An Academic Memoir: Untold Tales in lieu of a CV

I am what is called a professor emeritus - from the Latin, 'e' for out and 'meritus,' and so he ought to be.
Stephen Leacock 1938

A PhD Saga

In the autumn of 1958, as a first-year Arts student at the University of Alberta, I registered in Principles of Economics, a full-year course taught by Eric J. Hanson (1912-1985, PhD Clark, Chair 1957-1964). Following the mid-year examination I received a letter from Hanson inviting me to register in the Honors Economics program from which I graduated with First Class Honors in 1962. Along with my BA I was awarded the MacGibbon Gold Medal in Political Economy. (Economics and Political Science became separate departments in 1964) In 1968 I received my PhD from the University of Birmingham (UK) where I was a Commonwealth Scholar during 1962-64 and a Canada Council Pre-Doctoral Fellow in 1965-66. My PhD thesis, *Studies in the Term Structure of Interest Rates,* was awarded the Ashley Prize, a princely sum of ₤10, by the Faculty of Commerce and Social Science. (The economic historian, Sir William Ashley, was the founding Dean of the Faculty in 1901) Alan Walters, later Sir Alan (1926 -2009), Margaret Thatcher's sometime economic adviser in the 1980s, was my thesis supervisor and the Canadian economist Harry Johnson (1923–1977) the external examiner. Johnson held a joint appointment at LSE and Chicago as well as being the editor of the *Journal of Political Economy* (*JPE*) at the time. He conducted my PhD oral over drinks at a 5 pm reception sponsored by the University of Calgary at the June 1968 meeting of the Canadian Economics Association where we were both on the program. I went to the reception to arrange a time and place for the oral but Johnson replied to my query with "What's wrong with right now?" After an hour of drinking sherry on an empty stomach we proceeded to the bar where I was plied with Scotch for another hour by Johnson and his friends. I returned to my residence room and spent a miserable night regurgitating the sherry and Scotch into a wastebasket. I crossed paths with Johnson the next day and reminded him there was a deadline at Birmingham for the results of the oral. He replied that the letter had already been sent from Chicago, for reasons that will become clear later. For Johnson, the oral had been an amusement, for me a gut-wrenching ordeal. Only later did I learn that Johnson had a reputation as a prodigious drinker with a capacity that few could match drink for drink.

The British PhD system at that time was a model of simplicity – there were no prelims nor orals conducted by an examining committee, just one supervisor and one external examiner and that made the process correspondingly risky. Few will know the story of Robert Clower (1926 - 2011, editor of the *AER*, 1981-1985, and Fellow of the Econometric Society), whose DPhil thesis was turned down by John R. Hicks (1904-1989, Nobel 1972) while Clower was a Rhodes Scholar at Oxford in the early 1950s. (Oxford atoned for its error in 1978 by awarding a DLitt to Clower) By happenstance Clower and Hicks came to Birmingham in successive weeks, Nov. 2 and 9, 1962, to give talks to the Ashley Club. My diary entries record that Clower had "a smooth and effective delivery" whereas for Hicks I noted "delivery awkward and ineffective – a continual groping for words." Surprisingly, Hicks disparaged the increasingly quantitative approach to economics as practiced in North America. At one time Walters had mentioned to me that he was thinking of Hicks as my external examiner because in *Value and Capital* Hicks set out what became known as the expectations theory of the term structure. Needless to say, I am glad he chose Johnson whom he met on a visit to Chicago in 1967. That visit was probably initiated by Milton Friedman (1912-2006, Nobel 1976) since Walters was replicating for the UK some of Friedman's US work. Although Walters had voted Labour in the 1964 UK General Election, by 1970 he had joined the Monetarist camp and thereby started on the road that led to 10 Downing Street. (Walters' transition to Monetarism certainly occurred earlier than 1970 but I use that year because, courtesy of Walters, I have a copy of a two-page typed letter that Friedman sent to him on August 14, 1970. In that letter Friedman comments scathingly on Nicholas Kaldor's article, "The New Monetarism", (*Lloyd's Bank Review,* March 1970) using phrases such as "simple-minded Keynesianism" and "ignorance of what we have written.")

Like my PhD oral, my thesis also took an unconventional form. The term structure of interest rates was very much the topic *du jour* in the 1960s. At Chicago Douglas Fisher (1934-2017), Reuben Kessel (1923-1975), David Meiselman (1924-2014), Richard Roll, Lester Telser (1931-2022) and Neil Wallace were all doing research on the term structure at this time. At MIT two PhD students, Robert Shiller (Nobel 2013) and Richard Sutch (1942-2019), wrote term structure theses under Franco Modigliani (1918-2003, Nobel 1985). In 1964 Burton Malkiel, James Van Horne and John H. Wood completed term structure PhDs at Princeton, Northwestern and Purdue, respectively. Wood became a useful sounding board for my research when he took a lectureship at Birmingham for the years 1965-67 before taking a position at the Wharton School. My entry into this area occurred in the summer of 1963 when I came across David Meiselman's 1962 book *The Term Structure of Interest Rates* while browsing in the University library looking for a thesis topic for my MSocSc degree. A Ford Foundation prize winning thesis at Chicago, Meiselman's work had been recognized as a major innovation by formulation of a testable model of adaptive expectations for the forward rates embedded in the yield curve. I decided that testing the Meiselman model against British data would be a suitable exercise and to that end I collected an original data set from the archives of Pember and Boyle, the principal dealer in government securities in London, located on Princes St. across the street from the Bank of England. On the second day of my data collection visit I was invited to lunch with the partners in their private dining room. That room had a large mahogany table with a centrepiece of roses and two sideboards, one with a bank of telephones, the other with an array of liquor bottles. (The two essential working tools of British high finance in the 1960s?) The 1 pm lunch was preceded by drinks and lasted almost two hours. The waiter in livery made sure that one's wine glass never became empty. Looking at my first publication now I realize with horror that I failed to acknowledge the source of my data and the memorable lunch served up by the partners of Pember and Boyle.

Near the end of 1963 Walters suggested I stay on for a PhD, a suggestion that surprised me since my performance in the Master's examinations had been distinctly less than stellar. Stay I did and had I known then that John Grant, a Canadian PhD student at LSE, had already tested the Meiselman model on British data I might have changed my thesis topic and probably the trajectory of my career. However, Grant's results (*Economica,* v.31, 1964, 51-71) were negative so there remained scope for further work. Competition then came from Chicago in the form of their PhD student Douglas Fisher, who had come to the UK to collect data for a Meiselman type project. At this point I realized that I should try to get my test of the Meiselman model published as quickly as possible. Here Fisher had an advantage because I had been appointed as an assistant lecturer in Walters' Dept. of Econometrics and Social Statistics when my Commonwealth Scholarship ended in 1964. Although my teaching load was light, first-year teaching did slow down my research. In the end Fisher got there first with a paper that appeared in the August 1966 issue of *Economica* (v.33, 319-29). My paper, "Interest Rates, the Meiselman Model and Random Numbers", was initially scheduled for the December 1966 issue of the *JPE* but was delayed until February 1967 (v.75, 49-62). Both of us got positive results for the Meiselman model, although there was much in Fisher's paper that was questionable, including an econometric error that invalidated one of his principal conclusions. My eleven-page critique of his eleven-page paper was published in the August 1967 issue of *Economica* (v.34, 298-308).

My paper also attracted some fire. On May 23, 1967 Walters wrote to me from MIT as follows: "You have made quite a splash in Chicago. I was down there last week, and they obviously took your point very well indeed". I had three main points in my paper so this is puzzling. Apart from getting "nice"

Meiselman model results I also showed that one could extract plausible estimates of liquidity premia from the model's estimated parameters. Most likely though, Walters was referring to the last section of the paper where I demonstrate that the success of a Meiselman model depends critically on prior smoothing of the yield curve (hence the random numbers). Neil Wallace, however, (JPE, v.77, 1968, 524-27) did not take that point well. Nothing he says is incorrect but it is also beside the point. Robert Mundell (1932-2021, Nobel 1999), the then editor of the JPE declined to publish my response. When Mundell came to Edmonton in the early 1970s for a seminar, I queried him about this discourtesy but got no satisfactory reply.

Although I now had two publications in reputable journals, I continued with my term structure research. This work, comprising about 100 pages of analytical and empirical results, was completed in the summer of 1967 after I had returned to Alberta as an Assistant Professor of Economics. By the spring of 1968 I also had a note testing another term structure model accepted for publication in the *Review of Economics and Statistics* (v.50, 1968, 123-25). I bundled these 100 pages with my three publications and submitted them as my PhD thesis, a somewhat unorthodox hybrid of published and unpublished work but quite acceptable in the flexible British PhD system. This brings me back to my PhD oral and Harry Johnson's letter to Birmingham even before he had even asked me a single question. Publication of a scholarly work in a reputable journal is the benchmark for PhD level research and since Johnson, as editor of the JPE, was unlikely to think the JPE anything other than a reputable journal, sending a letter prior to the oral does not look like dereliction of professional duty. According to Alan Walters (letters to me June 21, 1968 and July 1,1968), Johnson sent a "glowing report" and recommended "a distinction". The 100 unpublished pages of my thesis yielded up two more full-length papers, one in the *Journal of Finance* (v.25, 1970, 809-818) and the other in the *Review of Economic Studies* (v.37, 1970, 396-406). The human capital acquired during my PhD research allowed me to produce three more publications in this area: a note in the *Swedish Journal of Economics* (v. 72,1970, 258-264), a comment in the *AER* (v.61, 1971, 229-230) and a full-length article in *Oxford Economic Papers* (v.27, 1975, 82-93).

There is one more anecdote from this period that needs telling. On March 29, 1967 A.D. Scott (1923-2015), an economist at UBC and incumbent President of the Canadian Political Science Association (CPSA), wrote to me with an invitation to join the editorial board of the newly established *Canadian Journal of Economics* for a two-year term starting in 1968. The journal was a project of the Canadian Economics Association (CEA) that came into being when Canadian economists decided to set up their own professional organization and end their longstanding affiliation with the CPSA. I can only speculate as to why such an offer would be made to a very junior member of the profession who at that point did not have a PhD nor any experience of refereeing. I see the not so invisible hand of Harry Johnson here. Johnson had been President of the CPSA the year before Scott and would have been instrumental in setting up the CEA and the new journal. Apparently, on the basis of one paper he decided that I was a promising young man with a set of quantitative skills probably lacking in some of the older board members. My own view of the matter is that Johnson overestimated the importance of my first paper. If I extrapolate backwards from 1970 to 1967 in Fig.5 of D. Card and S. Della Vigna (*Journal of Economic Literature*, v.51, 2013, 152), I find that the median number of citations of a paper in the JPE in 1967 is approximately 25. My 1967 paper has 52 citations, more than twice the median but certainly not large enough to make it a "must cite" paper.

<u>In the Trenches: Stories from the Research and Teaching Fronts</u>

Somewhere Paul Samuelson (1915-2009, Nobel 1970) once said that we write for the applause of our peers. And he might have added that we keep track of our peers by checking their citation record to determine how much applause we deliver. In the days prior to Google Scholar checking citations was a tedious process that required a trip to the Reference Room in the Library to pull from the shelf the *Social Sciences Citation Index*. By 1999 I had enough citations in the *Index* to be included in the third edition of *Who's Who in Economics* (Edited by Mark Blaug, E. Elgar, Cheltenham, UK). In the spirit of Samuelson's observation I will offer here a baker's dozen of my most cited papers and some associated stories. The number in parentheses on that list is the Google Scholar citation count for each publication as of October 2023. Do I hear the sound of one hand clapping?

1.(587) "The Likelihood Ratio, Wald and Lagrange Multiplier Tests: An Expository Note" *American Statistician,* v.36, 1982, 153-157

2. (406) "Evaluating the Linearized Almost Demand System" *American Journal of Agricultural Economics*, v.76, 1994, 781-793

3. (397) "Goodness of Fit in Generalized Least Squares" *American Statistician,* v.27, 1973, 106-108

4. (234) "The Bias of Instrumental Variable Estimators" *Econometrica,* v. 60, 1992, 173-180

5. (103) (with L.Lim) "Cubic Splines as a Special Case of Restricted Least Squares" *Journal of the American Statistical Association,* v.72, 1977, 64-68

6. (92) "The Cyclical Variability of the Size Distribution of Income in Canada: 1947-1978, *Canadian Journal of Economics,* v.15, 1982, 229-244

7. (76) "Expectations, Prices, Coupons and Yields" *Journal of Finance* v.25, 1970, 809-818

8. (76) "Testing Homogeneity in the Linearized Almost Demand System" *American Journal of Agricultural Economics,* v.80, 1998, 208-220

9. (71) "Goodness of Fit in the Seemingly Unrelated Regressions Model: A Generalization" *Journal of Econometrics,* v.10, 1979, 109-113

10. (64) (with H.Turtle and B.Korkie) "Tests of Conditional Asset Pricing with Time Varying Moments and Risk Prices" *Journal of Financial and Quantitative Analysis,* v.29, 1994, 15-29

11. (52) "Interest Rates, the Meiselman Model and Random Numbers" *Journal of Political Economy,* v.75 1967, 49-62

12. (50) (with W. Chan) "Invariance, Price Indices and Estimation in Almost Ideal Demand Systems" *Empirical Economics,* v.35, 2000, 519-539

13. (32) "Aggregation, Distribution and Dynamics in the Linear and Quadratic Expenditure Systems" *Review of Economics and Statistics,* v.74, 1992, 45-53

With the exception of #6, #7 and #11, all these papers can be linked to my teaching or the supervision of graduate students. This is clearly evident in #1 which was written specifically to provide an accessible graphical interpretation of the three tests and its subsequent citation in textbooks speaks to its usefulness as a teaching device. (I donated my academic books to the University Library but kept the textbooks that cite my work and they now make up what I call the Vanity Shelf in my home office) The motivation for #3 was similar. The standard treatment of Generalized Least Squares in textbooks did not provide a goodness-of-fit statistic, so I developed one. Ken Whyte of UBC, the originator of the SHAZAM Computer Econometrics Program, included it in his package as Buse's R^2. I extended it to the Seemingly Unrelated Regressions case in #9. In teaching the graduate applied econometrics course (Econ 599) I always included the literature on systems of demand equations and reading that literature produced #2, #8, #12 and #13. The Ag Econ tribe appears to have found my results helpful.

The genesis of #4 and #5 yields a couple of stories worthy of retelling. In 1980, an Iranian PhD student, Bakhtiar Moazzami, having spent some time adrift looking for a viable thesis topic, came to ask whether he could work with me. I suggested autocorrelation in simultaneous equations models because there were plenty of estimators with nice asymptotic properties but little knowledge of their properties in finite samples. The results of his thesis were published, but in lesser venues because some referees in the better journals thought the Monte Carlo design defective. Supposedly, everyone knew that the bias of an IV estimator of a structural equation increases with the number of instruments, citing Peter Phillips' article in *Econometrica* (v.77, 1980, 861-878). In that article Phillips had derived the exact distribution of the IV estimator and given an expression for the bias that depended on the number of instruments and a non-centrality parameter that was assumed to be constant. A few years later I noticed that the non-centrality parameter involved a matrix of reduced form parameters whose dimension must change as instruments are added, contrary to what everyone had assumed. It took me a few years to work out the implications of Phillips' error and whether the bias increases or decreases with the number of instruments depends in part on whether the instruments are weak or strong. There was nothing amiss with Bakhtiar's Monte Carlo design. He has had a successful career at Lakehead University and I have had my one and only publication in *Econometrica*.

My co-author on #5, Liat Lim, was a Commonwealth Scholar from Singapore who had come to Alberta to do a MEng in chemical engineering. During his two-year tenure of the Scholarship (1974-76), in addition to his engineering work, he took all three graduate econometrics courses (507,508,509) and received a grade of 9 in all of them (in our nine-point grading system of the time), a feat no other of my students ever managed. Although there were no PhD students in econometrics the following year, I agreed to run the Econ 608 PhD reading course for Liat and an academic visitor, W.C. Chang. For one of our weekly topics I chose a discussion paper by Dale Poirier of Toronto with the intriguing title of "Why the Almon Lag has gone to Pieces". Poirier proposed the use of cubic splines instead of Almon's popular polynomial distributed lag structure. (Cubic splines are segmented third-order polynomials with continuity restrictions at join points) During the discussion I suggested that the Poirier's proposal could be cast as a problem in restricted least squares but that conjecture required a formal proof which I assumed I would be looking at after term had ended. Three days later Liat arrived with a proof which required only a little tidying up before we published it in the Appendix to #5. He returned to Singapore where he was a pioneer of the firm Singapore Computer Services and retired as deputy CEO in 2003. Since then he has run a management consultancy that integrates concepts from classical Chinese philosophy, such as harmony, into Western management practices.

I exited the realm of research in 2003 when I published my last paper, "The Rise and Fall of Extraneous Estimation: Lessons from Econometric History?" (*Econometric Theory,* v.19, 2003, 78-99). This paper had a long gestation with a first version, "To Pool or Not to Pool? Why is That Still a Question?", presented at the 1996 meeting of the Canadian Econometrics Study Group at the University of Waterloo. The pooling of time-series and cross-section data goes back to the early days of single-equation demand analysis when a cross-section estimate of the income elasticity was inserted (extraneously) into the time-series demand equation to break the collinearity between income and price. The method was in common use between 1950 and 1980. I cannot recall precisely what prompted me to undertake this project. The idea may have arisen from reading Mary Morgan's *The History of Econometric Ideas* (1990, Cambridge University Press, Cambridge, UK) which gives an account of the origins of pooling that turns out, on closer examination, to be slightly off the mark. In addition to a thorough search of the literature I also checked the papers of Jacob Marschak (1898-1977) at UCLA and those of Richard Stone (1913-1991, Nobel 1984) at King's College, Cambridge. Stone was a prolific pooler but the method did not originate with him, as mistakenly claimed by C.L. Gilbert (*Economic Journal,* v.101, 1991, 288-302). This archival work established that the pooling procedure was independently proposed by Marschak in 1939 and Herman Wold (1908-1992) in 1940. (Wold gave a seminar to the Department in February 1978 as did Alan Walters in the autumn of that year, followed by Robert Clower a few years later) My analysis of the applied and theoretical work showed that theoretical refinements of the pooling procedure continued well after the technique had been abandoned by those working in demand analysis, which is to say that econometricians were sharpening tools that were never going to be used. Here I have to confess that I am one of the guilty parties because in 1981 I published "Pooling Time Series and Cross Section Data: A Minimum Chi-Squared Approach" (*Manchester Schoo*l, v.49, 229-244). Furthermore, as better econometric techniques for pooling were developed, they were rarely used in the applications. This suggests that the profession's gatekeepers, journal editors and referees, failed to be effective gatekeepers. The paper concludes with the following paragraph: *Whether this is a systemic failure in the culture of peer review is certainly a debatable question. Being in favor of better stopping rules is easier than implementing them. The interests of referees, the interests of authors and the larger public good (generating a better understanding of the economy) are not automatically aligned. Nonetheless, a useful first step would be a wider recognition that the problem may exist.* If this paper was my swan song, that song fell on deaf ears; it has received exactly zero citations in its twenty-year existence. T.S. Eliot has summed it up poetically for me, "*This is the way the world ends, not with a bang but a whimper."* (The Hollow Men, 1925)

I taught mostly Econometrics with Money and Banking my main secondary subject. In 1987 I received the Rutherford Undergraduate Teaching Award. The announcement included the following comment by a colleague: "He can expound the arcane innards of some econometric black box with thrilling precision and clarity and to students worth their salt this is irresistible" (Folio, April 23,1987). I suspect it was Brian Scarfe (1943-2017, DPhil Oxford, Chair 1977-1987) who was responsible for this hyperbolic assessment. If I lectured with precision and clarity that came about through hard work on my lecture notes which underwent revision in an ongoing process. Mel McMillan (PhD Cornell, Chair 1987-1997) became Chair that year and he was the first Department Chair to recommend to FSPC that Rutherford Award winners get double increments. I hope that rule still holds true today. I was not the first Economics professor to receive a Rutherford. Russell Pendergast (PhD Ottawa, 1921–1997), a chaplain at St. Joseph's College who taught Principles and Canadian Economic Development, was awarded the Rutherford in 1985.

Student evaluation of undergraduate teaching at Alberta began in 1968 when the Students' Union published a Course Guide which rated those professors who had agreed to have the course questionnaire distributed in their classroom. Its editor was David Leadbeater, a 1960s student activist, who later did an MA in Economics with us and taught at Laurentian until he became a casualty of that university's insolvency in 2021. I still have the 1969 edition of the Guide when 13 Economics courses were evaluated. On a five-point scale the best aggregate score of 4.24 went to Barbara Hodgins, a 1966 Honors Economics graduate, who taught Econ 305 (General Economics), a course open only to seniors who had not taken the first year Principles course. Bruce Wilkinson (PhD MIT, Chair 1972-1977) came second with a score of 3.94 in Econ 300, the full-year micro course. My 3.88 for Econ 404, the econometrics course, was above the Department average of 3.77 but Russ Pendergast fell below that average in Econ 305 (3.49) and above it with 3.90 in Econ 310, Canadian Economic Development. Over the years I have often wondered about "the noise" in student evaluations but sometimes that noise is easy to identify. In one of my Econ 341 classes I had three Business students who sat in the front row making a nuisance of themselves by talking incessantly. Giving them "the stare" did not deter them for long so I gave each a typed memo citing University regulations that gave me the authority to evict them for being disruptive - end of problem. The course evaluation comments had three identical remarks, "Worst teacher I ever had." I attached a copy of my memo to my annual report.

My Time in Tory

When I returned to Alberta in the autumn of 1966, the Economics Department had just moved into the newly built Tory Building, after spending eight years in North Lab where the Cameron Library now stands. That year John Delehanty (1933-2023, ABD (All But Dissertation) Minnesota, my hiking partner for some 40 years) and I were the solitary occupants of two of the six offices on the 9th floor (9-22 and 9-23), the other rooms being allocated to teaching space or fake claims to space by Political Science with rooms labelled Map Room and Joint Research. The southwest corner of the 9th floor housed a library jointly administered initially by Economics and Political Science and later by Economics alone. Margaret Howell, my excellent secretary, managed the library until the early 1990s when it was converted to a seminar room (9-15). The north-west corner was home to a computing lab furnished with Friden electro-mechanical calculators. That lab was short-lived as we soon switched to punched card input for the University's mainframe computer. Punch card machines for students' use were located on the main floor of Tory, a room also used by Computing Services for a twice daily pick-up and drop-off of inputs and outputs. As the Department's sole econometrician, I taught both the undergraduate and graduate courses and enrolments were sufficiently small that these courses could be taught in the lecture and seminar rooms on the 9th floor. For a few years the south-east corner served as a departmental coffee room. There was also a Faculty Lounge on the 14th floor where one could, in addition to coffee, buy a sandwich for lunch, but this agreeable amenity disappeared sometime in the early 1980s.

The four empty offices were soon occupied as this was a period of university expansion. Ken Norrie (PhD Yale, Economic History, Chair 1997-1999, Dean of Arts 1999-2001) was next to me in 9-24, followed by Balder von Hohenbalken (1932-2004, DrRerPol, Vienna, Mathematical Economics) in 9-25, Steve Lewis (PhD UC Santa Barbara, Macro) in 9-26 and Takashi Tsushima (1933-2001, PhD Johns Hopkins, Monetary Economics) in 9-27. Ken had Hollywood good looks and this assured him a steady flow of young women visitors, in and out of office hours. He left Alberta to be Provost at MacMaster. Although Balder, an erstwhile medical student and movie actor, had a doctorate in Political Economy, he was an excellent, and published, mathematician with an uncanny ability to think in n-dimensions. He had firm views on most topics and this firmness extended to the classroom where he would lock the lecture room to keep out latecomers. Sadly, he died in a freak fall from a movie theatre balcony. Steve returned to California in the early 1980s for a career at Sonoma State. In Edmonton Steve drove a snappy Rover 2500 and on Sundays in Santa Rosa he now drives a restored 1929 Ford Model A Coupe. Takashi shortened his life with an excess of alcohol. He took early retirement and I spent some time with him in Kawasaki City when I was on the program of the 1995 Tokyo World Congress of the Econometric Society. On the day of my departure he had two bottles of beer for breakfast. Let's call the drive from Kawasaki City to Narita airport interesting.

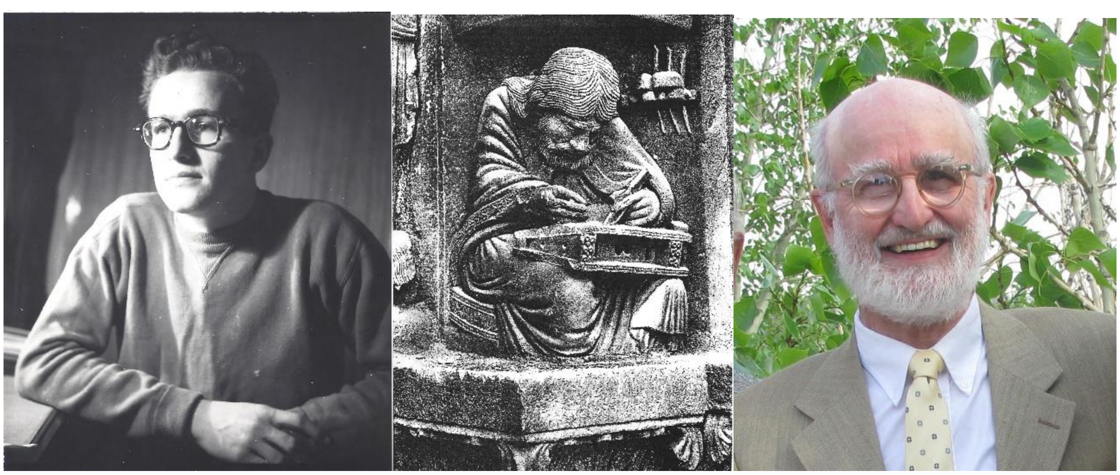
With the possible exception of Stephen Leacock (1869-1944) and Axel Leijonhufvud (1933-2022, "Life Among the Econ", *Western Economic Journal*, v.11, 1973, 327-337), the practitioners of the "dismal science" are not known for their humour, wedded as they are to the cult of rationality. But now and again our inner clown overwhelms our rational self and we engage in "stuff and nonsense." I once wrote a Capital Budget request for window and door alignment which read as follows: *The window and door in my office are in opposite corners whereas they should be located directly opposite each other. This lack of symmetry has been a source of considerable mental anguish to me over the last twenty years. Rectifying this structural deficiency would improve my productivity immensely. I take it as axiomatic that the University has some appreciation of the aesthetic point that symmetry is an essential element of beauty and without beauty our lives are shallow and unproductive. In the event that the vital point about beauty has been forgotten, let me remind you that Venus de Milo lost both arms, not one.* Brian Scarfe called it a "(h)armless joke." Our long-serving (1983-2005) and always thoughtful APO, Louise Edwards, replied tongue-in-cheek (Jan. 12, 1987) "Sorry we couldn't do this last year. Do you want to re-submit? PS Boy! Did you have me going!!"

The 1985 arrival on the 9th floor of Noxy Dastoor (PhD Essex), my occasional co-author and reliable dining partner with visiting econometricians, led to a quantum leap in the number of mock (and mocking) memos. When Robin Lindsey (PhD Princeton, a 9th floor resident famous for his cycling exploits in the mountain tunnels of Europe) decided to abandon us for UBC, Kathy and Doug West (PhD UBC, Chair 2009 - 2014) held a farewell party at their home where Noxy and I made him an Honorary member of the Brotherhood of Retired Econometricians (BORE). We presented him with a certificate and put to him the question all inductees must answer: Are you a BORE? The correct answer is "Yes, I am a BORE" To prove that we were equal opportunity econometricians I suggested at the time that those of the female persuasion should form the Sisterhood of Retired Econometricians (SORE) and we could join forces and become SOREBORES. On occasion students would add a bit of levity to create some *esprit de corps*. Sometime in the late 1980s the graduate students were selling University of Alberta T-shirts which listed on the back "THE TOP TEN REASONS I'M IN ECONOMICS." With my prior approval Reason #8 read "Always wanted a professor named A. Buse." The #1 reason read: "THE UNIVERSITY DIDN'T OFFER ASTROLOGY."

I took early retirement in 1997 but stayed on to finish some research projects and to be the coordinator for the Quantitative section of the Visiting Speakers program until 2003. Shortly thereafter I moved across the hallway to the Emeriti Office (9-12) which served as a convenient research base for a family history I co-authored with my brother, Dieter Buse. A copy of this history, *Our Grandfather's Axe: From Krumbeck to Canada by way of Poland and Russia 1756-1961*, is in the University Library. Anyone with a taste for European history might find it interesting. All told, with the exception of sabbaticals at LSE (Canada Council Leave Fellowship), New South Wales (SSHRC Leave Fellowship), Southampton and Victoria (four months), I made my academic home on the 9th floor of Tory for 49 years, surrendering my keys to Charlene Hill (the administrative assistant, formerly manuscript typist *par excellence*) in 2015. I occasionally visit Tory and walk the ninth floor to peer at nameplates and reflect on the passage of time. The disappearance of the oak-veneer doors and the battleship flooring during refurbishment heightens the disconnect between the past and the present. Or as the British novelist L.P. Hartley put it so aptly, "The past is another country, and they do things differently there."

Edmonton May 2023
(Revised October 2023)

A Triptych of Academic Life



1958 | 1962-2003 | 2010