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BEHIND THE CV: REFLECTIONS ON LARRY LAUDAN'S UNCONVENTIONAL ACADEMIC CAREER

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Larry's academic career, as is typical, is summed up by that most ritualized of documents, the curriculum vitae, a dry catalog of education, awards, publications, and academic positions. As far as the latter goes, his cv shows that at the age of 40 Larry resigned from a top flight philosophy program at the University of Pittsburgh, one that he had helped build. He moved to a fledgling science studies center at Virginia Tech, a university strongly oriented to agriculture and engineering, and consequently weak in the humanities. Shortly thereafter, Larry took a position as chair of the philosophy department at the University of Hawaii, an institution marginal in every way to American academic philosophy. In his mid 50s, he left academia. At 60 he rejoined it, accepting a position at the National University of Mexico, pivotal in the Spanish-speaking world, but unknown to most American academics. This succession of university appointments was characterized by a friend as "scarcely conventional" and regarded by most of his colleagues as something between quixotic and disastrous.

This "scarcely conventional" career resulted from Larry's effort to reconcile his lifelong pursuit of an intellectual problem with my own academic career, against the background of academic politics. The intellectual problem was that of understanding how to improve the inferences from the evidence to justified belief whether in a scientific theory, a legal verdict, or in life more generally. My career, like all women's in the 1970s, had to be forged in the context of the century-long reluctant opening of academia to others than white protestant males—Jews, working class, women, and blacks among other. The career was further complicated by my penchant for subjects unfashionable in my discipline of history and philosophy of science: geology,

technology, and food. For both of us, the relevant academic politics were the rigid university pecking order and the departmental micropolitics surrounding positions and research fashions.

For all this, Larry ended up feeling that his career, for all its ups and downs, had been deeply satisfying (as I did too). So this essay is less a defense of his decisions, than an invitation to new generations of scholars who, in the United States and elsewhere, face universities struggling to define their place in difficult political times, to consider careers other than the prescribed conventional ones. They would not, of course, take the same shape as Larry's as times are different, and so are the alternatives that might be possible.

I shall say nothing about Larry in the classroom although he was a born teacher. Nor shall I focus on his administrative work, though understanding that without institution building his intellectual ambitions would come to nothing, he handled work as chair, editor, conference organizer with skill and dispatch.

LARRY'S EARLY CAREER

Larry's early academic career in the 1960s, a time when American universities were expanding, was unusual only in being particularly fast track. From his early teens; Larry had been fascinated by astrophysics and the way in which small data points could be used to construct sweeping theories of the universe. Thus, after high school in Kansas City, he majored in physics at the local state university, the University of Kansas. From there, with his first wife, he went to Princeton. His goal was an astrophysics Ph.D. directed by the theoretical physicist, John Wheeler.

Discovering that his dissertation would depend on mathematical prowess, not on probing why it was more reasonable to believe one theory of the universe rather than another, he looked for a more congenial program. History and philosophy of science, just then being established in American universities with support from the National Science Foundation, promised a different way of understanding when theories were worthy of acceptance. He began his thesis on *The Idea of a Physical Theory from Galileo to Newton: Studies in 17th-Century Methodology* with a committee consisting of the distinguished philosopher of science Carl G. (Peter) Hempel famed for his work on explanation in science, Thomas Kuhn who had just published *The Structure of Scientific Revolutions*, and Charles Gillispie, a leading historian of science.

Larry found the twinned histories and philosophies of science by the Victorian scientific polymath, William Whewell and the French physicist, Pierre Duhem particularly inspiring. They had turned to history to elicit how science had progressed by choosing successively better theories, thus surging ahead of the humanities in terms of reliable knowledge. With those models in mind, Larry explored how leading scientists in the seventeenth-century scientific revolution justified their choices of new

theories. Like his heroes, in the back of his mind was the hope that this learning how to learn could be applied more generally outside the sciences.

Larry's committee was on the point of recommending him for positions in prestigious American universities when Larry announced that he was going to work with the pioneering historian of philosophy of science, Gerd Buchdahl in Cambridge, England. Cambridge too was an elite university, but it was outside the sphere of influence of American academics. Since the hierarchy of American universities depended on faculty at the elite universities placing their graduate students at closely similar institutions, they were miffed. He was told he could no longer count on their support.

In England, Larry completed his dissertation. He accepted a lectureship (assistant professorship) at another major institution, University College London, in the world's oldest history and philosophy of science department, perfect for his interests. At the same time, he, with Gerd Buchdahl, founded and edited *Studies in the History and Philosophy of Science* to create a place to publish the longform articles that they believed essential to an integrated history and philosophy of science.

Larry by now had carved out his intellectual niche. Neither at that time nor later did Larry see himself primarily as a philosopher. While he admired the clarity and argumentative precision of the analytic philosophy entrenched in Anglophone philosophy departments, he had no interest in metaphysics or traditional epistemology, let alone ethics or aesthetics. More important, he rejected the assumption that analysis of language and concepts would yield worthwhile knowledge, progress of knowledge, well-founded beliefs.

Instead philosophy, like science, was an empirical enterprise. His particular interest in learning how to improve inference from data to belief could not be answered without resort to particular, comparative cases, and thus to history. To clarify his ideas, Larry scrutinized the small number of crucial papers and then turned to conversation. For him, the back and forth of a graduate seminar, the question and answer following a talk, an informal conversation, or an internal dialogue were not only fun but intellectually crucial. (For those who did not know him well, the intensity of these conversations could be daunting). This was no ivory tower exercise. The results had practical importance, part of the broader project of how human decision making could be improved by clarifying the relations between evidence and belief.

In 1969, the philosopher of science Adolf Grunbaum offered Larry a position in the University of Pittsburgh Philosophy Department. Grunbaum was building the department into one of the two or three best in the nation, so even though the University as a whole was only mid-ranked, this was an appealing offer, especially with a large salary increase. Given Larry's interests, Grunbuam arranged for a joint appointment in History.

On arriving in Pittsburgh, Larry discovered that, because of departmental micropolitics, the situation did not live up to his expectations. Mainstream historians

regarded history of science not as the key to progress in knowledge, but as marginal and elitist compared to labor and environmental history. That administrators wanted the newly-available funding from the National Science Foundation that historians of science could bring in, only made the resentment worse. Larry was cold shouldered.

Larry thus enlisted Grunbaum's help in founding an independent Department of History and Philosophy of Science. Given the institutional setting, his interests and the department's tilted in the philosophical direction. By 1972, Larry was Chair and Full Professor. He was 31 years old. He probaby assumed he would spend the rest of his career building the department, and carrying on those crucial conversations with colleagues and graduate students and with visiting scholars from across the States and beyond that arrived every year.

OUR TWO-CAREER PROBLEM

But no. In 1972, Larry and I ran into each other at a meeting of the American Academy for the Advancement of Science in Washington, D.C. We had met some years earlier at University College London when I was a graduate student in History and Philosophy of Science, but both married, we had had only the most formal of relations. Shortly after this second meeting, we both left our first marriages, a decision that was made swiftly, but not impulsively. We were married two years later in 1974.

Unusually for the 1950s, my intellectual family had raised me to expect a career, not simply a job to supplement the family income. My all girls 'public' school, staffed by first-wave-feminist women, reinforced that expectation. Consequently, as an undergraduate I happily embarked on a degree in geology, a field that was hardly hospitable to women and won a First Class Honors degree. I was accepted to pursue a Ph.D. at the premier Lamont-Doherty Observatory at Columbia University. Before leaving, I stumbled onto the works of Stephen Toulmin and June Goodfield, Norwood Russell Hanson, and Thomas Kuhn in the university library. Enthralled by their revelatory understanding that the history of science was not just a random sequence of inventions and discoveries but linked by underlying intellectual traditions, I instead signed up for a History and Philosophy of Science Ph.D. at University College London.

Sad to say, the program was less than ideal. As was common in the British system, it involved almost no coursework or supervision, so what I knew of history came from my high school days and I learned nothing of philosophy. On my own, I decided to explore the links between the Industrial Revolution, the emergence of geology as a science, and the method of geological mapping. Maps were to geology what mathematics was to physics and astronomy, I decided, ways of concisely representing the phenomena and of making surprising predictions.

On the personal side, what would now be called rampant harassment was the order of the day at both University College London and the associated London School of Economics. A network that stretched from those institutions to universities on the east and west coasts of the United States took it for granted that any woman pursuing a Ph.D. would go to bed with them. Ironically this was an unintended consequence of the second feminist wave which was arguing for sexual liberation. Many male faculty considered seducing female students was the moral high road to the desired aim of destroying traditional marriage. It was not peculiar to those institutions. I was thoroughly confused. My conversations with other women academics in the last year or two made it clear how many women in Anglophone abandoned career hopes rather than be subject to it. I survived. Most didn't. "Why had I not reported this at the time?" asked Larry later. "Apart from having no idea to whom I would report it, would they have believed what I said about internationally-known full professors? Would you?" "No," he said. Point made.

In spite of all this, I realize in retrospect that I worked my way toward my own intellectual niche, which turned out to be distinct from Larry's even though our enthusiasm for history and philosophy of science was mutual. I was fascinated by uncovering the intellectual traditions that bind seemingly disparate historical events together. Where Larry leaned on conversation to clarify his ideas, I loved documents—books, maps or archives. On doing research relevant to an audience wider than just academics, we converged again. These insights came years later though. At the time I was glad to find the English equivalent to an American tenure-track position.

On arriving in Pittsburgh, a two-career academic marriage looked possible. I was offered a tenure-track position in History at Carnegie-Mellon University, an excellent institution next door to the University of Pittsburgh, to teach history of technology to engineering students. Luckily the hiring committee was unaware that history and philosophy of science assiduously avoided any connection with grubby history of technology. And because my dissertation dealt with the practical as well as the theoretical side of geology, I had learned a good bit of history of technology on my own.

Then in my third year, a new Dean of Arts and Humanities needed positions for a new Social Sciences Department. I, along with the rest of the untenured faculty was fired. Larry now had to face up to the difficult decisions a two-career marriage could entail. In spite of our premarital conversations, I think he had always assumed that, like his mother and first wife, I would be content to work at something pleasant to supplement the family income, but would not take on anything that would clash with his career.

We began considering the options. A tenure-track position at the University of Pittsburgh was out of the question. Institutional anti-nepotism policies were in place that (largely rightly) did not allow hiring or promotion of family members. Even had this not been the case, Philosophy and History and Philosophy of Science, like most other American academic departments, were overwhelmingly male, at best passively

resistant, at worst openly hostile, to women as colleagues. And it was obvious I was ignorant of philosophy. This was compounded by my research in geology. Philosophers of science, Larry included, took it for granted that theoretical physics was the archetypal science, that biology was barely worth consideration except insofar as it could be derived from physics, and that geology, lacking mathematical expression and universal laws (so they assumed) was not even worthy of the name science. Finally, although I was only two years younger than Larry, thanks to a "gap year" teaching in Nigeria and other interruptions, I was a generation behind him academically. The career gap made it all too easy for me to be characterized as an unserious, unscrupulous bimbo.

Maybe I should take a job in one of the many corporations headquartered in Pittsburgh? Maybe I should pursue a career in law? No. Maybe I should commute to a university in another city? No again. By now I knew Larry would not flourish with a wife in a nine to five job, let alone another city. He needed support and understanding—whether for his eccentric working hours from noon to 3 am, or his constant pipe smoking, or even for the endless dinners and cocktail parties his work involved, not to mention reading and copy editing drafts of publications.

I took an adjunct position at the University of Pittsburgh and did the day-to-day administration of the Philosophy of Science Center. Both were made-up positions to accommodate a spouse, the kind of positions that were to become increasingly common in American academia. While I was initially grateful for the efforts that had been made to find some kind of place for me, I grew increasingly resentful that these were dead end positions. Meanwhile Larry continued to publish, including his well-received first book, *Progress and Its Problems* (1977) and took on the Directorship of the Philosophy of Science Center.

After several years, I explained to Larry, rather emotionally I remember, that these stop gaps were not working. Our increasingly unequal status was wreaking havoc with the kind of partnership we had envisioned. Since Larry's children, who meant the world to him, now lived far away, visiting only a couple of times a year, that formerly compelling reason to stay in Pittsburgh had vanished. Perhaps the time had come to make good on his promise that I could pursue a career and look for two positions in one place. We talked this over with the only academic couple we knew in Pittsburgh, Rolland and Cristina Paulsten (Education and Linguistics respectively). With their support, we decided to go on the job market.

THE MOVE TO A LESS-PRESTIGIOUS DEPARTMENT

In 1980, the Dean of Science and Humanities at Virginia Tech offered Larry tenure in Philosophy, me my desired tenure-track position, in History as seemed appropriate, with joint appointments in the new Science Studies Center being set up under the leadership of the historian of science, Arthur Donovan for both of us. In

considering whether to accept, I knew I was asking a lot of Larry. Although the salary offered was good, Larry knew he would miss his valued colleagues and graduate students, perhaps have a higher teaching load and less money for travel and sabbaticals, and a lesser library. He feared he would lose his place at the center of the national network of scholars working on related problems.

And all this added up to loss of prestige, something much valued in academic life. Nonetheless, he accepted. If other men had made a downward move for the sake of their wife's academic career, we did not know of any. Decision made, Larry never expressed regret or complaint either at the time or during the rest of his life.

Fears of the academic periphery notwithstanding, in the years at Virginia Tech Larry not only continued to publish but produced some of his best known work in history and philosophy of science, consolidating his intellectual reputation. Among his articles and books were "A Confutation of Convergent Realism" (1981), *Science and Hypothesis* (1982) on how the history of philosophy of science had its own trajectory, quite different from the history of epistemology as generally understood, and *Science and Values* (1984) on adjudicating cognitive values. For conversation, he found the philosopher of statistics Deborah Mayo, Jarrett Leplin in the nearby 'research triangle' of North Carolina, as well as the visitors and seminars in the Science Studies Center invaluable in replacing his Pitt buddies.

The relevance of his work to broader issues was also becoming clear. With gusto, he entered the so-called 'science wars.' The legitimacy and authority of science which Larry so valued was being challenged by sociologists, literary theorists, and anthropologists, with potentially serious consequences. The relativism of this anti-science movement was unfounded, Larry argued. Science, although its theories could not be known to be true, and although scientists made many blunders, produced more reliable knowledge than any other intellectual endeavor.

Ironically at the same time, letters kept arriving from groups such as nurses hoping to learn how to achieve scientific status for their discipline by adopting one or another of the theories of scientific change then in play. Although Larry himself, in the wake of others such as Popper, Kuhn, and Lakatos, had proposed one of these theories of scientific change, he worried that the theories themselves did not begin to have evidential warrant, being based on a handful of case studies. This was the origin of what came to be known as the Virginia Tech project. Eight of us from Virginia Tech and elsewhere—both historians and philosophers—produced a catalog of the empirical claims made by the different theories of change. We then invited anyone knowledgeable, whether a scientist, a sociologist, a philosopher or a historian, to write a paper explaining why the case they knew best did or did not bear out one of those claims. The procedure was collegial, though historians of science bristled at the thought of being asked to test philosophical claims.

Along similar lines, when fellow philosopher of science, Michael Ruse, made the case against creationism using what Larry regarded as a bad argument that in the end

gives support to creationism, he leapt into the fray to explain why. It did not win him friends across the board, many believing creationism had to be defeated whatever the later fallout, but it was one of many times when Larry acted on what he believed.

I was busy too. I sent off my manuscript, *From Mineralogy to Geology: the Foundations of a Science*, to the University of Chicago Press. I was granted tenure. My next project—uncovering scientists' evolving ideas between 1750 and 1950 about why science evolved—was well under way, supported by the two major government funding agencies, the National Science Foundation and the National Endowment for the Humanities. Larry as always trusted me and supported my work in history of geology, technology, the history of the politics of science (and later food) even when he either did not understand its importance or was uninterested in the issues. Never did he try to divert me to topics that he thought more reasonable or more likely to advance my career.

We had a good social life. Living in the beautiful valley between the Blue Ridge and the Appalachians, was pleasant. To relax, Larry added local folk music festivals and playing the guitar, banjo, and dulcimer, to the strategy games he had played in Pittsburgh. I meantime was off hiking in the mountains. We could have stayed at Virginia Tech.

That was not to be however. Institution building is perilous. Creating the Science Studies Center had unleashed a micro-political storm. The Dean of Science and Humanities had diverted positions and money from existing departments (something I had already experienced to my dismay at Carnegie-Mellon). The new chair of History, who like the historians at Pittsburgh disdained history of science as marginal, fought to get them back. Larry's high salary, part of the practice just then beginning of hiring 'stars' to improve a department or university's rating, had not surprisingly generated resentment. We certainly made mistakes. So did others. Tempers flared, insinuations and rumors flew. Even normally steady and equable Larry was shaken. By the second half of the 1980s, the Dean had been forced to resign and Arthur Donovan, Larry and I had decided that the storm could not be ridden out. Once again, we went on the job market.

MOVING TO THE MIDDLE OF THE PACIFIC

In 1987, Larry accepted an offer to chair the University of Hawaii Philosophy Department. The department's strength, Asian philosophy, ranked even lower than continental philosophy in the opinion of analytic philosophers. With the mainland 2500 miles away across open ocean and email not even in its infancy, staying 'in the network' with colleagues looked difficult to impossible. If moving to Virginia Tech had been a demotion, this seemed as if we might be falling off the edge of the world. Friends chuckled at the thought of Larry under a swaying palm watching the waves rolling in, swopping philosophy for sunbathing.

Since Larry had forfeited much for me in our previous move, it was now my turn. As long as he was in charge, the chair of History told me, a position for an accompanying spouse would never be forthcoming. I started over in a part-time non-tenure track position in a marginal department (general science for future teachers) that the administration wanted to abolish as quickly as possible—an accompanying spouse dumping ground.

The Islands were exorbitantly expensive, the multi-racial population in which east Asian not Anglo culture predominated, thanks to the descendants of Hakka and Han Chinese, Japanese and Okinawans, Koreans, and Filipinos who had come as indentured laborers on the sugar and pineapple plantations, was not welcoming to mainlanders. I remember trudging up a hill on my daily walk, kicking one of the fist-sized invasive snails that are such a nuisance in the islands, reflecting guiltily that I was back to square one, and Larry too was worse off. We were going backward, not forward.

Once again, this initial angst proved to be not just overwrought but quite misplaced. We began to enjoy the intellectual life, not just the sun, sea, and sand so relentlessly promoted to tourists. Larry found philosophers of science, Ron Amundsen at the regional campus, Ron Pine at a community college good to talk to. The United States so over-produces Ph.Ds. that every university has some excellent faculty. Angus Graham, Roger Ames, and David Kalupahana in Chinese and Buddhist philosophy convinced Larry that these were intellectually serious endeavors misunderstood because of poor nineteenth century translations. Another colleague, an enthusiast for applied philosophy, encouraged Larry's growing interest in legal evidence. Then there were astronomers, vulcanologists, biologists, oceanologists and agronomists attracted by the physical environment. Many in the humanities specialized in east Asian Studies. For my part, I was fascinated by the historians, led by Jerry Bentley and distinguished visitors, who were figuring out how to write reputable world history in a place where courses limited to the 'West' made no sense.

Our careers did not seem to be suffering as we had anticipated either. Larry published his rebuttal to relativism in the form of a dialogue, *Science and Relativism* (1990). He was elected president of his professional society, the American Philosophical Association, Pacific Division.

I worked my way into a full-time, tenured, full professorship, and chaired the General Science Department as it morphed into a Biology Department. I was close to publishing my book on the history of ideas of scientific progress. As a hobby, I had begun exploring the many small eateries and mom n' pop stores locally, as well as the excellent and beautiful State Library, to understand the to-me exotic foods so important to the students and staff in the department. I came to understand that three diasporas, from the south Pacific, from Asia, and from Europe and the Americas, had each brought their food systems to these barren island and used them to survive and create a unique local culture.

After the predominant mainland use of food as a vehicle for snobbery, understanding food as a reflection of cultural history was refreshing, intriguing and challenging. I discovered a network of independent food scholars that stretched from England to the United States, from the Philippines to Mexico whose generous collegiality was a welcome change in academic reserve. My book, *The Food of Paradise: Exploring Hawaii's Culinary Heritage*, was about to be published by the University of Hawaii Press.

LEAVING ACADEMIA

We were, however, becoming increasingly restless in academia. Among our concerns about the direction it was taking were the disregard for undergraduate teaching; the rising cost of a university education for students and their families; the decline of collegiality that accompanied the star system; the inaccessibility of campus resources to the wider public; the increasing bureaucratization; and the uncontrolled proliferation of graduate programs and consequent growth of justifiably-disgruntled un- or under-employed Ph.Ds.

Our restiveness intensified during a visiting year in Princeton in the early 1990s. We had anticipated enjoying being back in the academic mainstream and the culture of the east coast. In many respects, our hopes were fulfilled. The Friday Seminars of the Davis Center of the History Department were a model of intellectual engagement, with their pre-circulated papers and unsparing but collegial debate. Meeting old and new colleagues visiting the School of Social Science in the Institute for Advanced Study for their year dedicated to science was a pleasure.

Other encounters were not so productive, however. The permanent members of the School of Social Science made it clear that that they had felt pressured to make science the topic for the year and that they were on the 'anti' side of the science wars. We visitors speculated about what was going on to create this dismal atmosphere. Larry was growing weary of rehearsing arguments against relativism. For my part, I was discouraged at the thought of publishing a book that showed how radically scientists' ideas about progress had changed over time. It could be too easily seen as yet more anti-science ammunition. I tried out on various faculty the alternative plan I was hatching of writing a world history of food using Hawaii as a model. They winced at the word 'food' and again at the word 'world.' Neither fitted their vision of good history.

And to our surprise, after Hawaii, the culture of the Princeton institutions now seemed tired and rather dull. Conversations all too often consisted of gossip about the elite universities of the coasts, western Europe and Israel or commentary on articles in the New York Times, the New York Review of Books, and the New Yorker. From the point of view of the Asia-Pacific region from which we now came, having this handful of publications circumscribe cultural life seemed a tad parochial.

We discussed our growing disenchantment with the seemingly unquestioned assumption that everything important academically happened in elite centers with the distinguished social scientist, Don T. Campbell, who frequently visited Princeton. He pointed out that among social scientists it was common knowledge that innovations were frequently introduced from the periphery, where there was less pressure to conform and more exposure to stimulating new influences. The problem was to get a hearing for the innovations in centers that condescended to the periphery. If that could be done, perhaps because of the reputation of the scholar on the margins, mutually beneficial interaction ensued. That, unfortunately, was not happening in the School of Social Sciences.

On our return to Hawaii, Larry and I mused that we had spent all our conscious years in educational institutions. Did we want to continue for another quarter of a century? Would we begin repeating ourselves? What new would we learn? Although Larry continued to hold as an ideal an integrated history and philosophy of science that informed the larger scientific enterprise, he saw no hope of that in the immediate future. He was not even sure he wanted to continue intellectual work. I was, by now yearning to have a go at a world history of food, laying bare the intellectual causes of major culinary changes. Larry had earned a good salary from a young age so that his university-matched savings plans were well funded. If we could earn a little money on the side, our savings would tide us over for the remaining years before we could claim the American safety nets of Social Security and Medicare. We were not tied to laboratories as so many scientists were. Was it time to leave academia, free up positions for younger scholars, and embark on a new adventure?

Yes, we decided. We assumed many late middle-aged academics like us with savings tucked away were considering something similar. It seemed not. A colleague from Harvard told me that he and some of his friends had also considered leaving academia in their 50s. But what about the loss of prestige and structure associated with a university position?

Never one for looking back once he had made a decision, Larry began planning our exit with me. He got advances on two popular books on the gap between the evidence and popular beliefs about risks, *The Book of Risks* (1995), and *Danger Ahead* (1995). Once again, he was happy to see his work have practical import when one was excerpted at length in *Good Housekeeping* in the same month that he gave his presidential address to the American Philosophical Association. I wrote seemingly endless encyclopedia articles, at least learning how to quickly summarize the essentials of a topic.

We had to find somewhere less expensive and less distant than Hawaii. We did not want to be nomads, nor to be confined to an expatriate community. We wanted an interesting location where we could become reasonably fluent in the language. The mainland United States? Hardly a new challenge. Europe? Too expensive. Asia? No way we could learn the language fast enough. Australia and New Zealand? Too far from family.

Mexico looked good. Prior visits to give talks at the National Autonomous University in Mexico City (the UNAM) had impressed us with the intellectual life and its pivotal position in the Spanish-speaking university world. The thought of seeing the Americas from the Spanish-speaking perspective was intriguing. We alerted colleagues at the UNAM to our decision, telling them that we would enjoy informal connections, such as participating in conferences or meeting graduate students, but that we were not interested in positions as we had decided to leave academia.

In 1996, we moved to Guanajuato, Mexico, a beautiful colonial town on the high central plateau, a place for several centuries at the center of the global silver trade, with architecture, libraries, and economic and political traditions that reflected this heritage. We set about learning Spanish, helped by doctoral students, particularly Godfrey Guillamin, who stayed with us exchanging lessons in return for conversation about his thesis.

Larry toyed with various ways to occupy himself. He wrote drafts of books on intellectual revolutions, the Copernican revolution, and political theory, but was not satisfied with any of them. He considered starting a business, an itch he had inherited from his entrepreneurial parents. An earlier stab at a computer game rental business had come to nothing, but something else might occur to him. Perhaps, given the risk books had brought lots of queries, consulting would be interesting and lucrative. Fiddling around with internet access with the computer specialist at the Guanajuato University Library was lots of fun. None of these ventures held his attention for very long.

What did hold his attention was comparative procedure in Mexican (Roman and Anglo-Saxon law). Helped by Mexican colleagues, particularly Juan Antonio Cruz Parcero and Edgar Aguilera, Larry, continuing his lifelong fascination with evidence and rational decisions, this time verdicts in criminal law, began working seriously on what he called legal epistemology. On our annual visit to Austin, Texas for libraries, shopping, and a base for visiting family, Larry introduced himself to the philosopher of law, Brian Leiter, at the University of Texas at Austin Law School, who not only introduced him to other philosophers of law but arranged for him to teach an accelerated course in jurisprudence in the Law School. Meantime, Ron Allen, Wigmore Professor of Jurisprudence at Northwestern University, who had once years before contacted Larry about a philosophical question, was back in touch with him. The two of them began on a series of jointly authored papers.

BACK TO ACADEMIA

So in 2001, Olbeth Hansberg, Director of the Philosophical Research Institute at the National University invited us to Mexico City and offered us both positions in the Institute. By this time, Larry was happy to accept. About that time, Spanish philosopher of law, Jordi Ferrer Beltran, visited the Instituto and was delighted

to discover in Larry a like-minded colleague. Before long Larry was organizing regular small conferences—all the better for intense conversational exchange—on legal epistemology. These brought together philosophers of law who might never have met otherwise from Mexico, Spain, France, Italy, Colombia, England, and the United States, including Amalia Amaya and Andrés Páez, the editors of this volume. Larry was particularly delighted that, unlike his work in philosophy of science that was largely ignored by scientists, lawyers took an interest in his work in legal epistemology.

Larry was now back from the periphery in two centers, the Institute which was a major hub in the network of Spanish-language universities, and the University of Texas at Austin Law School. This was something he had never anticipated on our move to Mexico. In 2008, about a decade after he had declared his departure from academia, Larry published *Truth, Error and Criminal Law* with Cambridge University Press.

I declined the offer from the Institute, since my research was just too far from its mission. I now felt self-confident enough to pursue intellectual work outside the academy. Meanwhile, while retaining my extra-university network of food scholars in the Anglophone world, I met Mexican historians and anthropologists of food in the seminars of the Anthropological Institute. Thanks to the generous Visiting Scholar status, which although unpaid, gave crucial library access first in Latin American Studies, then in History at the University of Texas at Austin, I was able to complete *Cuisine and Empire: Cooking in World History*. To my astonishment, food history had become trendy in history department micropolitics. Even geology was having its moment in history and philosophy of science circles.

During the Mexico years we made new friends, relished the excitement of working in new research areas, and had the enormous pleasure, thanks to our locations and invitations to Spain, Argentina, Panama, and Colombia, of seeing the Americas from a whole new perspective. It was a wonderful, invigorating fifteen years to top off our careers.

Then in 2012 Larry announced we were leaving Mexico, a unilateral decision that was completely out of character for him. Only later did I understand that he realized his Spanish, crucial for conversation, was slipping. Whether he understood it was the onset of dementia, something both his grandfather and father had suffered and he had always feared, I have no idea. Soon he resigned from UT Austin, though he continued to struggle to write. At her invitation, we moved closer to his daughter, and to his son too. Larry passed his remaining years largely peacefully and without ever knowing, or at least acknowledging, that anything was wrong.

CONCLUSION

Was Larry's unconventional career worth it? Not everyone would have found it so. The frequent moves, both permanent and for visiting appointments, took a toll in terms of time, money, and probably reputation. Remaining in one place, and hammering away at a single topic instead of switching research areas, might have meant more recognition of an official kind. Yet the journal Larry created and the departments in Pittsburgh and Virginia Tech that Larry established or helped establish still flourish. His works in philosophy of science and law are still cited. Legal epistemology is thriving in the Spanish-speaking world and the English-speaking one too. Larry always had time to puzzle over issues he believed to be important and to have at least some of them taken seriously in the extra-academic world. To have lived and worked (not just been a tourist or expatriate) in England, the mainland United States, Hawaii, Mexico, and (briefly) in Argentina and Spain was an honor. It was Larry's hope that his move from one university to another and from one country to others contributed to collaboration in the academic world, though that is for others to judge. Finally, Larry and I had changed in parallel ways over our fifty years together, we had made good friendships, we had remained equal partners, trusting each other, and making decisions jointly. The bet Larry and I had made on each other when we got together had paid off handsomely.