

## A Jevonian Seditious: A Mutiny to Enhance the Economic Bounty? \*

I became an economist because I lacked major league baseball prowess. Midway through college (1934) I knew the athletic rainbow had faded. I recall no grief: the long vigil of the practice field was deflected to library pursuits. The Great Depression had already obsessed me, judging it first as a political machination until I apprehended the economic stroke. I read 'everything', in a 'random run'. Family-wise, we were 'moderately' well-to-do, with never the sear of deprivation — it took the army to often leave me penniless!

Only my Mother's insistence drove me to college: she would not countenance my minor league baseball plans. A modern battery of guidance counsellors would have cautioned against more schooling for I was obviously 'unmotivated.' So much for assessment by rote. I had always been a good student, challenged by any competition — this probably provoked me into athletics, smarting to beat the big fellows at their own game. I still remain dubious of the vacuous writings on "motivation," and "morale." I see these aspects as exaggerated; a pledge to do a job and a sense of duty have evoked my own discipline; the lofty banalities bore me.

I have long venerated courage as the vital human ingredient, precious and uncommon: surely 'intelligence' is not a rare trait in university circles. Loyalty, as an attribute, I would list second only to courage. Too, observation tells me that success is a function of perseverance, given some reasonable matching of talents and interests. A youngster willing to devote many hours to a musical instrument, to the dance, to athletics, has a transferable asset to bestow elsewhere if the original goals go awry.

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\* Contribution to a series of recollections and reflections on professional experiences of distinguished economists. This series opened with the September 1979 issue of this *Review*.  
*We regret the untimely death of Professor Weintraub on June 20 while this article was in proof.*

If you will tell me why the fen appears  
 impassable, I then will tell you why I think  
 that I can get across it if I try.

(Marianne Moore)

## My Second Career

For my second career, I confess to an original innocence about who hired economists. Academic posts had dried up; business economists were a wave off in the future. The new Deal was devouring economists, but I knew nothing of this. Yet it didn't matter: I just enjoyed reading about big issues. (Years later I was intrigued by the eminent G.H. Hardy writing that he was led to mathematics only because he liked it, and saw no other diversion for his time.)

College prepared me woefully; the courses were descriptive and too easy. But they freed me to live in the library. There were three fortunate encounters: first, Herbert B. Dorau, out of the fine University of Wisconsin faculty of the 1920s, taught *Value and Distribution*, as the advanced economic course was then called. Caught in the narrow textbook web spanning Marshall's Principles and Gustav Cassel's *Theory of Social Economy*, he elected the former, although I quickly read both. Even then I knew that I could move faster than the class convoy — I prevailed on Herbert to approve of my methods. He agreed, provided that I do a 'report' on the Marshall concept of *Organization* as a factor of production. I literally scrutinized 'every' book that antedated Marshall: it meant an early exposure to the history of economic thought. I came to absorb bibliography, with Viner and Rosenstein-Rodan as special idols.

My great debt remains with Professor Dorau. Ironically, he was probably a bit more conservative than Milton Friedman. But he thought me special, and always assisted me with money, jobs, or just kind words of encouragement whenever my personal tidal waves overflowed. There is a moral here for those who quickly disparage the integrity of unlike ideologies.

Professor Marcus Nadler was simultaneously at the top of his fame, lecturing on money markets to Wall Street brokerage house seminarians: he was lionized, and money marketeers esteemed his every opinion. Annoyed and then intrigued by some saucy and unconven-

tional replies — I found it hard to restrain some class humor even then — he decided I was 'different', and he invited me to edit his copious writings, including almost daily economic editorials for the *Journal of Commerce*, the foremost financial newspaper at the time. I quickly married the *Federal Reserve Bulletin*, and began chasing the money market blips — I have never since felt insecure on the basic data. The experience explains my policy writings which include operating practices.

My supreme mentor, by sheer accident, was Thomas Francis Patrick McManus. Out of Iowa, he was teaching in the New York suburbs and audited some classes I attended. Soon he gravitated to me as a promising student; he knew well, despite the eminent reputation of many of my teachers, that I was underexposed to the exciting trailblazers of the 1930s. He directed me to Robbins' *Essay*, Keynes' *Treatise* — whose 2 volumes I outlined, page after uncut page — Hayek, Dennis Robertson, Knight's *Risk*, and *The Ethics of Competition*. (I could later correct Knight — in conversation — when he wrote in a review article that he had never read Wicksteed!) There was also Wicksell, Von Mises' *Socialism and Money and Credit*, Chamberlin, Joan Robinson, and Schumpeter's *Theory of Economic Development* — still his most original book, in my view.

My appetite was voracious, as Way gave Way to Way. I hit the journals, clamoring after the *Economic Journal* and *Economica*. Little that was then published escaped my eyes; my compulsion was to examine everything.

I had even acquired copies of Pareto's *Manuel* and Walras' *Elements*. I must have been fairly unique among American economists in owing these works, including the Auspitz and Lieben text — pinched in due time by my son E. Roy.

The listing gives some clue to my intensity, and without a 'teacher' — I was lucky in this respect! I have always preferred to teach myself, including mathematics where I have sometimes been a slow student. But the good fortune has been in not being beholden — as graduate students are today — to 'teachers' who seldom permit a stray thought to an *apparachik* wandering beyond the doctrinal reservation. An insidious and subtle thought control pattern is asserted through recommendations, promotions, and in publications. And so, we reap a sad harvest in homogeneous half-truths.

My social life in those days was really non-existent; but I have made amends over the years for an early shyness. Two anecdotes may

hold some passing interest. At my Ph.D. oral I was that rare charmer who *picked* his questions, always inquiring whether the interrogator intended a deeper probe. If not, I refused to answer. Calming the fury, my savior was a congenital Keynes-hater who appealed to the committee to overlook my uncooperative behavior, that I was 'the best they had ever turned out'. I don't generally recommend this posture to Ph.D. aspirants but I was confident that I was better prepared than my tormentors, and trusted their intellectual integrity. I was naive!

Anyone delving into my high school record would stumble on an astonishing fact: I *failed* economics! But the transcript would not disclose that it was a self-inflicted wound, to abort premature graduation in order to captain a championship baseball team. In college, the only lowly D grade was scored in freshman economics! My train invariably brought me to class 5 minutes tardy, where I would shuffle under my dull instructor's eyes. Punishment: my A grade was converted into a D! Ever since I have distrusted bureaucratic 'rules'; students know that I delight in violating the organizational gems handed on by academic fiefdoms adept at concocting rules for every imaginable non-contingency. (Our Founding Fathers did better in devising our Federal Constitution). Despite these blots, confined solely to economics, I also established some honor achievements.

Early on I detected a need for mathematics training and, despite the gag barriers erected by academic advisors, I managed to go through calculus. During the war, serving as the butt of platoon jokes, I clung to the 2 volumes of Courant and one by Osgood. Fortunately, a math aptitude runs in the family; my brother, a Harvard Ph.D., did early work in topology, and was unscathed in military action with Patton's 3rd Army before he was cut down by a now remediable disease. My older son E. Roy is a well-established mathematical economist; a younger son Neil is also math-gifted though his specialty is as a financial advisor. I stopped the daily math exercises long ago, under other pressures, but I have never felt that it was a subject beyond my ken. Now physics, on the other hand....

### The London School

Largely under the urgings of McManus, I pooled my meager reserves to attend the London School of Economics, arriving on that famous October 1938 day when Neville Chamberlin alighted from

Munich, waving the piece of paper proclaiming "peace in our time." The English had been distributing gas masks, and mocking up the small Anderson 'shelters'.

For the period October to middle May 1939, crossing on the luxury ship Queen Mary (though I did not travel luxury class!) the total cost amounted to about \$ 900, including comfortable living in Mecklenburgh Square, about a mile from LSE; several faculty people, including the young Nicky Kaldor, lived in the park-like atmosphere later bombed out. Not many years ago I participated in a 4-day LSE conference; the cost well exceeded \$ 900. A footnote: when my expected monthly stipend was delayed I saw Kaldor and, as a brash student, I asked for a loan of about £ 20. He complied, unhesitatingly. I repaid the debt shortly, and as other of his friends will guess, he told me he had 'misplaced' the check, and would I draw another! Through the years, in sending him materials I always include the caution that he 'misplace' the item in a 'conspicuous' spot! These small 'failings' endear him to his friends.

At London I sat in on Lionel Robbins' famous lectures on *Economic Analysis*, which was practically obligatory. There was Hayek on the *History of Thought*, Kaldor on the *Theory of Production*, Durbin on *The Trade Cycle*, and a course in mathematical economics which it is most charitable to forget: the instructor spent his time *proving* the rules of differentiation. One seldom saw his face, only his back. Regrettably for me, Roy Allen was off in the United States so I was denied his infectious personality.

The LSE highlight was the Friday seminar chaired by Lord Robbins, devoted to topical issues such as devaluation, Keynes, employment policy, floating exchanges, etc. On the factual aspects I was abundantly prepared from my work with Nadler, and participated fairly actively. I know that Robbins, from words transmitted by Alfred Stonier and Paul Rosenstein-Rodan, rated me highly. The other graduate student who entered what was primarily a faculty fray was Tibor Scitovsky, displaying the same perspicacity revealed through his career. Watching Kaldor dazzle the group by his astounding gymnastics in his mastery of the speed engine in Keynes' *General Theory*, I knew the bell had tolled, destroying my vested attachment to the *Treatise*, to Hayek, Robertson, etc. I read the *GT* many times in quick repeated dips. Kaldor, by example, directed me to the trail that was to orient my thinking.

Other faculty seminar participants included Frederic Benham, who had published the important elementary textbook in economics, and

Frank Paish, always well informed on monetary affairs. There was George Schwartz, who later became an eminent journalist. Ludwig Lachman, with whom I was quite friendly, was also in the younger circle until he sailed off to the United States prior to a long and illustrious career mainly in South Africa.

I also attended classes at University College, mainly with Paul Rosenstein, getting to know him well in the student-teacher relationship; too, there was Al Stonier, and Hugh Gaitskell for ping-pong. I recall a lecture by Erik Lindahl, but my eyes were fastened on his young wife. Rosenstein would also bring in City people, to "explain" how markets worked. Savored memories involve Paul Rosenstein, hosting lunch at unaffordable places (for me) in the Soho on several occasions. I still recollect Paul, with awe at his language command, in the *History of Economic Thought*, thumbing through and citing continental economists to impart a cosmopolitan flavor to the evolution. My admiration has not wavered despite my teaching in the subject from original sources for over 30 years. Robbins would refer to Paul as the man of "total erudition," a well-merited accolade.

### The London-Oxford-Cambridge Seminar

Frankly, my uneven preparation inhibited me from reaping the full reward from the LSE lectures. A few incidents stick out. Dennis Robertson had just arrived from Cambridge to assume the professorship vacated by T.E. Gregory who departed to become advisor to the Indian Reserve Bank. Robertson's collision with Keynes was common knowledge. The course he inherited from Gregory involved about 10 lectures on American Banking, emphasizing institutions, practices, data. I had fallen into the habit of talking with him about the exciting New Deal reforms, even handing him some condemnatory pamphlets by the pompous self-styled *National Monetary Economists' Committee*. Despite his dissents from Keynes, he dismissed this crew (correctly) as a bunch of disgruntled preachers.

In any event, after my many amendments up-dating his lecture notes, he gently asked if I would not mind giving about half of the lectures! Flattered, I can advance a unique claim as Sir Dennis' lecture caddy!

I vividly recall a meeting of the Political Economy Club, to which Lionel Robbins invited me. The respected E.H. Phelps Brown, whose book on *The Framework of the Pricing System* (1936) did so much to promote general equilibrium theory, was giving a paper, assisted by George Shackle, on (roughly) "The British Trade Cycle." The dominant theme shied away from monetary theses, stipulating that in the mid-1920s the British recovery was sparked by the export trades. At question-time I ventured that his paper did *not* preclude a monetary explanation for, in a wider frame of reference, the Federal Reserve Bank of New York had actively assisted, through easier money, England's return to the gold standard. Quickly there was a commotion — Hayek, Hawtrey, Robbins stoked the controversy and I won smiles as some sort of 'bright lad'. I hope Sir Henry has forgiven my audacity for he has always been one of my favorite economists admired, because of circumstances, mainly at a distance. Not long ago I corresponded with him, offering my assessment of Sir Roy in the United States, to fill in Sir Henry in writing his brilliant memoir to my irreplaceable friend, Roy Harrod. Shackle, the most thoughtful of economists, must long ago have forgotten the incident; he is a treasured 'pen pal'. I also remember meeting him, and Erwin Rothbarth — a brilliant young man who was to die in the futile paratroop assault on Holland — at about that time.

London, obviously, was exciting, not for LSE and the University College but for its cultural atmosphere. A pall hovered over the city the day that Hitler marched into Prague. My escape was a very rare afternoon sad visit to the 'flicks'.

Undoubtedly the chief vehicle for graduate student interaction was the round-robin London-Oxford-Cambridge (L-O-C) seminars, organized at the London end by the painstaking Andrew de Neuman. He read widely but his expertise was on the Polish coal industry and, after a scary wartime escape in peasant disguise from Poland, he aided the British ministries with his accumulated knowledge. Andrew was to become a prolific art collector until a premature death in the 1950s. Through him I became quite friendly with Oskar Lange, then visiting London after settling some family affairs in pre-war Poland. Too, I remember Ernest Doblin, later to do the initial UN national income accounts.

I gave an interminable L-O-C seminar before Piero Sraffa and the Cambridge people, meeting David Champernowne whose reputation already extended beyond his years. My topic was "The New Deal Legislation," a subject of fascinating interest despite British vagueness

on details. The audience survived, and I walked to the railroad station with Dr. Bruno Foa, an economist well-known to Italian audiences — who became a lifelong friend. I thus count the seminar as a huge success.

From the L-O-C seminars I came to know “all” the graduate students or teaching assistants of my own age group in England: I remember the young Arthur Lewis, and Tom Wilson starting his fine dissertation study of the trade cycle. There was A.J. Brown, at Oxford, measuring liquidity, etc. Conversely, on my return to the United States, I literally knew nobody in my age group. There is a moral: it involves some plus and minus aspects of graduate school associations.

Shortly after I arrived at LSE I had my initial graduate student ‘conference’ with Professor Robbins. I told him that I was translating Pareto’s *Manuel*, and that I had completed 122 pages. His advice: desist, for Mme. Pareto would ask outrageous royalties — Pareto’s famous *Mind and Society* volumes had just recently been translated and published. I put the *Manuel* aside; it was not to be available in English until 1971. Perhaps I would have botched the job but its presence would have been hastened.

I also proposed doing a *Theory of Diagrammatic Economics*, for indifference curves, the production function, and elasticity of substitution concepts had just recently poked through, not to mention Hayek’s “structure” of capital. Robbins judged my intentions as an undue focus on one ‘method’ although I only contemplated merely some update of the long forgotten Henry Cunynghame, *Geometrical Political Economy* (1904).

Although cold water had been poured on me, and by a wise man, in fairness Robbins was always a model of politeness, courtesy, and considerateness in many encounters. But the negative reaction stiffened a resolve of *never* to query others on what I would study or write: it is best to commit your own errors of judgment.

### Price Theory: the Start

Stripped of Pareto I began a draft of my “massive” Ph.D. thesis on “Monopoly and the Economic System.” My private compass confused me: to write on monopoly I thought I had to investigate competition

and all the formalisms connected with it. Seated daily at the same corner window spot in the LSE research study room, occasionally browsing through popular articles, I decided to try my hand at writing them. In short order I wrote 3 pieces, sold all, and the extra pounds was Manna from Heaven; one article was even reprinted, detected by a friend, and after a protest I was paid again. I even harbored the suspicion that I might make it as a journalist in London, if all else failed.

I did see, but not meet, Keynes on the occasion of at least one meeting of the Royal Economics Society. But I was to know most of his economics intimates over the subsequent years. My close friend, Asik Radomysler, would nudge me, to refer to Keynes as “The Tiger.” I still recall a “cute” *Coronet* issue — it juxtaposed Keynes and a Pekinese dog facing on opposite pages!

So I did my monopoly dissertation, attended classes, enjoyed my friends, and loved London — a sentiment dimmed only after too many of my London friends died. Returning home, bereft of a job, having a few contacts, it was McManus who performed the pivotal rescue with a teaching post at St. John’s University where I worked for the most decent supervisory Dean who I was ever to encounter.

### Early Publication

I taught anywhere from 15 to 27 hours a week — yes, the numbers are accurate. Still, I found the energy to devote time to my own writing, though it often meant I would work all night. The nocturnal habit still persists though the pace has slackened after a decade of illness. Of course I am a bit deaf to complaints of the modern generation on the onerous nature of a 6-hour teaching schedule.

Again, to try my hand as an unknown author in my own country, I wrote on “Inflation and Price Control” in 1940, and “Compulsory Saving in Great Britain” in 1941, the latter dealing mainly with Keynes’ views on war finance. The publishing angel was the *Harvard Business Review*; the former item contains my early recognition of money wages in inflation, treating it almost casually, presuming “everybody” understood that from Keynes.

On the strength of these publications Walter Salant invited me to come to Washington; his intervention fostered an urgent telegram to

work for the Treasury after Pearl Harbor. It was the usual bureaucratic game of 'hurry over and wait for some work to turn up'. I have always reserved a special warmth for Walter Salant — a profound early analytic disciple of Keynes — for his civilized display of objectivity in evaluation. I was, alas, misled by his example, for it was not to be repeated very often: the old boy network has been alive and well in the United States, in innumerable self-serving subtle ways. Economists go to great lengths in cementing good ties rather than in respecting good work. I can now bring some objectivity, and scorn, to the practices; they mock the pretention of a 'scientific' community.

My first theoretical piece, on "Price Cutting and Economic Warfare," in the *Southern Economic Journal* appeared in January 1942. I still think the analytic probe goes beyond Martin Shubik's renowned 1959 "Market Structure" which seizes only on financial strength, ignoring cost advantages. Manifestly, my article broke with the neat "equilibrium" solutions to duopoly. About 15 years later I was to find ample confirmation in a timber study for the U.S. Forest Service.

In the Fall of 1942 I had practically simultaneous articles in the *American Economic Review*, the *Quarterly Journal of Economics* and the *Journal of Political Economy*, an uncommon feat I suspect. The former dealt with the "Foundations of the Demand Curve," from which Friedman was to borrow references and then take an incredible bite at me over a point for which *his* mentor Henry Schultz was culpable! A mite unfair — but I write often enough to play the game.

The article was also the first that Paul Samuelson had refereed for the *AER*: he deplored my *not* reading Triffin. In the ensuing tiff I retorted that the *QJE* had accepted a long critique of Triffin in which he generously conceded the burden of my thrusts at his meritorious book: to this day the monopoly-pure competition dichotomy remains an obscure mystery despite the modern generation's resolution by default.

The *JPE* article, accepted graciously by Jacob Viner, was titled "Monopoly Equilibrium and Anticipated Demand," involving both objective *and* subjective demand *and* cost curves. Silence still reigns over the subject though Robert Clower was to make use of the thesis some years later, in the *Economic Journal*. Surely with the modern baptism in "expectations", the subject remains lively. I still stare in disbelief at mathematical economists assigning priority for "subjective" demand functions to Negishi — who was a distinct late starter. I go into shock on noting subjective curves injected into general equilibrium systems by

authors not even apprehending the potential for indeterminateness. The matters remain significant — perhaps our profession will turn to them in 2010!

These articles, an unusual accomplishment for a Ph.D. trained outside our oligopoly mills, were written alongside my thesis revision, now edited by Horace White, Jr., an able economist whose father owned the Blakiston Press. Horace was the blithe spirit who inaugurated the *AEA Readings* volumes. He was about my only friend in my age group with wide professional ties. It was a grave loss for the profession that he drowned in our first troop transport sinking, en route for a State Department mission. My book literally went down with him. Returning after nearly three years of military service I was not amused by a famous economist almost bragging that the book manuscript sat in his file cabinet "protected from the exigencies of war": there is ample evidence that he pecked at it. If the book surfaced in 1943 or 1944 as planned, it would have been on the heels of the fresh texts of Boulding and Stigler.

### Bureaucracy and War

I was a total misfit at the United States Treasury, not only because of the partisan ideology there, but because I was too absorbed in my own work. I transferred to the OPA very briefly, where Walter Salant urged me to work on postwar monetary reconstruction; his insights were magnificent but my priority was military service against Hitler. For about the two months stay, my desk was adjacent to Jacob Mosak and Paul Baran. Still frustrated, I left for New York to edit the *Monthly Review* for the Federal Reserve Bank — and found the atmosphere in the Domestic Department stifling; in the foreign sector there were the budding stars Arthur Bloomfield, Frank Tamagna, and Henry Wallich. The Bank was at ideological loggerheads with the Treasury. I occasionally drafted a tentative Open Market Committee position paper for the judicious and influential John Williams. But I was aghast at the childlike responses to my exercise of my assigned editing job; there would be protests for changing, say, a 'however' to a 'thus'! I vowed never to behave in the same way. My attitude carries over to book reviewers; I had observed some traumatic effects on friends who grieved over an unkind word.

I quickly established my personal independence at the Fed, which displeased some moribund executives who were harlots for the big City Banks. But all the time I was chafing for military service, to join an older and younger brother. The 'invitation' came on the very morning E. Roy was born. Thereafter, despite some desires to defer me, I insisted on being in service.

On correlation analysis, I claim I won the war: the logic is impeccable for until I entered the military we were not winning; as soon as my training was complete, the tide turned. So much for correlation. My military 'career' involved mainly being at war with our army. My wife never understood why my officers were too leery to court martial me: to get me out of sight they sent me on 'Detached Services', which was very nice. My problem was that I felt my grasp to be equal to that of the generals — and higher ranks.

I felt keenly that the war against Hitler and his evil works was the good cause. Needless to say I am underwhelmed by those who wrote paeans on "freedom" but who evaded danger in the one great trial in their lives. Freedom is more than a vague word: I have never abandoned the concept of freedom as the solitary preserve of reactionaries.

Ken Galbraith has never appreciated his good luck in my rejection of overtures to join the Strategic Bombing Survey. His tribulations with Paul Baran would have been magnified.<sup>1</sup> A bizarre note: during the battle of the Bulge, with our military flying in everything, and handing a rifle to anyone who could hold one, I was flown back to London for 'intelligence' work. Once again, the bureaucratic waltz; it was not unpleasant playing softball in Hyde Park amid the occasional V-2 bombs.

I was relieved from active duty on Friday, Christmas Eve 1945, returning to my wife and child after nearly a 3-year parting. I vowed to relax for several months; the pledge lasted from Friday to Tuesday when I went job-hunting; not wanting to go near the Fed I scampered home with two offers. I was ready to resume the life that had been interrupted by an open-ended 'jail' sentence.

<sup>1</sup> See his *A Life in Our Times* (Houghton Mifflin, 1981).

## Career Resumption

I remember talking in London with Tibor Scitovsky, in uniform, on the obstacles in restoring the professional pieces. Within days I decided to try my hand at an article, thus my "Monopoly Pricing and Unemployment" in the *QJE* (1946). (There are more references to my articles in Chamberlin's separate bibliographies than to any other author.) I still regard the unemployment article as having merit, despite its defect of assuming the income level constant. The premise was to haunt me for many years.

My *Price Theory*, an extensive revision after the ordeal of my earlier monograph, appeared in 1949. I guessed that the very generous publisher's review of the book reflected the genius of Kenneth Boulding. In concert with my sentiments for Walter Salant, I have always been grateful for his kindness to an unknown. Curious, I am still able to single out these people for objectivity; the trait is not widespread.

The book was widely praised. I still think there are unexploited sections dealing with multiple markets and multiple time periods, lost in a 'textbook.' Too, I was doing some linear programming in words! Writing in *Kyklos* in the mid-fifties Thomson Whitin, pioneer of inventory theory, reviewed it belatedly, to invite attention to its 'considerable contribution to price theory.' I had not known Tom, but he had been offended by some silly AEA remarks of a 'prominent' economist. This was another generous, wholly unsolicited, act in the small list, to praise a virtual unknown, that sticks in my mind.

After *Price Theory*, the intermediation of Joseph Dorfman — a dear friend in my New York days — Simon Kuznets, and Raymond Bowman, landed me at the University of Pennsylvania. Considering our diverse techniques, I have always teased Simon Kuznets about his wanting a compatible cigar smoker as a colleague.

## The Pennsylvania Years

Countless drafts and the better part of a decade, with time out to win a war, went into *Price Theory*: I might still be rewriting it if my wife had not balked: I felt denuded in handing it to a publisher. To transmit the income concepts then coming into vogue, and because of an unflattering

reference to a demagogic priest in the one U.S. title extant — I was teaching at a Catholic University — I took just a summer to write *Income and Employment Analysis*. This, too, was fairly well received, with the printing exhausted within the year. But as textbooks were now regaling students with the income parables, I abandoned any thought of a revision. Too, my interests shifted as I joined the Penn faculty.

I wrote on "Consumer Monopsony" for the *Review of Economic Studies* (1950), an article that delves into the constant marginal utility hypothesis. While the inspiration was undoubtedly Hicks, some of his demand *Revision* overlaps. I hit hard, in early 1953, at the pernicious Wall Street ordained Eisenhower monetary policies, and did a 1955 critique of the Federal Reserve 'bills only' policy. The latter evoked a treasured, personally penned, letter from Allan Sproul, the great President of New York Fed, thanking me for being an academic who illuminated the issues.

I had omitted income distribution from my *Price Theory*, dismissing the orthodox marginal productivity theory, which assumed full employment, as a chicanery I could not condone. Too, I had thought that the Washington wartime analysis of the 'Inflationary Gap' was evasive, and that the AER debate between Walter Salant and Milton Friedman had taken refuge in microtheory to resolve a macro issue.

My *Approach to the Theory of Income Distribution* was conceived in this spirit; the writing tormented me for over 8 years, for there were no useful precedents. It remains my most original book despite some glaring (smaller) errors. A functional macrotheory apparatus gives it a distinct structure — I think.

Earlier, it prompted a lead article in the *AER* (1956) on wage theory, and one in the *EJ* (1957) on the microfoundations of aggregate supply. D.H. Robertson generously wrote me that it was 'lucid and definitive', and that thenceforth he would not return to the subject again. I confess that I have lost or misplaced his letter, along with the one by Sproul, in my move to Canada, though they were my prized possessions. These articles gave exposure to my methods on Aggregate Demand and Supply; curiously, the AD function generated more puzzles than the interdependent supply relation. Some 20 years later, with supply-side economics the mode, Lawrence Klein cited these pieces in his AEA (1978) presidential address.

Strangely, in Keynesian quarters, where it should have been welcomed, my *Approach* was disparaged: typically, G.L.S. Shackle was the lone exception. One review by a very 'eminent' Keynesian ridiculed its basic posture — and some 11 years later lifted its crucial features

wholesale — and without the magnanimity of even a reference despite parallel passages where I had clearly anticipated his 'fresh' conclusions. My opinion remains that there are many uncovered points still hidden in the book, not least its theory of profits — a central subject on which neoclassicists, from Walras on, bare the vapidness of their models.

I began to perceive the dominant position of money wages in the *Approach*. Shortly after it was published, I was smitten by its quintessential nature in developing the theory of the price level, starting from a simple truism: I thought then, and now, that the only effective reply to the Quantity Theory of Money was to advance a better truism than its Equation of Exchange, and with a better 'constant' than money velocity. So, in *A General Theory of the Price Level*, my Wage-Cost Mark-Up (WCM) equation was born. In origin I owed nothing to Kalecki; it was only after I found the wage *share* (or price mark-up) "constant" that I recalled, and referred, to his work. I resist differentiating my product too much from Kalecki, but its analytical and empirical coverage are *not* 'identical.'

My *General Theory of the Price Level* was written in 4 days, with 14 days elapsing from conception to publisher, for I had to await the statistical data. I can hardly claim it to be the best economics tract ever written, but there are few competitors for a 4-day stint! Amusingly, one 'talented' reviewer opined that 'one equation and one constant hardly make much of a theory.' But: how many books have one major equation, and isolate the unmatched 'constant' in economics? Playfully, I 'turned the other cheek' in assessing the reviewer's work, long since lost in the stacks.

My *General Theory* also injected a policy stance, named the "Watchtower Approach." I spent the summer of 1959 in the office adjacent to Walter Heller, and suspect that the Kennedy *Guideposts*, except to a legalist, were borrowed from me without any recorded expression of indebtedness. Indeed, I marvel at the lack of a single protest by any of the *Guidepost* 'authors' to disarm Lerner's harsh (1960) review. In subsequent Heller staff meanderings, there still remains some monumental confusion on the theoretical impacts of fiscal, monetary, and money wage policy.

I mentioned the 'sarcastic' Lerner review. Abba apologized to me on several public occasions, and even more in private meetings. At all times I respected his integrity, and delighted later in his frequent company, with his beloved wife. His death leaves a deep personal bereavement.



Much of the 1960s were devoted to other varieties of incomes policy; the thought never left me; others might see it as an obsession. It was over this period that Ken Galbraith, on his own initiative, commented generously, in correspondence, over some of my policy writings. If ever I had some twitters of personal depression it was in the 1960s, and they were relieved by his magnanimity. I admire him most among contemporary economists despite his sin of writing lucidly, and hitting the best seller lists. I am not aware of any economist with an equal sensitivity to an idea, or the skill to distill and synthesize diverse thoughts. I am proud to know him, and admire him from near and afar.<sup>2</sup>

Leaving Penn for Canada — there was boredom and some tax advantages — I quickly built up a powerhouse department at Waterloo. Most importantly, this was a productive time: I wrote 18 articles in the 2-year sojourn. Though I will spare the details, Tax-tied-Incomes-Policy (TIP) was born during the Canadian stint. From unrelated correspondence, I put it into Galbraith's hands by relinquishing a weekend of golf. Ken was enthusiastic, insisting that it must get into the public domain with all dispatch. Within 2 months *Lloyds Bank Review* (January 1971) gave it world exposure. With Ken, I can never thank Len Silk amply for devoting several *New York Times* columns to TIP. This peerless economic journalist scooped many silly journal editors that I can name. The Wallich-Weintraub collaboration followed a luncheon arranged by Len Silk at *The New York Times*.

Much subsequent work has gone into refining and publicizing TIP, and rounding out my theory of the price level, which has become a respectable competitor of the Quantity Theory — the early derision has long expired. My opinion? It is destined to replace the Friedman reversion to David Hume despite the hordes at central banks, in journals, on prize committees, and in head counts in economic departments. Truth will out — in the neoclassical long run. Nonetheless, in stressing the wage level for prices, I have not neglected the special clout of money on jobs and production.

More recently, in apprehending the importance of the wage share, I have 'generalized' the Kalecki-Kaldor-Robinson proposition of 'wage-earners spend all, capitalists save all.' The little extra touch opens up

<sup>2</sup> For the record, the very successful *Journal of Post Keynesian Economics* was founded when, in agreement with Paul Davidson, I wrote Ken of the need. He agreed instantly to underwrite the necessary financial support for starting up operations. I am aware of other unsung deeds performed by him.

whole new avenues in simplifying and exploring economic theory. From this work macrotheory can be provided with the theory of income shares that Keynes by-passed. The wage share enters the theory of: (1) employment, (2) nominal and real income, (3) the price level, (4) real wage determination *sans* marginal productivity, (5) the average propensity to consume and (6) aggregate and consumer sector profits.<sup>3</sup>

These are tall claims but some recent and forthcoming publications should sustain them. Others, I am well aware, will be the ultimate judge.

For the record, Bertil Ohlin, was adamant that I visit him at his apartment in Stockholm in 1976, for he had a cold and could not attend my lecture. Reason: he wanted to tell me that "you have added to Wicksell." In mind, were the passages on the Clock-Time Sequences, *à la* Dennis Robertson, in my collaborated article with Paul Davidson (*Economic Journal* 1973) on "Money As Cause and Effect." Ohlin could apprehend this, yet others (like Leijonhufvud) are so myopic in assessing Wicksell as to be unable to recognize an idea staring at them.

### The Quantitative Record

I have written from 17 to 21 books, depending on some 'classification' problems. I have published 100 articles in professional sources, and over 200 in more popular outlets. So there exists, one way or the other, over 325 signed pieces.

I mention the numbers for one reason: I myself typed maybe 85 percent of the pages involved. Except for brief interludes I have lacked secretarial or research assistance.

I admit to no gratitude to the university, for administrations have never tried to lighten my burden, in contrast to their fawning over the perennial grant-solicitors.<sup>4</sup> I have abstained from such beggary after one rejection; I am simply not an establishment darling. I say this without self-pity, remorse, or regret. But it is a clue to how the 'research' belt has been perverted, saturated in self-serving inequities. Beneficiaries "love" the system; rationalizing pieties abound.

<sup>3</sup> Cf. my "An Eclectic Theory of Income Shares," *Journal of Post Keynesian Economics* (1981).

<sup>4</sup> I exempt Andrew Postlewaite who made the first gesture recently in 33 years.

### Travels and Friends

I have been lucky in having had the opportunity to travel, in Europe, Australia, Asia. I have visited about 30 countries and lectured in over 250 places, sitting in over 175 airports. I once lectured in 17 cities in 37 days, in France, Ireland, Spain, Germany, feeling very much like a presidential candidate, scarcely recalling at which hotel I slept.

The travels have made the teaching and paper grading rewarding. Likewise, I have been fortunate to have had as a usual travelling companion my wife Sheela, and as a debating companion and formerly friend the noblest figure of all, Roy Harrod; too, there was always comfort in our joint association with the late Abba Lerner. The Penn visitors included Ivor Pearce, Miles Fleming, Francis Seton, Joan Robinson (at Waterloo), Nissan Leviatan, Haim Barkai, Ephraim Klunman. My one true rock of support among friends since 1942 has been Arthur Bloomfield. Paul Davidson has climbed from student to close friend with rare analytical gifts. In recent years I have revelled in my Italian associates. But I must stop with this brief listing.

### The State of Economics

As I approach retirement I cannot suppress an oracular litany on the state of graduate economics education in my own country. Except in the length of reading lists, it is indistinguishable from undergraduate training. As a long-time chairman of the graduate program, living by its pseudo-democratic rules, I have always been amazed by the staff presumption that a student *must* take their course or remain *uneducated!* Hence the proliferation of courses, differing mainly in title rather than content: it is as if a barricade is built to forbid a student to acquire any learning on his own; the joys of exploration are forbidden fruits. Individuality is beaten down. Personally, I would have rebelled at the contrived hurdles.

I have long taught the *History of Economic Thought*, mainly because there was only *one* other person (Bloomfield) qualified to teach it, and he was often otherwise engaged. Using only original works, few students bother to read them: for they are too preoccupied in grinding out a coefficient for econometric models. Most of the economics faculty,

sharing the same innocence of our past, have wanted only *pro forma* compliance to the study. Opposition votes favoring darkness abound, with the course retention serving as some window-dressing.

I am hardly a Marxian scholar yet, worst scandal of all, it falls to me to examine the occasional serious student who elected to do a field exam in Marx for the Ph.D. Too, I have been the department "radical," about as 'dangerous' as Herbert Hoover. This speaks volumes about American economics faculties.

### The Chief Educational Failing

In my own mind the chief failing of our graduate schools is in keeping students under lock and key, and of exploiting them to write on subjects of interest primarily to their mentors, and asking to squash independent thought. And this holds, I fear, at most of our Ph.D. factories, where students are beholden to teachers for their intellectual capital, for job recommendations, and even publications. I cannot recall an article out of the major doctoral mills criticizing their mentors — for anything of substance. Of course, a handful of students have stepped into these citadels of learning, and have been turned off by the teachings. But these are 'rare' cases. Small wonder that our Ph.D.'s resemble peas in a pod, retaining their own institutional biases, without a paradigm's difference between them. We need look no farther for the bankruptcy of American economics, barren of any novel formulation or original policy phases.

It is the 'elder statesmen,' with noble incantations in praise of independence, and practical adherence to intolerance, who bear the blame. I know of no easy remedy. Ivor Pearce once proposed that we shoot every economist who becomes influential! This, undoubtedly, is too drastic and unconstructive a solution. But the problem remains.

After my long career in economics I have become dubious about our professionalism, and remain amazed at the constrained desire to alleviate poverty, the Marshallian objective. Economists, however, surely unlike physicians in medicine, *prefer issues to solutions*; they enjoy philosophic debates without ever deleting sterile prescriptions: the revival of 'dead' fish is an occupational pastime. Ridiculously, despite colossal blunders by our most decorated luminaries, none of them has

ever admitted to a 'mistake.' Truth is the unfailing victim. Our chaotic economies bear testimony to the price being paid to sate our ego.

Obviously, I think that our profession is loaded with sheep. If I have been a bit different, it is because intuitively I somehow borrowed from some passages in Jevons about *The Noxious Influence of Authority*. Jevons wrote:<sup>5</sup>

"... authority has ever been the great opponent of truth. A despotic calm is usually the triumph of error. In the republic of sciences sedition and even anarchy are beneficial in the long run to the greatest happiness of the greatest number."

I think I have abided by this creed, writing, behaving, without intimidation. If I have often railed in mutiny at the Establishment — which exists — I think the cause has been just, namely, to enhance the economic bounty and to iron out its division. The Keynesian complacency, and the monetarist failing, in the modern stagflation tango has only added zest to my own seditious assaults.

### The Future?

My life can be summed up as one of teaching, writing, writing, background music, tennis, golf, travelling, desserts, and many close associations, including a happy marriage. Would I do anything differently? Yes, on leave in 1957 I regret not spending the time with Joan Robinson and Nicky Kaldor, for I was doing some parallel work.

As I face retirement I still enjoy writing. I take comfort in an impression that my forthcoming book will be my best, regardless of how others ultimately receive it. Optimism costs no more, and is psychologically less of a strain on yourself and others, than pessimism.

*Philadelphia*

SIDNEY WEINTRAUB

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<sup>5</sup> W.S. JEVONS, *The Theory of Political Economy*, (5th edition), Chapter VIII, concluding remarks.