, I was nominated for Full Professor, but the nomination was summarily rejected by the Dean—even though the sheer volume and citation impact of my publications easily matched those of older scientists who had advanced successfully. The year after that, my new department chairman tried again, this time documenting

my case much more meticulously and gathering powerful testimonials from international experts. In July 1977, I officially became the youngest full professor of science ever appointed at the University of Toronto.

The major areas of research and key publications that resulted in my rapid promotion were:

- **Short-distance transport:** Studies on fluid flow through the plasmodesmata connecting individual plant cells (Tyree, 1970; Tyree and Tammes, 1975).
- **Pressure bomb mechanics:** Refining pressure bomb applications in broad water relations studies (Cheung et al., 1975; Tyree and Hammel, 1972).
- **Phloem translocation modeling:** Theoretical mathematical models evaluating three competing mechanisms for sugar transport: electro-osmosis, the Canny-Phillips model (Tyree, 1975), and the Münch Pressure-Flow model (Tyree *et al.,* 1974).

Through rigorous physics, I was able to prove that the first two models simply could not account for the massive physical magnitude of sugar mass-flow required over long distances, whereas the Münch mechanism was mathematically sound. Because of a chronic lack of institutional funding for physical lab setups at the time, about half of my total research output during these years consisted of these pure mathematical models.

Partway through this promotion process, I managed to begin traveling internationally again. Despite telling me a year earlier that they would never grant me one, Ottawa suddenly issued me a Certificate of Identity in August 1972 after I requested proof of my landed immigrant status to defend my thesis. This document made it possible for me to attend key scientific conferences in the Netherlands, Germany, and Greece. My stateless ID card always raised eyebrows and led to intense questioning at consulates and border checkpoints, but once I told my story, I was always met with deep empathy from border and consulate agents. I finally received my official Canadian citizenship and passport on January 5, 1977.

Under university rules, I became eligible for a year-long research sabbatical anytime after August 1977. I chose the University of Messina in Sicily, a perfect location because it allowed my wife to simultaneously pursue her passions in classical archaeological studies.

My One-Year Sabbatical Turns into Three

In Messina, I collaborated closely with Sebastiano Salleo on plant water relations, and I used the opportunity to invite key collaborators from France and the UK to visit, which completely reshaped the future trajectory of my research. Most notable among them were Pierre Cruiziat and John Milburn. Cruiziat connected me deeply to the French plant physiology network, while Milburn introduced me to the fascinating concept of listening to the acoustic sounds made by plants under severe water stress. Their collective intellectual influence was pivotal to the major transition from my earlier theoretical work to what I would soon build back in Canada.

While in Italy, I applied for a prestigious Killam Research Fellowship from Canada. The award was highly competitive, open to all fields of science and engineering, and ultimately paid my full salary for two extra years, allowing me to conduct independent research anywhere I wished. However, Italian customs laws mandated that I either remove my imported scientific equipment after two years or pay astronomical import taxes on it.

I returned to Toronto with a burning desire to transition my research out of the lab and directly onto trees in a natural forest environment. Unfortunately, doing forestry research is incredibly difficult when your Botany Department is trapped in the center of a massive metropolis, located right across the street from the Provincial Capital building. I had to figure out a creative way around this limitation.



Figure 2. Performing experiments in Clermont-Ferrand (France) with Pierre Cruiziat and Christian Bodet in 1991.

The Snake Island Institute

Then, I had a completely crazy idea. I spotted a classified ad for a summer cottage for sale on Snake Island in Lake Simcoe. Snake Island is part of the Chippewa Indian reserve; the cottages have absolutely no road access, and the island is only reachable by a seasonal ferry service. Because cottage owners simply lease the ground plots from the reserve, the properties were incredibly inexpensive. Best of all, the center of the island was entirely untouched forest, and the island was practically abandoned for eight months out of the year.

I considered buying a cottage, expanding onto it a research laboratory, and conducting field research on the vast array of trees standing right beyond the backyard. After securing formal permission from both the Chippewa caretaker and my department chairman, I invested my own personal savings to buy the property and built a functional lab on the site. I lived and worked on the island for over four years. A brilliant postdoc, Mike Dixon, lived and worked right alongside me on the island, many students spent their summers assisting us, and graduate students completed their entire PhD dissertations at what we affectionately called "The Snake Island Institute."

During this era, I initiated four major areas of research that would continue to define my career for decades:

1. **Hydraulic Architecture:** I began mapping the hydraulic architecture of large trees, systematically measuring the precise hydraulic resistance from the main trunks down to the branches, twigs, and leaves, and relating those physical values to internal plant anatomy (Tyree, 1983; Tyree and Ewers, 1991).
2. **Ultrasonic Acoustic Emissions (UAE):** I designed and fabricated a custom Ultrasonic Acoustic Emission detection system to monitor cavitation in water-stressed vessels of plants. I loved the audio-frequency system John Milburn had shown me in Sicily, but his design required a completely soundproofed room and manual listening to count events. I attended an international engineering conference where I learned that UAE technology was being utilized to detect structural metal fatigue in the wings of fighter jets while in flight, and I decided that was the exact methodology I needed. Because the commercial equipment was far too expensive to purchase on my budget, I rented a setup for two months to confirm it worked on plant tissue. Then, using electronics as a personal hobby, I built my own custom system from scratch, complete with UAE amplifiers, event counters, built-in system memory, and a serial interface hooked up to an S-100 desktop computer. All I had to buy were the specialized UAE sensors. As I began publishing our findings (Tyree and Dixon, 1986, 1983), I built a few extra systems for global colleagues so they could access the technology. Interestingly, John Milburn never truly believed our ultrasonic data and declined to ever use a UAE system himself.
3. **The Mystery of Maple Sap Exudation:** Inspired by Milburn, we became fascinated by what physically causes maple sap to flow so vigorously in the spring. To study this under controlled conditions, I cut six-foot-long maple stems in the dead of winter and mounted them inside a specialized freezer, running the base of the stems directly through the insulated freezer wall into the warm lab where they were connected to a water source. This allowed us to precisely measure water movement during freezing and thawing cycles while monitoring minuscule temperature differentials inside the wood and the air. Three years of precise experimentation solved the long-standing mystery: in maples, water actively flows *into* the branches as they freeze, and exudes *out* as they thaw—the exact opposite of almost every other tree species. We proved this was due to highly unique anatomical differences in the cellular structure of maple stems compared to other hard woods (Cirelli *et al.*, 2008; Johnson *et al.*, 1987; Tyree, 1983).
4. **Thermocouple Psychrometers:** For years, I wanted to build a specialized thermocouple psychrometer capable of measuring the exact temperature differential between the surface where water evaporates and the surrounding chamber air where water vapor potential is measured. A tiny fraction of a single degree Celsius difference would finally explain why psychrometer readings and pressure bomb values so frequently disagreed. I hired Mike Dixon specifically because he possessed the unique technical skill to build them. Our new temperature-corrected stem hygrometer successfully resolved the long-standing international controversy (Dixon and Tyree, 1984), and thousands of Dixon's psychrometers were subsequently commercialized and sold worldwide.

We published more than 30 papers out of the Snake Island Institute. I tried to use this immense productivity to convince the University of Toronto to provide a permanent, road-accessible forest research facility closer to campus. The Province of Ontario even offered to give the university an existing building on a heavily forested plot that could be easily converted into a satellite lab.

But the university administration's answer was simple, rigid, and clear: "No. We have had a strict policy for decades. No specialized satellite labs are allowed for individual professors. We didn't even grant special facilities to a Nobel Prize winner who requested them.".

The message to me was loud and clear: it was time to look for employment back in the United States. Unsolicited job offers from American institutions were already rolling in. I knew that by making the right choice, I could secure vastly more laboratory space, advanced equipment, and research funding than I could ever hope to receive in Canada, where I had already hit the absolute ceiling of available federal grants.

Politically, the timing was perfect as well. The U.S. government was offering official amnesty to Vietnam-era draft evaders. Technically, I wasn't a draft dodger because I had never actually been inducted or fled, but I did need to secure a green card after having cut legal ties with the country years prior, and I figured the amnesty environment would smooth my transition back home.

The Move to Vermont

To make a long story short, I decided to move to the University of Vermont (UVM) in December 1985 to become a non-teaching Research Professor and Director of the Proctor Maple Research Center (PMRC). The job came without tenure, but UVM told me, "Don't worry. The PMRC has been funded with USDA money for decades, and it never goes away." I was given very good startup support through a special USDA grant, got off to a wonderful start, and hired the best postdoc I ever had: John Sperry from Harvard University. Harvard was actually one of the places that wanted to hire me to work at the Harvard Forest, but their endowment was about to end with no replacement funds in sight, so UVM seemed like the better option.

My research output rose quickly. I had labs on campus and at the PMRC, but I was still dogged by bad luck. A fire at the PMRC destroyed the entire building, along with all my research notes and equipment at that location. I quickly learned that UVM carried no fire insurance on the building itself. The good news, however, was that all the equipment was insured for full replacement, and even old, non-working equipment in storage could be replaced under the

policy. The insurance would also pay salaries to reproduce the lost, unpublished data.

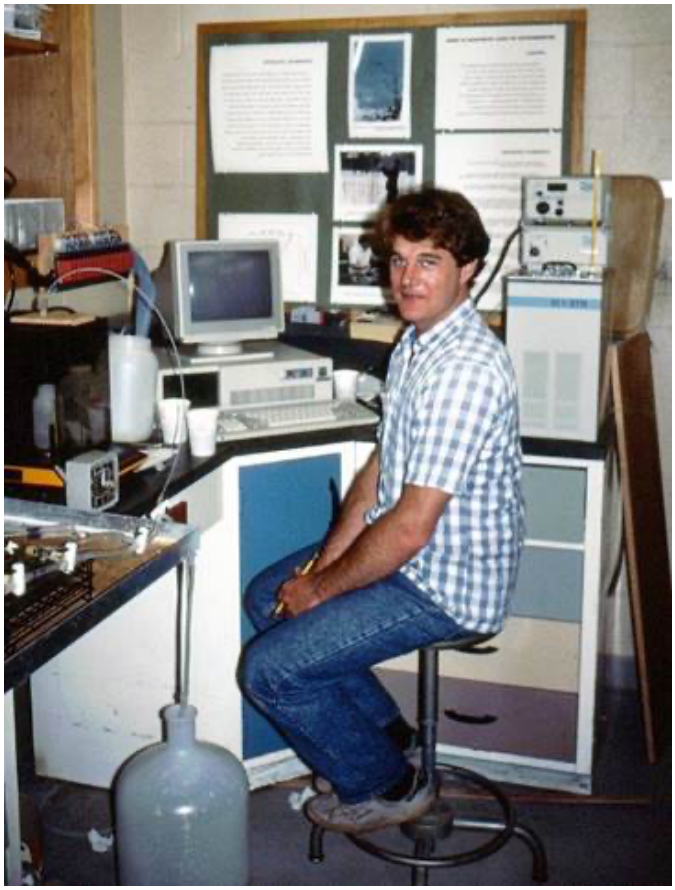


Figure 3. John Sperry at Vermont University (1989).

The best news of all was that maple research was as newsworthy in Vermont as motherhood and apple pie. We received front-page coverage in local newspapers and strong attention from radio stations. I took the lead in making the most of that moment and launched an "adopt-a-maple-tree" fundraiser, which brought in gifts from all over New England. We had schoolchildren in Vermont selling cookies to raise money, and the effort was quickly picked up in neighboring states. Before long, we received participation from schools as far away as New York City, accompanied by a wave of newspaper and radio coverage. Soon, Vermont Senator Patrick Leahy promised to advance a bill in the U.S. Senate to help finance a new building. The University, which had been suffering from bad press for several years, had little choice but to embrace this outpouring of good publicity and support the fundraising campaign. We ended up with a $1 million maple research center—four times larger than the old one—within a year and a half.

A year later, financial issues arose again regarding the security of my salary. Because my position was not tenured, USDA funding was no longer sufficient to guarantee my Research Professor appointment unless I raised part of my salary each year through grants. I went to my chairman seeking a tenured position, but after he consulted the Dean, they told me that would not be possible. At that time, I was being approached by the University of Victoria in British Columbia about an endowed Chair in Forestry with excellent research facilities and no teaching. I decided to look into it and informed the Dean at UVM. When I was officially offered the job at UVic, I informed the Dean again, and he immediately granted me tenure.

So, I stayed at UVM, even though I knew the university had no immediate source of money to pay me. I suggested a temporary solution: a cross-appointment with the U.S. Forest Service, which maintained a lab right on the UVM campus. My role there was to direct an existing research group studying the environmental impacts on forest health. In return, the Forest Service paid 50 percent of my salary while providing more lab space and additional research funds. At about the same time, the Smithsonian Tropical Research Institute—another branch of the U.S. federal government, conducting tropical research in Panama—offered me an unsolicited research grant and lab facilities at its nature preserve on Barro Colorado Island in the Panama Canal Zone. I was excited about that opportunity as well.



Figure 4. Barro Colorado Island research team, 1992.

All the research themes and methods from Snake Island were successfully transferred to Vermont and the tropics, resulting in many interesting papers and models. Two important techniques were added. I perfected a high-pressure flowmeter, which permitted the measurement of the hydraulic conductance of whole leaves, whole shoots, or whole root systems (Tsuda and Tyree, 2000; Tyree *et al.*, 1995); the HPFM was subsequently commercialized for use by other labs worldwide. We also adopted the design of large root pressure chambers from Germany. We fabricated some quite large chambers that could hold large potted plants (either whole or excised root systems), which permitted many important experiments where air-pressure control of root-water potential was vital.

For the next several years, I directed four different labs with a research team of 20 to 28 scientists, postdocs, and students. When I look back on it, I realize I was at the peak of my career, but I was also badly overcommitted. I worked six to seven days a week, ten to fourteen hours a day, trying to keep up with my obligations to advise

people, write grants, publish papers, and attend meetings. I could sustain that pace for only a few years before something had to change.

In April 1991, the U.S. Forest Service made me an irresistible offer: a 40 percent salary increase, a $350,000 annual research budget, and the freedom to work internationally on projects of my own choosing. The offer was part of an experiment at the Northeastern Station to see what could be achieved if top scientists were freed from most restrictive federal rules and allowed to focus entirely on their work. It enabled me to reduce my administrative burdens, escape chronic overwork, and return to what I did best: pure science. Over the years, I have had the pleasure of working in 22 different countries. Many international colleagues had invited me to work in their laboratories, and this new role finally allowed me to accept those invitations. One of them drew me directly into an international controversy over the Cohesion-Tension Theory.

The theory had first been proposed by Henry Dixon around 1898 and was strongly opposed by Francis Darwin, the son of Charles Darwin and a leading figure in British science. The idea that water could be transported under negative pressure is difficult to believe, and Francis and others had alternative explanations that seemed more plausible. But over the decades, as those alternative explanations were systematically eliminated, the Cohesion-Tension theory again came to dominate the classroom. I had previously answered a minor challenge to it at a conference in 1997 (Tyree, 1997).

The controversy subsided until Ulrich Zimmermann turned his cell pressure probe to xylem vessels and reported that he could not measure negative pressures below about -0.3 bars below absolute zero, which was far too small to support the Cohesion-Tension theory. He eventually convinced enough journal editors of his findings that ecologists could no longer cite the theory to explain the ecological relationships that mattered to them. Those ecologists then reached out to colleagues who still believed the theory, and they all agreed that only I had the resources and time to address the problem. Collectively, many of them wrote to ask me to help rebut Ulrich Zimmermann's claims.

I began a collaboration with Ernst Steudle's lab. Ernst had once worked alongside Ulrich, but by then they were bitter enemies and had even, at one point, come to blows in a fistfight on stage at a conference. In my opinion, the pressure probes in Ernst's lab were better designed than Ulrich's. My challenge was to get my hands on both kinds of probes, prove the mechanical difference between them, carry out critical experiments on a chosen species, and prove that a plant could truly develop negative pressure. I chose maize. I had to collaborate with Ernst while also pretending to remain friendly with Ulrich and open to proving him right—no easy trick when both men were sworn enemies.

I worked with Ernst for two years, together with Wei, a refugee Chinese physicist who was older than I and who agreed to work with me in both Vermont and Germany for three years while earning his PhD at UVM. He conducted the precise maize experiments that demonstrated the Cohesion-Tension theory was correct. I also visited both German labs to evaluate the mechanics of the two different probes. When I visited Ulrich's lab, I had to pretend that I was on the verge of accepting his views just to gain access to his equipment for some tests without plants. I discovered that the rubber seals in his probe leaked air at -2 bars below absolute zero. Over Christmas vacation, I borrowed one of his probes without permission, used his poorer quality glass tip in our maize experiments, and found that it leaked in the xylem at -0.2 bars. I returned the probe undetected before New Year's Day.

In the end, we were able to explain exactly why Ulrich could not confirm the Cohesion-Tension theory—his probe leaked air—and we were able to verify the theory with a better-designed pressure probe for the first time in history. Ernst's pressure probes could measure negative pressures down to -10 bars in maize, which perfectly agreed with our pressure bomb measurements on excised leaves.

Our evidence was strong enough to convince journal editors to allow our ecology and physiology colleagues once again to cite the Cohesion-Tension theory in support of eco-physiological interpretations. But the papers by Wei, Tyree, and Steudle (Wei *et al.*, 1999a; 1999b) were still not enough to silence Ulrich Zimmermann. He simply ignored them and continued to publish new papers defending his disproven ideas. Eventually, a group of about forty authors came together to publish what was intended to be a final, definitive rebuttal. The paper was only two pages long with a few essential references. After that, we never heard from Ulrich Zimmermann again on the issue of the Cohesion-Tension theory.

Accomplishment of Recovery

To me, the most interesting part of my life is how I managed to recover from years of failure in school caused by the devastation of stuttering and, perhaps, being on the autism spectrum. Was my stuttering independent of autism, or was it somehow caused by it? I will never know. My stutter finally disappeared when I was twenty-eight, but some of the coping mechanisms I developed remain with me even now. I learned that I could sometimes avoid stuttering over troublesome words by slightly mispronouncing them. At age eighty, I still mispronounce many of those words. Fortunately, people usually assume it is an accent I picked up someplace where I once lived. If they ask, I usually just say yes.

If anyone with autism reads this, please take note: if you are struggling in school, it is entirely possible to improve and do better. You just need a goal strong enough to drive you forward. Anyone who is happy with life is a success; you do not need a college degree to be happy.

Later Accomplishments

Over the course of my career, I received 11 scientific awards and prizes, ranging from minor honors to major international recognition (see list at the end of the document). The prize I valued most was the Marcus Wallenberg Prize, which I received in 2002. It is sometimes called the Nobel Prize in Forestry because it is presented during a formal two-day ceremony featuring the King and Queen of Sweden, with the King personally presenting the award. One especially pleasing aspect of the prize was the nearly $200,000 gift that accompanied it. I asked the Wallenberg Foundation to donate the funds in my name directly to a charitable trust so that I could distribute them to charities of my choice. I arranged it that way because if the money had been given directly to me, the IRS and New York State would have taxed away nearly half of it.



Figure 5. September 2002 at an event with the King and Queen of Sweeden for the Marcus Wallenberg Prize.

Declining Grants and Another Move to Canada

I remained with the Forest Service until retirement age. After I received the Marcus Wallenberg Prize, they raised my appointment to the highest level possible (grade SS00) and gave me another substantial salary increase. But the Forest Service could not control Congress, and Congress kept reducing the research budget. Eventually, I was earning a generous salary while my research program was being supported by increasingly inadequate funds.

A rescue came unexpectedly when the Department of Renewable Resources at the University of Alberta offered me a position with a large lab and a generous research budget. Unfortunately, Canadian salaries did not come close to matching U.S. federal salaries. I emailed my Forest Service director, Michael Rains, and wrote: “I am being paid a large salary but do not have enough research funding to make good use of my time. The University of Alberta will provide me with a lab, a generous research budget, and support staff if the Forest Service will cooperate by continuing to pay my salary and by signing a memorandum of agreement with them.”.

I also pointed out that the research I would do in Canada was essentially the same as the work I would have continued in Vermont. If I stayed in Vermont, however, I would not be able to do that research because I no longer had the funding to support it.

Michael Rains immediately emailed the Washington office to ask for opinions from the International Forestry division and several other divisions. International Forestry strongly supported the idea, but other divisions did not. Michael Rains was still leading the Northeastern division, where we had been experimenting with what could be accomplished if we relaxed the usual Federal rules. I assume he used that spirit of flexibility to justify signing a five-year agreement with the University of Alberta. Three highly cited papers would result from my period in Alberta (Cai and Tyree, 2010; Galvez *et al.*, 2013, 2011).

Only in my final year of the agreement did I discover an unexpected secret: Michael Rains had signed the agreement with the University of Alberta but had never officially filed it with the Forest Service. Instead, he had kept it privately in his home. By the time I learned this, he was already retired. The consequence was that when the agreement came up for renewal, there was no official record of it, and I had to return to work in Burlington, Vermont.

China: Work After Retirement

I spent less than a year back at my old Forest Service office. My new boss told me that I should write four review articles; he even gave me the titles, then hinted that I should retire. There was no mandatory retirement age at the Forest Service, but if I stayed, they could easily have assigned me a very boring job.

As soon as I returned to Vermont, colleagues in China approached me about two possible research positions there. At the time, the Chinese central government was trying to recruit Chinese citizens who had gone abroad for PhDs and become full professors in science, engineering, or mathematics. About 10 percent of the recruiting effort was directed at recently retired Western professionals with a history of collaboration with Chinese scientists. The initiative was called the 1000 Talents Program. They were offering me a higher salary than I was earning in the United States, more research funding and equipment, access to graduate students, postdocs, and staff, and a generous signing bonus paid up front. After further discussions and a review of the contract, I took a month of vacation from my Forest Service job and flew to China to evaluate for myself. I inspected the facilities, signed a contract, received the signing bonus payment, and made sure I could transfer the funds electronically to the United States. At the end of that working vacation, I returned home and retired from my federal job.

Only one publication from China has had time to reach highly cited status (Wang *et al.*, 2014). I do not want to focus here on my research in China, but rather on my impressions of China before I moved there, what it was like to live there, and what I learned about the politics of living inside the country.

Chinese Universities and government control

China went through many political and economic upheavals over many decades. In response to the Student Democratic Movement of 1989 the Communist Party increased control over Chinese universities by placing Party Secretaries alongside every university president and departmental chair. These Party Secretaries were given veto power over decisions at every level. China has roughly 1,000 universities, with an average enrollment of about 30,000 students, and all are funded by the central government in Beijing.

By 2008, there were clear signs of growing prosperity, gradually expanding freedom of speech, and greater openness to the West. I accepted a job in China because I wanted to watch that growth from the inside and see whether I could contribute to it in some small way. In 2011, Western news reports still described widespread graft and embezzlement, but I did not realize how close I would come to those realities until years later.

One offer for research in biophysics was from the Center for Micro and Nano Mechanics (CMNM) at Tsinghua University in cooperation with Nanjing University. They sent me two of their PhD students to co-supervise while I was at U of A. It was fortunate I declined that job because the program was terminated two years later over embezzlement issues.

Five Years of Politics at Northwest A&F University, Yangling, Shaanxi

A&F stands for Agriculture and Forestry; NWAFU is in the top 10% of Chinese universities and was picked by the central government as one they wanted to advance to a higher level. University politics became evident as soon as I arrived in Yangling. I had been recruited by Prof. Zhang; he had done research with me for a few weeks at the U.S. Forest Service and was a mentor of Cai, a teacher of tree genetics in the College of Forestry. He told Cai that she must travel to Alberta to do a PhD under my mentorship. This was not her choice; I later learned it was Zhang's order to Cai as her boss.

At the time I was recruited by Zhang, he oversaw the Office for International Cooperation. Zhang was in favor of my appointment to the Forestry Department, as was the Party Secretary for the University. I did not realize until months later that the Chairman of Forestry was against it, but he could not openly speak out against the Party Secretary. On the first week before I signed a contract, I was shown around the Forestry Department and several other departments and was told I could pick any one of those to work in. I did not know why they were doing that, but Forestry seemed to imply they preferred I choose somewhere else. But I chose Forestry because I had a long-time working relationship with Zhang and Cai. They did not offer working space in their rather old buildings, but instead in a new molecular biology building with no forestry staff.

The molecular biology building was hastily constructed with a leaky roof. They were finding and plugging the leaks as I moved in. My three labs were filled with unopened crates of molecular biology equipment intended for a recently recruited professor who defected at the last minute (perhaps not a good omen).

I was not the first 1000-Talents professor to be recruited to NWAFU. The first was a Chinese climate change professor from the University of Montreal; he was recruited the year before me and given two whole floors in one of the two Forestry Faculty buildings. He did not resign his job in Canada; he worked half-time in China and half-time in Canada. Nevertheless, he was given a large working space and a large staff. He kept his space sealed off from all visitors, even visitors from the Forestry Faculty. Only authorized people could enter! Why? Climate change isn't a state secret… or is it in China? I never bothered to figure it out, but recorded it as another instance of strange politics.

The Communist Party and Student Politics

One of my PhD students happened to be the president of the Communist Party organization for all students on campus. He seemed to think that this status entitled him to special treatment—in effect, a PhD with very little effort. When he realized that I expected him to produce genuine laboratory results, he recruited a senior undergraduate to do research in our lab on his behalf. I welcomed her. She was eager to learn and did very good work. She became a coauthor on one of my papers, and he objected that his name was not included as well. I held firm and waited to see whether there would be consequences. How powerful could he really be? I was prepared to pack my bags if necessary. In the end, nothing happened.

Teaching and Campus Politics

When I signed my contract, I made it clear that I had done virtually no classroom teaching for the previous thirty-five years and had no interest in doing anything other than full-time research and one-on-one teaching in the lab with my own students. The Dean of the Graduate Faculty strongly objected, but I was given a two-year reprieve from classroom teaching. In the third year, I was ordered to teach a graduate-level course based on a book I had published that could serve as the textbook. Professor Cai helped me manage the class and wrote and administered the final exam in Chinese.

Graduate courses at NWAFU typically enrolled 20 to 40 students and were taught in a blitz format: 20 lectures, four per day, over five consecutive days, in either Chinese or English. That was demanding enough in one's native language and nearly impossible in English. Within the first ten minutes, you could see the students' eyes glaze over and comprehension fade. I thought the class was a total waste of my time and of little value to the students.

The following year, when the Dean of Graduate Studies again told me to teach a class, I refused outright. I assumed that would be the end of it, and at first I heard nothing more. Then one day my secretary told me that the students were waiting for me. I told her to go to the classroom and announce that the course had been canceled. Again, I

waited to see what would happen. How powerful could the Dean really be? I was prepared to pack my bags and leave China if necessary. Once again, nothing happened. I was never asked to teach again.

My Support Staff and Politics

The Faculty of Forestry did provide me with MSc and PhD students and assigned Dr. Cai to work with me because of my long-standing connection with her in Canada. But I was given no other faculty support and no postdoctoral researchers. I quickly learned that this was highly unusual. Senior professors were normally given much more support than a single assigned colleague, and Cai spent very little time in my lab. When I asked her about it, she explained that her former colleagues in tree genetics wanted her to work with them and that she wanted to do so as well. I told her that if that was her preference, it was fine with me, and she should do what she wanted. After that, she never came to my lab again.

It was therefore up to me to recruit collaborators from outside NWAFU. I had no trouble finding Chinese citizens who wanted to work with me through my network in China, Europe, and North America. What surprised me was that NWAFU resented my bringing in people from outside. They liked the 1000 Talents program, but they wanted it to benefit only NWAFU employees. There was little they could do, however, to stop me from doing what I considered necessary.

One such colleague was a graduate student from another university who spent nearly two years in my lab. He was the smartest student I had ever had in my career: Wang Yujie. (In China, the family name comes first.) I recommended Yujie to John Sperry for PhD study at the University of Utah. John wrote back, "I am not interested; I have had several Chinese students, and they have all been a disappointment." I replied, "John, I used to tell everyone that you were the best postdoc-scientist I ever had in my lab. But I must tell you that Yujie is better than you were when you first came to my lab."

John's plan was to retire early after receiving his most recent NSF grant. When Yujie arrived in Utah, John explained the goals of the grant and watched him work for a few months. After that, he essentially turned the entire project over to Yujie and let him pursue it as he wished, offering guidance only when asked.

Yujie finished his PhD and then moved into a more permanent position at Caltech, though it still depended on five-year cycles of grant renewal. More recently, concerned about declining research funding in the United States and what that might mean for his long-term prospects, he accepted a permanent job in China. Yujie, his wife, and two daughters moved back more than a year ago; I wish them the very best.

My Five-Year Review and the End of My Contract

As you might guess, there was a fight over renewing my contract, but I did not care very much because I really did not want to stay. I had met all the minimum requirements stated in writing. One requirement was to publish a specified number of papers based on work done within the university, and I had published two more than required. Some of the reviewers came from Beijing, the chair was from the Chinese Academy of Sciences, and the rest came from the Faculty of Forestry and other departments at NWAFU.

I do not remember many of the details, except that there were some very negative comments from the Dean of Graduate Studies; he was rebutted by someone else in Chinese and then stormed out of the room. Most of the discussion was in Chinese. The other thing I remember is that the chair asked, "Didn't you collaborate with other universities in China?" I answered, "Of course I did. I published more with other universities than inside NWAFU, but I was told not to include those papers." He immediately asked to see them all. After reviewing those papers, he praised both the quantity and the quality of the publications. He recommended that my contract be renewed, if I wanted it.

At first, the Faculty of Forestry asked me to stay only long enough to finish writing papers and to mentor the remaining students through graduation, which would take about six months. I agreed. But when I was about to leave, they offered to renew my contract at only 60 percent of my previous salary, saying they could not afford more. I declined and returned home. The 1000 Talents program then ended, but I later learned that within two years NWAFU had hired four more professors at my original salary level outside of the 1000 Talents program.

My return to the USA lasted less than three months.

Back to China: Zhejiang Normal University

In China, a "Normal" university is one traditionally focused on training teachers. The name is still used today even though most Normal universities have evolved into full-fledged research institutions. My job offer at Zhejiang Normal University (ZJNU) came through my former postdoc from NWAFU, Dr. Yang Dongmei. She took a three-year leave of absence from her permanent position in the Ecology Department of the College of Life Sciences at ZJNU, spending one year as a postdoc at Harvard and two additional years working directly with me.

The College of Life Sciences hired me in part because of my national performance ranking in the Life Sciences division; I usually ranked annually between seventh and fifteenth across all of China according to ScholarGPS. Ecology did not yet possess an accredited PhD program, and the Dean of Life Sciences believed that my prominent presence on the faculty might improve their chances of meeting the strict national requirements.

I knew absolutely nothing about the internal accreditation process, but I was entirely willing to help because they offered a good salary, excellent laboratories, modern equipment, and a generous research grant wrapped into a three-year contract. Foolishly, I assumed that if I could not personally supervise PhD students, I would simply hire postdocs to run the lab, as one often does in the United States. That turned out to be a mistake. I discovered too late that Chinese university departments without active

PhD accreditation are legally barred from hiring postdoctoral researchers.

I requested to work at ZJNU for only five to seven months per year. Because I had no physical laboratory space during my first year, I was given permission to collaborate remotely with Professor Steven Jansen at the University of Ulm and Dr. L. Pereira from Brazil. I was asked to develop a mathematical model that would provide the theoretical basis for their pneumatic method of plant embolism detection. At first, I had my doubts about the method, but as the physics of the model developed, those doubts completely disappeared, and a vigorous collaboration followed.

Pereira later flew to Yangling to carry out further validation experiments with improved versions of our equipment. I was frequently invited to travel to Brazil to continue this work, but political unrest there and fluctuating research funding continually delayed those trips. In the end, I never managed to make it there.

I did what I could for six years, but the ecology program ultimately failed to gain its PhD accreditation after two formal attempts. ZJNU graciously stated that they were willing to continuously renew my contract until age ninety. By then, however, my memory loss was slowly increasing—just enough to cause serious, frustrating problems when programming complex models and writing technical scientific papers. Furthermore, I simply did not want to miss any more valuable time with my wife and grandchildren.

In 2023, I said to the Dean, "Please keep my name on the faculty roster if that helps your department, but do not pay me. Because of declining health, I may not even be able to visit the campus again." I have not written any new code or text in the three years since I returned home, and I have published only one paper in the last five years. Nevertheless, in the most recent national ranking released in April 2026, I was still ranked 13th in China, and I place roughly 7,500 out of 30 million scientists and engineers globally (the upper 0.025%). I hope that standing continues to help their department.

The Rapid Expansion of Higher Education in China and Its Effect on ZJNU

Higher education in China is completely free; anyone who qualifies academically can attend university at little to no personal cost. The central government regarded this policy as essential to rapidly accelerating the country's industrial and technological development. In that macro sense, the policy was a massive success, but it has also produced predictable modern consequences: large numbers of recent college graduates now face severe unemployment. Many are forced to return home to live with their parents or take unskilled labor jobs.

Because ZJNU possessed only an MSc program in Ecology, many of the students enrolled in that specific track saw very little employment future for themselves in the current economy. As a direct result, they were often far less motivated to put sustained, rigorous effort into their research projects, or to learn enough professional English to communicate easily with me. This is in stark contrast to my earlier experience at NWAFU, and it greatly reduced my interest to continue teaching graduate students in China.

Vast Changes in Chinese Development

When I arrived in China, there was already a vast freeway network linking all the cities, but traffic was still light and consisted mostly of taxis and trucks. Construction was everywhere, and the jobsites were swarmed with workers like ants in anthills, while most people traveled to work by bicycle. High-speed trains, running at about 325 km/h, connected most cities, and frequent domestic flights linked many of the same destinations. Minor roads, however, were often slow, full of potholes, and at times nearly impassable.

Ten years later construction was still active, but many buildings stood empty. Building sites are more machine-driven with far fewer workers. Freeways were now crowded with private cars, and bicycles are mostly children's toys. The highway system had expanded further, and high-speed rail had become an even more common and economical way to travel between cities.

University life is very different from life at universities in the United States. Everyone lives and works on campus, and most essential items can be bought there as well. Most university teachers own cars, but they have very little reason to drive them often. Major shopping is usually within walking distance, and if you do take a car, it can be difficult to find parking close enough to make driving worthwhile.

China has become one of the most industrialized and automated countries in the world, as well as a leader in major manufacturing and digital technologies. China is the most successful authoritarian government—whether of the right or the left—in the world when measured by its ability to raise standards of living, education, and industrial development. At the same time, China has expanded its control over ethnic minority regions and imposed policies of ethnic suppression. In that respect, it is not unique; all major democracies have their own histories of suppressing ethnic groups as they expanded into new territories.

After a period of somewhat greater freedom of speech, the Chinese government has become more repressive and more aggressive in censoring online information. Young people who upset party leaders may find themselves punished for things their parents or grandparents did during the Mao era. I have met several people who live with that fear. It is a troubling combination and, by itself, a good reason for me to want to leave China. Another compelling reason was that I could not obtain the neurological care and prescription drugs I needed for Epilepsy and Alzheimer's disease while living there.

I returned home only to find the Trump administration moving us in what seems to me a similar direction. For that reason, I do not feel I can be overly critical of Chinese politics.

At least half the total pleasure I took in my long career came from directly motivating my best students, giving them the logical and analytical tools they needed to succeed, and then watching their independent careers beautifully unfold. At least three of them seem firmly on track to become significantly more successful than I ever was, and two others—John Sperry and Hervé Cochard—have already officially surpassed me based on their "H-factor" metrics on ScholarGPS. That reality makes me remarkably happy indeed.

The DNA of Our Future in Life Sciences: My View

DNA is the beginning, not the whole story

At times, I worry that the life sciences have become too heavily dominated by DNA research. Even so, I do not think we should despair. DNA is the alphabet of life—A, C, G, and T—and we are still only beginning to learn how to read the vast instruction books written in that alphabet. That is what makes DNA research so exciting. Evolution selects more competitive species rewriting the instruction book for successful ones.

But those books are still only instructions for how living organisms build and reproduce themselves. They do not explain how living systems function, why one organism performs better than another, or how biological structures produce real-world capabilities. A single error in a DNA 'word' can cause a living system to fail, just as a typo in the design of a machine part can cause the entire machine to malfunction.

Why biophysics still matters

My own interest in life sciences has always been to unite biophysics and biophysical chemistry to understand how living machines function. Like all machines, living systems must obey the laws of physics and thermodynamics. If one organism is more efficient than another at capturing energy, transporting materials, or sustaining life, those differences must ultimately be explained by structure, chemistry, and physical function.

That means we need more than genetic information alone. To understand why DNA works, we must also understand the anatomy, chemistry, and physical behavior of living systems.

Why ecology belongs in the picture

Organisms do not exist in isolation. They depend on one another and function within larger cooperative systems. For that reason, ecology is also essential. To understand life fully, we must understand not only individual organisms, but also the biophysical ecology of the systems in which they live.

What universities should remember

I hope university leaders and those most focused on DNA research will recognize that genetic knowledge, however powerful, is still only part of the reference library of life. If we want to understand living machines in full, we must preserve and strengthen the other disciplines that explain function.

- **Genetics** helps us read the instructions of life.
- **Anatomy** is essential to understanding the structure behind the function.
- **Biophysics and biophysical chemistry** help us understand how living systems operate.
- **Ecology** helps us understand how families of organisms function together within larger systems.

In my view, the future of life sciences depends on keeping all these perspectives in balance. DNA research is necessary, but it is not sufficient by itself. We need a broader scientific vision—one that restores neglected fields, supports emerging ones, and deepens our understanding of how life actually works.

Publications Cited

Cai, J., & Tyree, M. T. (2010). The impact of vessel size on vulnerability curves: Data and models for within-species variability in saplings of aspen, Populus tremuloides Michx. Plant, Cell & Environment, 33, 1059–1069. https://doi.org/10.1111/j.1365-3040.2010.02127.x

Cheung, Y. N. S., Tyree, M. T., & Dainty, J. (1975). Water relations parameters of single leaves obtained in a pressure bomb and some ecological interpretations. Canadian Journal of Botany, 53, 1342–1346. https://doi.org/10.1139/b75-162

Cirelli, D., Jagels, R., & Tyree, M. T. (2008). Toward an improved model of maple sap exudation: The location and role of osmotic barriers in sugar maple, butternut and white birch. Tree Physiology, 28, 1145–1155. https://doi.org/10.1093/treephys/28.8.1145

Dixon, M. A., & Tyree, M. T. (1984). A new temperature corrected stem hygrometer and its calibration against the pressure bomb. Plant, Cell & Environment, 7, 693–697. https://doi.org/10.1111/j.1365-3040.1984.tb01875.x

Galvez, D. A., Landhäusser, S. M., & Tyree, M. T. (2011). Root carbon reserve dynamics in aspen seedlings: Does simulated drought induce reserve limitations? Tree Physiology, 31, 250–257. https://doi.org/10.1093/treephys/tpr012

Galvez, D. A., Landhäusser, S. M., & Tyree, M. T. (2013). Low root reserve accumulation during drought may lead to winter mortality in poplar seedlings. New Phytologist, 198, 139–148. https://doi.org/10.1111/nph.12129

Johnson, R. W., Tyree, M. T., & Dixon, M. A. (1987). A requirement for sucrose in xylem sap flow from dormant maple trees. Plant Physiology, 84, 495–500. https://doi.org/10.1104/pp.84.2.495

Tsuda, M., & Tyree, M. T. (2000). Plant hydraulic conductance measured by the high pressure flow meter in crop plants. Journal of Experimental Botany, 51, 823–828. https://doi.org/10.1093/jexbot/51.345.823

Tyree, M. T. (1970). The symplast concept: A general theory of symplastic transport according to the thermodynamics of irreversible processes. Journal of

Theoretical Biology, 26, 181–214. https://doi.org/10.1016/S0022-5193(70)80012-1
Tyree, M. T. (1975). Some inconsistencies that arise when the Canny Phillips model of phloem translocation is applied to known ^{14}C assimilate profiles in plant stems and petioles. Canadian Journal of Botany, 53, 1128–1131. https://doi.org/10.1139/b75-133
Tyree, M. T. (1983). Maple sap uptake, exudation, and pressure changes correlated with freezing exotherms and thawing endotherms. Plant Physiology, 73, 277–285. https://doi.org/10.1104/pp.73.2.277
Tyree, M. T. (1997). The cohesion-tension theory of sap ascent: Current controversies. Journal of Experimental Botany, 48, 1753–1765. https://doi.org/10.1093/jxb/48.10.1753
Tyree, M. T., & Dixon, M. A. (1983). Cavitation events in Thuja occidentalis L. Ultrasonic acoustic emissions from the sapwood can be measured. Plant Physiology, 72, 1094–1099. https://doi.org/10.1104/pp.72.4.1094
Tyree, M. T., & Dixon, M. A. (1986). Water stress induced cavitation and embolism in some woody plants. Physiologia Plantarum, 66, 397–405. https://doi.org/10.1111/j.1399-3054.1986.tb05941.x
Tyree, M. T., & Ewers, F. W. (1991). The hydraulic architecture of trees and other woody plants. New Phytologist, 119, 345–360. https://doi.org/10.1111/j.1469-8137.1991.tb00035.x
Tyree, M. T., & Hammel, H. T. (1972). The measurement of the turgor pressure and the water relations of plants by the pressure bomb technique. Journal of Experimental Botany, 23(1), 267–282. https://doi.org/10.1093/jxb/23.1.267
Tyree, M. T., & Tammes, P. M. L. (1975). Translocation of uranin in the symplasm of staminal hairs of Tradescantia. Canadian Journal of Botany, 53, 2038–2046. https://doi.org/10.1139/b75-228
Tyree, M. T., Christy, A. L., & Ferrier, J. M. (1974). A simpler iterative solution of Munch pressure flow systems applied to long and short translocation paths. Plant Physiology, 54(4), 589–600. https://doi.org/10.1104/pp.54.4.589
Tyree, M. T., Graham, M. E. D., Cooper, K. E., & Bazos, L. J. (1983). The hydraulic architecture of Thuja occidentalis L. Canadian Journal of Botany, 61, 2105–2111. https://doi.org/10.1139/b83-226
Tyree, M. T., Patiño, S., Bennink, J., & Alexander, J. (1995). Dynamic measurements of root hydraulic conductance using a high pressure flowmeter for use in the laboratory or field. Journal of Experimental Botany, 46, 83–94. https://doi.org/10.1093/jxb/46.1.83
Wang, R., Zhang, L., Zhang, S., Cai, J., & Tyree, M. T. (2014). Water relations of Robinia pseudoacacia L.: Do vessels cavitate and refill diurnally or are R-shaped curves invalid in Robinia? Plant, Cell & Environment, 37, 2667–2678. https://doi.org/10.1111/pce.12315
Wei, C., Steudle, E., & Tyree, M. T. (1999). Water ascent in plants: Do ongoing controversies have a sound basis? Trends in Plant Science, 4, 372–375. https://doi.org/10.1016/S1360-1385(99)01466-1
Wei, C., Tyree, M. T., & Steudle, E. (1999). Direct measurement of xylem pressure in leaves of intact maize plants: A test of the cohesion-tension theory taking hydraulic architecture into consideration. Plant Physiology, 121, 1191–1205. https://doi.org/10.1104/pp.121.4.1191

Honors

- Killam Senior Research Fellow (The Killam fellowship paid full salary for awarded to one or two scientists annually based on a national competition open to all sciences and engineering.)
- C.D. Nelson Award (given by Canadian Society of Plant Physiologists for "outstanding contributions to plant physiology," 1979.)
- Scientific Achievement Award-IUFRO (given by International Union of Forestry Research Organizations at the 19th World Congress, 1990, Montreal, Canada for "increased understanding of transport in plants").
- Humboldt Research Award for Senior US Scientists (130,000 DM in recognition of past accomplishments in research and teaching; awarded Oct. 1996. Only one other USDA employee has ever received this award.)
- Japanese Society for the Promotion of Science (Senior visiting scientist fellowship for two months in Japan; awarded July 1996)
- USDA Forest Service Distinguished Science Award Dec. 1996 (For outstanding contributions to understanding plant-water relations, stress physiology and biophysics of land and short distance transport of water, sugar and minerals.)
- Marcus Wallenberg Prize Sept 2002 (For "his pioneering scientific discoveries leading to basic understanding of water transport in trees") Awarded by the King of Sweden in Stockholm and included cash prize of $217,000.
- Honorary Doctorate, St. Francis Xavier University, Canada June 2003.
- Presidential Rank Award 2005 In recognition for scientific contributions to US Federal Service Included cash prize of $28,000
- Gifford Award in Tree Physiology 2007 given by Canadian Society of Plant Physiologists for outstanding contributions to Tree Physiology in Canada.
- Elected Fellow of the Royal Society of Canada (Nov. 2008).