

“The Passing of the Guard in Economics”

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This is a lucky audience. When I accepted the invitation to give the present lecture, my first temptation was to choose for a topic *A Sweeping New Non-Substitution Theorem Valid for Technologies Lacking Primary Factors of Production*. How did you miss that dire fate? By chance I remembered what Raymond Goldsmith said three decades ago when the two of us walked out at the conclusion of Arthur F. Burns' *AEA* Presidential Address: “Let this be a lesson to you, Paul. When you come to give yours, don't impose on a captive audience at the end of a long day a complex scientific discourse.”

Therefore, I have chosen to talk tonight about the great generation of economists who dominated our science in the 1930–1980 half century. You know their names: Ragnar Frisch and Jan Tinbergen; Joan Robinson and Nicky Kaldor; Abba Lerner and Oskar Lange; Harold Hotelling and John Hicks; Tjalling Koopmans and Jacob Marschak; Frank Knight and Jacob Viner; Piero Sraffa and Wassily Leontief, Simon Kuznets and Michal Kalecki; James Meade and Roy Harrod; Gottfried Haberler and Friedrich Hayek; Erik Lundberg and Bertil Ohlin; and many others just as colorful and original as the names I have happened to mention in this helter-skelter ordering.

Not all on my list are dead. A living scholar could make the cut by being over the age of 80. (That goes the *New Palgrave Dictionary* one better: to be embalmed in its pages, you had to be dead or merely 70. Even I was eligible.) Have I committed myself to the view that scholars over 80 have already run their race? That is not my intention: Hicks, Tinbergen, Leontief, Haberler, Hayek, and Meade—at this very hour that we are meeting—are busily engaged in the creative destruction that is called scientific productivity.

What do these various names have in common? All were born after 1885. Most are not American, even though the triumphant rise of American economics after 1940 was enormously accelerated by importation of scholars from Hitlerian Europe. In addition, as we say in Heckscher-Ohlin trade theory, free trade in ideas is a powerful substitute for free trade in the productive factor called professors. How very much we American economists have learned from studying the writings of Frisch, Robinson, and all the rest who resisted calls to cushy American chairs.

Some of these named economists have received the Nobel Prize. Most have not. In my judgment every one deserved that award or higher. Each time when a Kalecki died, or a Harrod or Robinson, or Blankety-Blank, it made my blood boil that the Committee in Stockholm had missed doing its duty. It was small consolation to realize that Einstein got his Nobel late, and that Tolstoy was passed over, so to speak in favor of Pearl Buck and John Galsworthy.

I included in a footnote of a 1981 obituary for Ohlin a fictional list of Nobel Winners in Economics for the years 1901 to 1930, prior to the 1969 funding of such a prize by the Bank of Sweden. As Casey Stengel would say, “You could look it up in the *Journal of International*

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Economics." Many on tonight's list would have earned their laurel wreaths during the 1931–1968 hiatus that discretion made me leave blank.

Back when I was a graduate student, characters roamed the quadrangles. My wife and I agreed that all our teachers and friends were characters. As time passed, we used to speculate whether there really had come to be less characters; or whether, as we were prepared to believe, it was just the case that less colorful people now came our way or that our palates had lost the power to detect nuances of character because of overexposure to spicy sensations.

Besides being creative intellects and energetic researchers, my heroes are a colorful collection of human beings. One of them at most could pass in a crowd without being noticed, but I shall not give a hint as to who that one might be. However, the temptation to tell the following story is at this point irresistible:

On a visit to Princeton I once attended the usual afternoon tea in Fine Hall where the mathematicians always gathered. As I looked around the room, my eye was struck by the oddball characteristics of everyone present. I shall skip names. But when I mentioned this to my host, inviting him to notice what a Polaroid picture would preserve from that room in that instant, he was in no way offended or surprised. "Yes," he said, "it was only the other day that a stranger appeared here looking for a mathematician whose name he had forgotten. When we asked him to describe the chap, he could only say that there was nothing unusual about him. With one voice everyone cried out, 'You must mean Al Tucker'! And we were right.

If someone like me does not describe what these great scholars were like in personal terms, that information will be lost forever. Their good scholarly works will live forever in the learned journals and book publications. Being called a character is not equivalent to a derogatory description. These were admirable and lovable people, who were much admired and loved—not least by me. So, when I record that Nicky Kaldor was sometimes deemed to be self-centered or unthin, I shall hasten to include mention of some incredible Kaldorian feats of theoretical intuition.

NICHOLAS KALDOR

Specifically, in Kaldor's case, it was a sight to see him and Joan Robinson argue at the Monday night Secret Seminar in Cambridge. Literally, each talked 90 percent of the time. Impossible, you say, for 100 to be divided into a pair of 90's? Well, who said anything about how much listening was getting done?

I have told elsewhere, but without full disclosure of all names, how in the early 1930s Thomas Balogh came upon his two London roommates arguing, the erudite Paul Rosenstein-Rodan and the exuberant Nicholas Kaldor. When he later left the apartment with Kaldor, Balogh said, "I didn't realize, Nicky, that you knew Pareto's work." "Actually, I don't," Kaldor replied, "but after Rosie described the problem, I saw how it had to go."

Of Kaldor's many intellectual feats, let me mention only two. His 1940 theory of the trade cycle involves Keynesian curvaceous saving and investing schedules that intersect 3 times, involving 1 unstable income level surrounded by 2 stable intersections. Adding a flexible accelerator element to this multiplier mechanism, Kaldor assumed that accumulation of positive investment would raise the stock of capital and depress the propensity to invest at each income level. Knowing nothing about differential equations, much less about van der Pol-Rayleigh second-order equations of autorelaxation type, Kaldor by mere intuition inferred the existence of a limit-cycle fluctuation of determinate amplitude and period, which would be stably approached by the system after any perturbation from the stable long-term equilibrium. Hats off to him!

Less well-known is Kaldor's independent 1937 discovery of the von Neumann model where outputs are produced by themselves as inputs, with no reliance on the *primary* factors of labor or land. In a polemic with Knight on capital theory, Kaldor pointed out that when slaves and machines are producible by slaves and machines, the absence of any non-augmentable primary factor rules out the good old law of diminishing returns! In steady-state equilibrium *sans* joint products, there is a unique interest rate (equal in own-rate terms for all goods). Alterations in tastes for the respective goods or in the propensity to save and invest various proportions of the goods, Kaldor somehow perceived, could not alter his unique interest rate. Therefore, he could validly infer that always his equilibrium interest rate equalled the maximum potential growth rate of the consumptionless von Neumann growth model. Just as Kaldor didn't know Pareto's works, he didn't know von Neumann's work. But once Kaldor was confronted with the problem, he did see how it had to go!

KNIGHT, VINER, AND HABERLER

Who could forget Frank Knight, a little dumpy figure in a workman's cap when he first gave a guest lecture to us University of Chicago sophomores? His squeaky voice emitted a mixture of Will Rogers' profundities and Ludwig Wittgenstein one-liners. Anyone so understandable you knew had to be a deep thinker.

Or consider Jacob Viner, Knight's colleague, who towered above him by a full inch. Viner not only made female graduate-students cry, he reduced lieutenant-colonels and first sergeants to tears in his postwar seminars. When I wrote an official Viner obituary, I investigated the accusation that he had mellowed with the years. Notarized testimony about his final year at Chicago suggested that, if Viner ever did mellow, it was on the train from LaSalle Street station to Princeton Junction.

According to my doctrine of balancing reportage of personality traits by reportage of scientific breakthroughs, let me say a few words about the works of Knight and Viner. The Second Chicago School, that of Milton Friedman, George Stigler, and Aaron Director, is distinguishable from the First Chicago School of Knight and Henry Simons. Science does not stand still. But sometimes in the current Cook County preoccupation with Pareto optimality and libertarianism, something of the Knightian concern for interpersonal equity seems sadly lost.

Knight used to say that he became an economist because his feet hurt: so, rather than follow the plow, he signed on in our subject. The true reason is no less interesting. Either from the autobiography of Alvin Johnson or of Norbert Weiner, I learned that Knight began at the Cornell Graduate School in philosophy. But he talked too much. When the exasperated chairman gave Knight the choice of talking less or leaving the field of philosophy, legend has it that he levitated down into economics. Luckily he did not have to drop one notch further for it was Knight who used to say: Sociology is the science of talk; and there is only one law in sociology—Bad talk drives out good!

Knight's atheism was almost prurient in his disdain for religion, particularly Christianity. This was a violent reaction from his fundamentalist parental indoctrination. In *New Palgrave* [1987, volume 3, p. 55], Stigler retells brother Bruce's account of how under parental suasion the eleven Knight children "... signed pledges to attend church the rest of their lives. Returning home, Frank (then 14 or 15 [and the eldest]) gathered the children behind the barn, built a fire and said: 'Burn these things because pledges and promises made under duress are not binding.'" Sadly though, Knight could not burn away the scars. Never could he leave religion alone. And I found it infinitely sad, to learn from Donald Dewey [1986], that even in old age

Knight was plagued by insomnia as he could not stop thinking about the follies of organized religion.

With respect to Jacob Viner, this favorite pupil of Frank Taussig fully anticipated in his non-mathematical way the Lerner-Leontief pure theory of international exchange—which is isomorphic with the competitive equilibrium of *Value and Capital*. Viner's boner in trying to get Wong, the mathematical draftsman, to make the envelope to the family of descending U-shaped cost curves pass through their bottoms amused his admirers and mortified him. As his graduate class was ending, he admitted to me he had been in error for some mysterious mathematical reason. "But," he proclaimed, "I can do it." Being all of 19 I quipped brashly, "Yes, with a good thick pencil you can." Make no mistake though, Viner's 1937 *Studies in International Trade* is incredibly erudite and acute and the hundreds of hours I have spent with that book rank with the most delightful interludes in my professional life. Along with Viner, the Haberler who in 1930 broke out of the fetters of Ricardian one-factor-of-labor models by use of the *production-possibility frontier*, surely deserved a Nobel Prize; or together they deserved a joint one for their trade innovations.

Personally, I owe much to my teacher Gottfried Haberler. From Schumpeter I got encouragement and praise; it was Haberler who marked my early manuscripts with scores of needed emendations. With the impatience of youth, I sometimes rebelled at Haberler's eclecticism—until he one day pulled me up short by saying: "Paul, how do you know Mother Nature is not eclectic"? Thanks to him, I have felt little tempted by narrow positivism and Napoleonic reductionism. The model on my shield and sword is this: "Be as eclectic as the data require, seeking to avoid two kinds of error: over-simplicity and shapeless description."

Joseph Schumpeter, like Keynes, was born before 1885 and is ruled out of the present review of the troops. Alvin Hansen, my surrogate father in academic life, I have omitted in tonight's lecture for the reason that my 1985 Godkin Lectures, *How Keynes Came to America Through Harvard*, were largely devoted to his genius. As I documented there, Hansen was more than a great policy advocate—the so-called American Keynes. His analytic writings, and those of the disciples in his workshops, pioneered developments of macroeconomic modelling. Here I shall only cite my recent write up of the third multiplier-accelerator model—that of Keynes-Hansen, which deserves to be remembered more vividly than it is.

You and I would be here all night if I were to do full justice to all the names mentioned in my original list. So I must be brief, indulging in hit-and-run comments.

FRISCH, TINBERGEN, AND MEADE

Just as our Lord's house has many mansions, the edifice of science needs varied personality types. Ragnar Frisch was the loner scholar, much like John Hicks. Frisch had able students—Trygve Haavelmo, Leif Johansen, and others—but Frisch also had the need to be alone and follow his own star. He believed in that star. After being appointed to a United Nations' economics commission around 1950, he became impatient with its literary deliberations. The cure, he decided, was for him to teach mathematics to his fellow commissioners. Since Frisch had just discovered Marshall's doctrine of external economies, he picked that for the text of his first sermon. Roy Harrod and the other distinguished international economists did not come back for the second lesson. Frisch found this inexplicable and went out of channels in pressing upon his countryman, Trygve Lie, the first Secretary General of the United Nations, the need for Lie to compel the commission members to attend the needed remedial sessions.

The economist world applauded when Jan Tinbergen and Frisch received the first Alfred Nobel Award in 1969. I called Tinbergen a "humanist saint" in a *Newsweek* column. Not many of us scholars are saints of any kind. But James Meade might be paired with Tinbergen in this beatification. When Meade received his Nobel in 1976 along with Bertil Ohlin, I described him in the *New York Times* as the last of the utilitarians "... who, meeting a coatless person on the street, would give him his coat." James corrected me by return post, saying in effect: "It is true that I would believe I ought to do just that, but can I be confident that I would live up to my ideals"?

Don't make the mistake of thinking that saints have to be fuzzy thinkers. Our encyclopedias are chock-full of important hard theorems discovered by Tinbergen and Meade. One instance that concerned my own work will illustrate. After my 1948 article deduced that goods mobility would often suffice to ensure complete (rather than partial) factor price equalization in international trade, half the economist world was engaged in proving this wrong and the other half in proving why it was indeed the case. Tinbergen was quickest off the mark in writing down the conclusive mathematical equations. And Meade led the crowd in showing why more goods than factors would, in a zero-transport-cost world, lead to an uncountable infinity of (inessentially) indeterminate geographical specialization patterns. Neither of these saints thrusts forward his claims for scientific innovation. But be assured that neither is unaware of his just merits.

MARSCHAK AND KOOPMANS

If Frisch was a loner, Jacob Marschak was a warm and tireless member of the working parties seeking scientific truth. At age 79, as at age 30, Marschak worshipped the pursuit of science. Jane Austen spoke of spinsters who lost the bloom of youth at 25 and of admirals who were old crocks at 39. In science it is not the legs that are the first to go. It is enthusiasm that oozes away as cynicism seeps in. Not so with Jacob Marschak. To the end he pursued the Holy Grail and knew it to be worth pursuing. For my money Marschak's axioms on expected utility are the definitive ones, preferable to those of von Neumann and of Ramsey—no mean accomplishment.

Tjalling Koopmans was a serious scholar, with many deep and versatile analytic discoveries to his credit. He did superlative empirical work on tanker shipping rates. Long before the dawn of modern finance theory, he worked out for Penn Mutual insurance company how it could hedge in the bond market its predictable future cash outlays—a pearl he cast before swine who ignored it. With Trygve Haavelmo he pioneered the technique of *identification* in time series. Before there was a subject of linear programming, at the War Shipping Board Koopmans discovered that subject while knowing nothing about the works of Kantorovitch, Hitchcock, or Dantzig. His researches in turnpike theory, like those of Radner, McKenzie, and Morishima, were deep. I could go on and on.

Tjalling was also a serious person. At a Nobel conference on Lake Constance some students took his picture. It was a charming likeness of a smiling and handsome scholar, but he and his wife didn't like it. When Risha and I pressed them as to why they didn't like it, they finally said it wasn't typical. At second glance I realized that it was the smiling that the camera caught which they regarded as non-representative. That was false advertising! Koopmans was seriously opposed to fine writing in economics, not a common crime in our field. According to his code of scientific honor, mere elegance must not give ideas an unfair boost.

LUNDBERG, HAYEK, HARROD AND HOTELLING

If Koopmans was content to be serious, Erik Lundberg was an unglorious Swede, ever with a quip to puncture pretension or stuffiness. Last summer, fifteen days before he died at the age of 80, he reminisced from his sick bed with Assar Lindbeck and me. We talked about Alvin Hansen whom he liked. Lundberg remarked: "In 1937 I was waiting for Hansen in his Harvard office. Seeing my book on his shelf, I naturally took it down, rather expecting its pages not to be cut. But every spread was underlined with copious marginal notes." At this point, there was a twinkle in Lundberg's eye when he went on to say. "So naturally, when Hansen's next book came out on similar matters I was a little disappointed to see that no mention had been made of my *Studies in the Theory of Economic Expansion*." I am glad to add, that this Lundberg classic is still the most quoted treatise of the Stockholm School of macroeconomics. And it deserves to be.

You all know the works of Hayek. Perhaps not all have read his little memoir concerning his World War I encounter with his cousin Ludwig Wittgenstein; few may know that his University of Chicago Press book on psychology is held in awe by such Nobel physiologists as Gerald Edelman of Rockefeller University. (When those two corresponded about the mind-body problem, Hayek wrote to this effect: "When I was in my low seventies, I suddenly became depressed and scientifically sterile. Unaccountably, a year later the old zest returned. But once again a similar pattern came to pass a few years later. With the discovery of new instruments of anatomical observation, I learned for the first time of two scars of coronary infarcts dating from those periods. There's the mind-body problem for you." Who said Hayek disdained Baconian empiricism?)

It is a scandal that so original and prolific a scholar as Roy Harrod never held a chair at Oxford or any other British university. The quintessential don, Harrod looked a bit like a handsome American Indian. His microeconomic contributions were first class: the correct form of Viner's envelope is given in Harrod's 1934 paper on imperfect competition. Only 90 years after Cournot he independently discovered the marginal revenue (or dR/dq) concept, and he was robbed of acknowledged credit for naming the concept only by Frank Ramsey's high standards as the referee that editor Keynes relied on. His growth theories you all know.

Though Harrod was like Kaldor in lacking mathematical training, like Kaldor he was brilliantly intuitive. As of 1935 he may have understood Keynes' *General Theory* better than the Master did. His 1937 definitive account of that model antedates Hick's better known one, and is isomorphic with it. Harrod did not have to be a great reader: easier it was for him to invent the principle of acceleration than read about it in musty journals of the World War I period; that way, besides, he could more likely improve upon received theories.

Like Frisch and Hotelling, Roy believed completely in his star. If Keynes could revolutionize economics, so could Harrod. If Keynes could in addition clear up the theory of probability, so could Harrod. ("How long is an unknown road?" Harrod was the first to answer this basic question of induction: "As long again, on the average, as you have already come on it." The question, "How big is the square of the road's length"? is not one he addressed in print.)

Gilbert Ryle said that at Oxford Faculty meetings he always first determined which side of an issue Roy was on. That way he knew which motions to vote against. Rarely, Ryle observed, did Harrod prevail. But Ryle thought it a beautiful sight to see a battleship being attacked by a foil, and often hard pressed thereby. Freudian memory makes me remember lines like these from Harrod's biography of Lindemann (Lord Cherwell), Churchill's physicist braintruster. "Lindemann was not too popular in the pre-war common rooms. But the Prof was a genius.

Unfortunately you had to be a genius to recognize that fact. I was the first at Oxford to fully appreciate him."

When I was Harrod's guest at Christ Church High Table in 1948, he told me he was scheduled to give a public lecture on the atomic bomb.

Samuelson: But do you know anything about the atomic bomb?

Harrod: No. And at first I was inclined to turn down the invitation. But as I got to thinking about the subject, I realized my ideas on the subject were as good as anyone else's and so I accepted.

Harrod did me one good turn. He published in book form his fugitive financial journalism from newspapers and brokerage letters. When he looked at his handiwork, he found it good. When I looked ex post at his ex ante predictions, I was less sure of that. So I resolved never to republish my similar items and have been a happy man ever since.

Harold Hotelling had two great careers. He applied his mathematical arsenal to microeconomics: We still use his discount formulas for allocating machines and resources over time. His neat model of competition along a line illuminated both oligopoly and the tendency in politics for two rival parties to push platforms toward the center. The duality theorems that Shephard and I polished up are already present, *ab ovo*, in Hotelling's 1932 masterpiece of competitive pricing. His 1938 Presidential Address on welfare-pricing is a jewel, fit to be mounted next to Frank Ramsey's classic of the 1920's on second-best taxing and pricing. Besides all this, Hotelling revolutionized statistics teaching in America, leading us into the age of Fisher, Neyman, and Wald.

One story is enough to dramatize Hotelling's complete belief in the vital importance of the subjects he studied. In the black year after Pearl Harbor, Hotelling in a 1942 M.I.T. seminar on statistics declared in all seriousness: "Despair not. The Allies, in Britain, America, India and the USSR, have by far the better statisticians. Germany and Japan are doomed to ignominious defeat." And so it came to pass.

HICKS AND LERNER

I shall reserve for another occasion fuller remarks about John Hicks, whose work and my work have exhibited many parallels. Let me at the beginning pay him the highest compliment that one scholar can give to another. It was Mayor Gibson of the wasteland we call Newark, N.J. who said, "I don't know where America is going. But wherever it is, Newark will be found there waiting." Time and again when I arrived at some interesting discovery in value theory, there Hicks was already waiting for my arrival. Hicks being the kind of scholar who works from inside his own mind, I could often joke that I take unfair advantage of him: he depends on his own originality, whereas I had the advantage of what it is that both of us know. As I once said in a University of London Stamp Lecture, my own code is to recognize and acknowledge all that anyone has ever discovered in my science and then go after the bigger game of seeking to advance the subject under that constraint. Glory aside, it is an efficient procedure for science to follow.

When I was 21, John Cassels at Harvard told me that Hicks was the world leader among the younger economists. In 1936 Hicks was 32 and this was a prescient judgment since he was something of a late bloomer. I thought that later his countrymen insufficiently recognized Hicks' quality, in part because he was no part of the coteries that Balkanized British economics during the 1935-1950 epoch. As a grand old man, as *the* grand old man, Sir John Hicks today is seen to be the scientific innovator he has always been.

Abba Lerner was in a class of his own. Starting LSE night school at older than usual age, in two years he was turning out classic papers in economic theory. If Lagrange's mechanics is a Shakespeare poem, Lerner's *Economics of Control* is a Mozart concerto. His Model T Keynesian writings, looked at from today, are truly ferocious. His simple policy programs are chastely logical. If Kaldor reminds one of the physicist Pauli, Lerner reminds us of the brilliant Szillard (who patented nuclear fission before Fermi or Hahn invented it).

Abba was a dean's nightmare. When Oskar Lange, a Polish socialist, was being offered chairs at Chicago, Berkeley, Ann Arbor, and everywhere, Lerner was receiving only guest professorship invitations. He was a premature Bohemian, and I do not mean from Czechoslovakia. Under his sandals were no socks, and for a score of years it was a reflection on American academic life that a post worthy of Lerner's genius was not to be found.

Three Lerner stories I have told many times. When he developed his functional finance at the University of Kansas City around 1940, his disciples suggested he call it Lernerism. But he told me in all seriousness that he refused lest it lessen the program's appeal. This is the Lerner who visited Trotsky in Mexico City to persuade him to use Lerner-Lange pricing under communism. The assassin dispatched from Moscow scotched that campaign in the bud. It was this same Lerner whose friends had to suppress his wartime publication of a scheme to run the war by giving Generals Eisenhower and MacArthur each abstract purchasing power to be used in bidding for resources in auction markets. At the 1946 Cleveland AEA meeting, Lerner commandeered an empty hall to propose that the Russians be given the choice of introducing democracy or being atom bombed. When I told Lionel Robbins and Hayek at LSE about these activities, Hayek said concerning the auction proposal, "What's the matter with that? I've proposed as much." And concerning the preemptive bomb strike, Robbins uttered the exact same words.

Dogmengeschichte would be incomplete if 1) I did not refer you to Tibor Scitovsky's wonderful appreciation of Lerner's contributions, and 2) remind you of Lerner's ingenious wire handicrafts. Once when I told him about just having seen in Paris a wonderful exhibit of Caldor mobiles, Abba replied, "Mine are better."

LANGE, KALECKI, AND KUZNETS

Oskar Lange was the organized teacher and disciplined researcher that his friend Lerner never tried to be. Lange offered to return from Chicago to Berkeley if Lerner could be part of the package deal, but the Associate Dean said no dice. The years 1934–1944 were a marvelously productive decade for Lange in microeconomics, macroeconomics, and Marxian analysis. An erstwhile Polish social democrat, Lange was persuaded by patriotism to go back as Polish U.N. Ambassador and subsequently as Vice President. Unlike Kalecki, Lange got caught in the bureaucracy and in power politics. His scientific muse suffered in consequence and he died too young.

Michal Kalecki was led to his independent discovery of Keynes *General Theory* from his study of Marx. That is Joan Robinson's considered view, and she used to tell the story of Kalecki's taking to his bed when he learned he had been forestalled by Keynes' *General Theory*. Like Tinbergen, Kalecki pioneered econometric measurement of business-cycle models. He was a brilliant simplifier: If capitalists saved less, that would add to their profits, raising real income just enough to keep labor's fractional share constant at the level determined by the system's degree of monopoly.

McCarthy harassments aside, I fear Kalecki was not a success in the late 1940s at the United Nations. He could not believe in the prosperity that came to pass in the Age after

Keynes, and he always was firmly a man of principle. Kalecki always stood by his guns and died out of favor in Poland, a victim of anti-semitism and totalitarian politics.

Simon Kuznets is the one scholar on my list linked least with economic theory. His skills with history and statistics more than compensate for this preference of his. When he wanted to, Kuznets could do theory—as Hicks and I learned when we studied his *Economica* paper on how to interpret in welfare terms the aggregative ΣPQ data that are known as national income. I first learned from Kuznets's early writings about the fabricating of Slutsky cycles by the differencing and the moving-averaging of random numbers. Like Marschak and Leontief, Kuznets had been a Menshevik rather than a Leninist Bolshevik: their backing the wrong horse was our gain in American economics. Simon was as soft-spoken as I am boisterous. When students would call out, "Speak louder," Kuznets would whisper, "Move closer"—a case of the fallacy of composition and of advice that would not be needed if it could be received.

LEONTIEF AND SRAFFA

Wassily Leontief was my beloved teacher. I am as much younger than him as I am older than Bob Solow, another foal from the Leontief stable. Wassily was 29 when we first met, and last time I saw him at NYU, in his ninth decade of life, he had not changed an epsilon. It now turns out that while Calvin Coolidge was napping in the White House, the young Sraffa and the young Leontief were forging the tools of input/output. As far as I can remember, neither of the two has ever referred to publications of the other and I suspect that this is an honest reflection of what each has felt to be the need to know about the other's work. Mine is a different philosophy. With the Germans I say of their two Works: "Herring is good. Chocolate is good. How good must be chocolate and herring."

Piero Sraffa's writings speak for themselves. In his seventh decade he unveiled his researches on producing commodities by means of commodities. In villages throughout Italy, India, and East Anglia seekers of truth burn the midnight oil studying the truths contained in the 99 pages of Sraffa's classic. All over the globe Sraffa's magnificent edition of Ricardo's *Works* graces the coffee tables of the educated elite.

If our profession has a Sir Galahad, Piero Sraffa is it. That explains Michal Kalecki's famous story: "When I first went to Britain, I hoped to meet an Englishman. Throughout the British Isles I could discover only two. One was a communist [presumably Maurice Dobb]; and the other turned out to be an Italian."

A sadder property of Sraffa as a producing scholar is encapsulated in the following sample of Maynard Keynes's wit. When told what was finally decided to be the diagnosis of a mysterious ailment of Kaldor in wartime Cambridge, Keynes replied: "What, Nicky with athletes' foot? I don't believe it. Next you'll be telling me that Piero has writer's cramp!" Soberly speaking, it was a tragedy that Sraffa had a psychological block against lecturing, and a blessing that his writer's block was less than absolute. I was proud to be his friend.

THE INCOMPARABLE JOAN

My time is almost up. Joan Robinson I've left to the end. In my measured judgment, she was a great economist. Since I have written recently on "Remembering Joan" Samuelson [1988], I shall briefly sample a few of my remarks.

In the years 1932 to 1982 Joan Robinson contributed to every field of economics. Her 1933 *Economics of Imperfect Competition* is a classic that nicely complements Chamberlin's classic of the same year. She was in the Kahn-et-al Circus that helped Keynes gestate the *General*

Theory. Her article on output as a whole, Robinson [1933], was the first leak to outsiders of the Keynesian revolution to come. Her books on Model T Keynesianism were invaluable. In Robinson [1938] she made quantum improvement on stability of trade theory: she corrected the error that a currency depreciation must, in a stable system, depreciate the terms of trade of the country depreciating. Her 1942 Essay on Marxian matters is a masterpiece: she is pro his ideology but unsparingly she rejects his faulty labor theory of value. Robinson's 1956 *Accumulation of Capital* is a pre-Sraffa classic. On the inadmissibility of a *scalar* aggregate of capital to serve as a productive factor, she was dead right. She pioneered with Kahn, Kaldor, and Pasinetti the post-Keynesian macro tautologies of income distribution, but rejected their applicability to the real world because of its stubborn failure to display balanced growth.

The daughter of a Major General and descendent of Maurice the Christian socialist, Joan Robinson first admired Stalin's USSR, then Castro's Cuba, then Mao's China, and even North Korea. During the cultural revolution she told me that work in the countryside would be good for a sedentary professor of economics. (I must admit that I prefer tennis.) What Samuel Johnson applied to second marriages applied to Joan's passion for social reform; it was a triumph of hope over experience.

When Diogenes reached Joan, he could douse his lamp and end his search. Here are some words I wrote in the to-be-published piece entitled *Remembering Joan*:

I once remarked to an old Cambridge hand that she [Joan] must think well of another leftist scholar there. He corrected me: 'Oh no, she is contemptuous of him as a lackey of the Party Line'.

A Polish scholar once volunteered to me: 'I love that woman. When she visited Poland, all the brass turned out to give her the royal treatment. Replying to the toasts and fine words, she proceeded at once to the forbidden topic that all of us were thinking about, saying, "Why are we wasting our time on these matters when the workers in Posen are rebelling? What is the reason for that?" We young people revered her for that.'

I learned more from Joan than she learned from me. And that is not a snide comment—at least not solely. She despaired of Solow and me as perverse or worse. Here is the first paragraph of a late letter from her to my colleague Harold Freeman who had just written the first edition of his indictment of American capitalism under the title of *Towards Socialism*:

29 October 1979

Dear Professor Freeman

I was absolutely amazed that such a sensible book could be written in the Economics Department at MIT.

How did you hit it off with Samuelson and company?

Harold did not have the heart to inform her that we were dear friends and that it was he who had got MIT to hire me and also Bob Solow.

Robinson was not a great woman economist. She was a great economist.

FINALE

The drama of science is that of the Golden Bough, where ruling chiefs are slain by new contenders. "What have you done for me lately"? is the query of the Muse of Science. King

Isaac Newton is dead. Long live King Clerk Maxwell. King Maxwell is dead, long live King Albert Einstein. And so it goes.

My pinup-stars labored well in the vineyard. All of them, with no exception, enjoyed the game of science and it rewarded them mightily.

Yes, we do know more than our predecessors in science. We do so because it is they that we know.

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