

The Life of an Economist*

Recruitment

It is virtually a theorem that economists start out intellectual life specializing in some other subject and switch about age 20. Sir John Hicks began in mathematics, John Williams in English, Max Millikan in physics, Robert Solow in sociology, etc. The one counter-example known to me is Paul Samuelson who wanted to be an economist for as long as he can remember, but he is *sui generis*. The reason, I believe, is that young people grow up in a fairly homogeneous environment, and become aware of the fascination of complex social interrelations only on reaching a certain maturity.

Too much attention is given in the United States from too early an age on choice of career, if this view that some careers can be judged only as one comes of age be correct. Another element in the equation is one's relations with one's father. Later at M.I.T. when guiding students, I several times had promising young men decide to leave economics for law. In two cases, it had turned out that their fathers were lawyers. At college, during a period when men rebel against fathers, they had been moved by a charismatic teacher and, following him as father substitute, had been seduced into economics. When the rebellion stage had passed, a basic affinity for the law reasserted itself and economics was abandoned. The same problem existed at M.I.T. on a wider scale in science, where bright young people, enticed by a scientific Pied Piper in secondary school, had come to a university specializing in science and technology when they ultimately wanted different careers. In the

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1950's, before undergraduate concentrations in humanities and social sciences proliferated, the economics department had a bimodal selection of undergraduate majors, an early cohort of weak students who failed to be admitted into popular departments, such as electrical engineering, whom for the most part we failed and expelled, and a second delayed group that came into economics in the third year or even the first term of the fourth year, having finally decided that mathematics, science, or engineering was not their basic bent. Many of these went on to graduate school — elsewhere, for we resisted the temptation to keep them on as graduate students — and had successful careers.

One should not make too much of adolescent rebellion, despite Mark Twain's statement that it was remarkable how much his father had increased in intelligence and judgement between the time Mark Twain was 15 and when he became 21. A small set of economists follow their father's footsteps. Historically, the most distinguished are perhaps John Stuart Mill and John Maurice Clark. Among contemporaries Sir Alec Cairncross, Walter Heller, Lord Kaldor, Sir Donald MacDougall, Walter Salant, and Paul Samuelson have sons or daughters in economics. George Stigler once suggested that professorships in economics be made hereditary, but I do not know whether he had a candidate for his successor.

I had a lawyer father, and there was perhaps a certain amount of resistance to following that career, which he would have chosen for me. In college, my start was in classics — Latin and Greek — but I switched at the end of the second year into economics, ostensibly because of a pedantic and disagreeable teacher in Horace. Certainly I was not moved by the excitement of my first courses in economics which were badly taught by graduate students little older than me (the mature members of the department were not great teachers either). In retrospect, the cause, in a world sliding into depression, was the innate appeal of economics gradually dawning on a young man outgrowing adolescent enthusiasms.

The 1930's attracted people into economics because they wanted to understand why the system was breaking down. Curiosity is a more effective stimulus to work in economics than the desire to make a particular kind of living, or than the wish to do good in the world. The profession attracts brains counter-cyclically, although it may now turn out that stagflation will prove to be as

powerful a magnet as depression in the 1930's which brought in Samuelson, Friedman, Meade, and similar giants.

In the preface to *The World in Depression, 1929-1939*, I have commented on the chances that permitted me to work on ships in the summers of 1929 and 1930, and led to the Graduate School of International Studies at Geneva, Switzerland, under Sir Alfred Zimmern, in the summer of 1931. These experiences pointed the choice to international economics.

Formation

Depression makes economic education attractive, but it does not help finance it. My family was hard hit, and I was unable to win one of the few fellowships available in economics at that time. By luck, I was offered financing at Columbia by the alumni of a social fraternity that was in danger of collapse and needed a few more bodies to establish a critical mass. They offered to pay my way to law school, but had no objection when I chose economics. The opportunity in the fall of 1933 came after I had worked for a year as an office boy in a marine-insurance brokerage firm.

Columbia graduate education in economics at that time suffered from several drawbacks. First was the sizeable number of master's candidates relative to the Ph.D. program, who filled classes but were for the most part not serious economists. Secondly, many faculty had part time jobs elsewhere in the city, typically at the National Bureau of Economic Research downtown, which made contact between students and faculty difficult. Third, the faculty — notably Mitchell, Clark, Angell, H. Parker Willis, and the like — had not been led by the 1929 crash and depression to rethink lectures developed in the 1920's. An instructor in international trade on one occasion, reading from notes, said "Take the price of wheat at \$ 2.00", which produced a murmur from the class which knew that the price was nearer fifty cents, and that the notes were 10-years old.

I would argue, however, that graduate education is produced far more by one's fellow students than by the faculty. At Columbia we lacked the numbers of outstanding students that Harvard had in those years, but benefited enormously from one-year transfers

from Chicago of Milton Friedman and Allen Wallis, from Rockefeller fellows from Europe - Fritz Machlup, Michael Heilperin, E.A. Radice, E.F. Schumacher. The biggest stimulus for me was the transfer to Columbia from Cambridge, England, in 1935, of H.H. Villard who had attended the Keynes' seminar. This was before the *General Theory* had actually appeared, but Villard was an evangelist, and the Columbia students felt an excitement akin to Keats' "On First Looking into Chapman's Homer", as he communicated the new approach to what we now call "macroeconomics". In 1936, Villard and I organized an informal seminar that met in the apartment of Arthur R. and Evelyn Burns. That proved as stimulating as any organized instruction. It also helped me get a job at the Federal Reserve Bank of New York, when W. Randolph Burgess attended one evening and I arranged through him an interview at the Bank.

These paragraphs are unduly severe on the Columbia faculty. In the School of Business, H.P. Willis arranged for many of the students in his seminar to publish their papers in a symposium entitled *The Economics of Inflation*. I choose not look back now at that misguided effort. B. Haggott Beckhart encouraged me as a first-year graduate student to submit a term paper on competitive exchange depreciation for publication. James Angell and I never completely agreed whether equilibrium in the balance of payments meant no net gold movements, as he thought, or no net movement of gold and short-term capital, as I insisted, but he commented faithfully in long single-spaced letters on each chapter as I submitted it to him in 1936 and 1937, after leaving the university, and I have tried to follow his example. And Vladimir Gregorovitch Simkovitch was a never-ending source of stories. Eli Shapiro recounts hearing him say to Michael Florinsky, who had mentioned teaching: "Mihail, let me tell you about teaching (pronounced teachink): take one cup of ideas, mix (meex) with a bucket of water, give students one drop an hour."

Real World

The 1930's were not a good time to get an academic job. Aaron Gordon said later that in 1934 the Harvard Department of Econ-

omics had requests for only one teacher. It went to E.M. Hoover who had waited longest. The next year there was again one job, and it went to Gordon (today our top students have four or five offers each). I do not recall that I contemplated applying for an academic job. I wanted to understand the foreign-exchange market. The Federal Reserve Bank of New York job did not open up right away, and I spent the summer of 1936 in the U.S. Treasury Department under Harry Dexter White, working with Frank Coe on purchasing-power-parity calculations for the French franc which was devalued in the fall. After three months, on October 1, 1936, I switched to the New York bank, working half-time in the Foreign Research Department on British problems and half-time in the Foreign Department in a small section on foreign exchange with Emile Despres.

As it happened, I did not get into academic life for 12 years, largely because of the war and postwar recovery. For one who ends up teaching, that is too long, but a year or better, two, is highly desirable. Too long is likely to make it impossible to perform effectively as a scholar. An agricultural economist who spent forty years in research in the Bureau of Agricultural Economics and on retirement received a grant to distill his experience into a book found it impossible. Having written 2-page policy memoranda for forty years, that is what he knew how to do; unfortunately 200 2-page memoranda do not make a 400-page book. An economist going directly into government who wants to hedge against the possibility that he will later prefer academic life should publish a couple of articles before settling into government. That is more difficult today in the highly competitive scholarly world than it was in the 1930's. In addition, Columbia University before World War II, still retained the German requirement that a doctoral candidate must submit 75 printed copies of his dissertation, which it then used to exchange with other universities and build its library. I had no money for this purpose, but luckily my new wife did. The cost was \$ 2,000 for 600 copies. Columbia University Press wanted to publish only 400 copies to be sold at \$ 3.00 retail, of which we would get back half. After subtracting 75 copies for Columbia and 25 for personal distribution, 300 copies at \$ 1.50 each would yield \$ 450 for a net loss of close to \$ 1,400. With difficulty I persuaded the Press to print 600 at my expense, writing the loss

on investment down to \$ 1,250. When these were exhausted, the Press asked me to finance a new printing, an opportunity which was declined.

The Federal Reserve Bank of New York was a splendid place to work in the 1930's, partly because of Allan Sproul, later president, and John H. Williams, the economist-vice-president who commuted from Harvard, and partly because I worked with Emile Despres who became one of my closest friends. Allan Sproul belonged in a Pantheon of great men I worked under — in most cases far under. It includes General George C. Marshall, General Omar N. Bradley, and William L. Clayton. My contacts with the last three were on the whole remote; Mr. Sproul wrote me a letter some five years before his death in 1978, and we kept up a correspondence. John Williams was the originator of the key-currency concept, which I have traced back to 1932, when he served with E.E. Day as a U.S. representative to the Preparatory Commission for the World Economic Conference of 1933. It is a concept which originally did not seem very striking, but one that has had tremendous survival value, coming back to mind time and again.

Emile Despres had one of the finest minds I have known, subtle, sophisticated, penetrating. He has not left the mark in economics that he should have done, because he was a perfectionist, one who was unable to submit formal papers for publication until he had worked on them still more. The profession contains many economists with this disability. It is an enormous waste of resources. The opposite combination of an easy flow of words but little to say is also encountered. When Despres was teaching at Williams College, which then had no graduate program, it was said that the best graduate education in the United States was obtained by getting a job as an assistant professor there, near him.

In February of 1939, before the German invasion of Czechoslovakia, I accepted a job on the economic staff of the Bank for International Settlements. This proved a mistake, as outbreak of war cancelled the monthly meetings and much of the work of the Bank. It was interesting to see Per Jacobsson close at hand in that environment, although I did not find myself in sympathy with his economic views. When Paris fell in June, 1940, Despres who had transferred to the research staff of the Federal Reserve Board

in Washington arranged a job for me there, in the international section.

The economic staff of the Federal Reserve System is often regarded as a reservoir of talent, available for deployment, since the operating functions of the System do not require all that research. In the summer of 1940, the Board was turned to provide manning for the American side of the Joint Economic Committee of Canada and the United States. Alvin Hansen on leave from Harvard to the Board was made chairman; I became secretary. The Canadian chairman was William Mackintosh of Queens University, and the secretary Alexander Skelton of the Bank of Canada. Among the other American members were Harry Dexter White, Jacob Viner, E. Dana Durand. It was interesting, and working full time on Canadian problems for 2 years, I acquired a great deal of information, with, however, a high rate of obsolescence. Moreover, the assignment contained the seeds of its own destruction. As soon as the Committee had introduced American and Canadian groups working on common problems, as in price control, lend-lease, production priorities, and the like, to each other, the Committee was asked to withdraw. In the spring of 1942, it turned to postwar problems of Canadian-American relations. These lacked immediacy, given the state of war. Accordingly when Emile Despres invited me to move again to O.S.S. in the summer of 1942, I did.

William J. Donovan had been a distinguished general in World War I, a New York lawyer, and a man with the seminal idea that the armed forces in the United States would leave a lot of things undone. To fill the gaps, he built an Office of Strategic Services (O.S.S.) that later developed into the Central Intelligence Agency. A major division was the Research and Analysis Branch (R and A), which had a Board of Analysts, including as economists, Emile Despres and Edward S. Mason. In the economics division were sections to work on intelligence on enemy production, manpower, agriculture, and estimate war materiel.

In addition to economists, Research and Analysis branch had geographers, political scientists, and especially historians. There was a methodological struggle between historians and economists. When it came to estimating Russian wheat production, for example, Russian historians claimed that the economists could hardly make a contribution if they did not know how to read Russian, which would

give them access to crop reports and the like. The economists, on the other hand, claimed that with data on acreage, historical yields and weather they were in better position statistically to estimate output, and the wisps of evidence from the daily press were diversionary rather than helpful. Over the years I have become more sympathetic with the historians' position as I read economic studies strong on technique and weak on background. O.S.S. had some brilliant successes using both techniques combined. Walter Levy could determine when to bomb given plants for hydrogenation of oil by reading German railroad-rate schedules obtained through Switzerland. When feedstock coal began to move in volume, German railroads lowered the rate, and railroad rates of course had to be published. Donald Wheeler's manpower section collected obituaries of soldiers from local newspapers of German towns adjacent to Switzerland and with census data blew up the sample into an estimate of German casualties on the Eastern Front.

In due course it was revealed that the American Air Forces were ill prepared to bomb enemy targets, having at best some vague Douhet-Seversky notions of bringing the enemy economy to its knees by battering it back to the Stone Age. When these notions seemed inadequate for operations, the Air Forces in Europe and in Washington sought help from O.S.S. R and A set up an Enemy Objectives Unit in the Economic Warfare Division of the U.S. Embassy in London. Chandler Morse, Walt W. Rostow, and William A. Salant were the first O.S.S. economists to man it in the fall of 1942. In February, 1943, I changed places with Morse. E.O.U. had an initial problem in winning the confidence of the American air forces who had started out using British intelligence with its long headstart. It was done by finding a British mistake. E.O.U. was not privy to Ultra, the source of intelligence using decoded German radio traffic. Air Ministry intelligence came to depend upon it too much. Using aerial photographic interpretation (see Constance Babington-Smith, *Air Spy*, New York, Harper and Row, 1957), Polish intelligence which came to Britain in enormous volume, prisoner-of-war interrogations and every possible source, we concluded that the Folke Wolf plant at Bremen had moved to Marienberg, Poland. The Air Ministry claimed it had not, wrongly as it proved. From that time on, the Air Force was willing to listen to its most unmilitary economists.

War is a relatively simple economic problem. The objective function has only one argument — winning — instead of difficult tradeoffs among growth, stability, income distribution, etc. in peacetime, and one constraint, to keep the domestic civilian economy moving. Full mobilization is an equilibrium in which one can not increase the military effort by transferring one person from home front to fighting services — an ideal that Britain approached but Germany never did, although the British failed to achieve optimum allocation among air, navy, and ground forces. E.O.U. economists articulated a theory of bombing which rested on the premise of invading Europe — Air Chief Marshall Harris thought the war could be won by bombing alone, a Douhet-Seversky position, with hammer but no anvil. Our theory of bombing combined intuitively — the theories had not been developed yet — input-output on one hand, and capital theory on the other. Intuitive input-output theory said that in a complex interdependent economy, to take out one row of inputs — such as ball-bearings or oil — would bring the whole economy to a halt. Capital theory held that it all had to be done within a time constraint, since given enough time, labor could be substituted for any missing input. These theories have doubtless been formalized and extended in such organizations as RAND since those primitive days. We thought of "depth" — the time today's output was behind its ultimate use on the fighting front — and the "cushion," or availability of inventories, civilian supplies, and the like, that could be diverted to protect military uses. Such targets as steel or electricity were rejected on grounds of both depth and cushion. We were initially ignorant of the time it takes to build an effective striking force and of tactical problems of deep penetrations into Germany, problems handled in any event by the air forces, but which affect target selection.

In due course, the problem was posed as how best to use strategic air forces to ensure success of the invasion of Europe. E.O.U. found itself in a controversy on this issue, a debate between various analysts who advocated bombing railroad centers or marshalling yards, and our own view of driving military trains bringing reinforcements and material back from the landing site by cutting a ring of railroad bridges of a certain size. Railroad centers in our view had a cushion of civilian traffic close to 85 percent. Interdicting all railroad traffic by destroying bridges (which took three weeks

to repair as opposed to four hours for bombed track) was required. The debate still goes on. In his autobiography, *From Apes to Warlords*, (New York, Harper & Row, 1978), Lord (Solly) Zuckerman defends attack on railroads centers, and dismisses the economists as a priori, unscientific and amateur. The controversy is pursued in *Encounter* for November, 1978, and June, 1979.

I was privileged in May and June, 1944, to assist with tactical bombing operations during the invasion on the intelligence side, and in July to join G-2 staff of 12th Army Group commanded by General Bradley, in due course aided by Robert V. Roosa.

Having been overseas from February 1943, to June 1945, with one brief respite, I was anxious after V-E day to go home. Despres again came to the rescue. Many O.S.S. staff had transferred into the Department of State to work on postwar problems such as German reparations. Despres himself was scheduled to go with Undersecretary Clayton to the Potsdam meeting concerning the occupation, and needed a backstop. I made it back to Washington by June 12, and after a week's leave was busy in the Department of State. That fall I tried to switch out of occupation issues and worked briefly on the British loan. German problems remained exigent, however; and soon I was back in the Division of German and Austrian Economic Affairs with Rostow, William Salant, Harold J. Barnett, and others of O.S.S. days. This continued until June, 1947, when the Secretary of State, G.C. Marshall, broached a European-wide recovery plan. I moved into that work in the Department.

Just as we had intuitively reached the essence of input-output theory and capital theory in E.O.U., so in the Department of State we intuited the theory of second-best. A number of firms in Allied countries wanted to acquire German firms for one and another reason; theatres to show films, a sewing-machine plant to replace a direct investment that had fallen to the Russians, a monopoly such as 4711, and the like. Our division insisted that there should be a moratorium on direct investment in Germany until after monetary reform. This, of course, is the theory of the second-best. When markets don't work, don't use them. Revisionist historians assume that the United States as a capitalist nation was anxious to buy up German industry cheap. In actuality it was easy to persuade our betters of the wisdom of the policy. Secretary Byrnes came

from South Carolina, General Clay from Georgia, Undersecretary of State Clayton from Texas. We had only the word, "carpet-bagger," reminding them of the army of locusts that had descended on the South after the Civil War, to have them understand the merits of waiting.

On the Marshall plan, most economists served as advocates rather than objective analysts. I happened to be located at the cross-roads of a series of country and commodity committees. It was revealing how each country expert wanted more, not less, aid for the country he was analyzing, and each commodity expert thought Europe needed more of his product. In retrospect, what is disturbing is the fact that we argued that Europe had a balance-of-payments deficit that had to be made up, whereas in the intermediate run, of course, as Machlup later pointed out, foreign aid determined the deficit, rather than the deficit foreign aid.

Worse: We argued as if we could forecast the needs of 17 countries in 26 commodities for 4 1/4 years as if it were a partial-equilibrium problem and any saving in any commodity would change the overall aid bill. In fact we were so fearful of revealing the limits of our knowledge to the Congress that no one dared change the original calculation of \$ 5.2 billion for the first year and a quarter. The result was that when a change was required in any of the 26 commodities, another change was made in a residual figure. The joke among the statistical staff was that computers in the basement of the Pentagon building which we used were unable to produce any other answer than \$ 5.2 billion for the next several months.

Those were years of hard work, with one-week vacation in 1945, none in 1946, or 1947. In 1948, I had an operation, lost weight, and decided to quit government for an academic job. Some part of the decisions was the belief that President Truman would be defeated for the presidency in the fall of 1948. That proved wrong. Nonetheless I looked around for an academic job. An invitation to give a seminar at Princeton turned out badly. Professors Lutz, Graham, and Viner took strong exception to my defense of the Marshall plan, holding rather the belief that appropriate budget balancing and exchange depreciation by European countries could have restored equilibrium. The same seminar at Yale had the bad luck to find Viner on hand in New Haven, invited to the seminar,

and no more receptive. Finally, Richard Bissell, who had been the chief staff member on the Harriman economic report analyzing the Marshall plan's economic impact on the United States, told me of an opening at M.I.T. I applied, visited the campus, was not asked to give a seminar, which may have been fortunate, and got the job.

Academic Life

Many people find it anomalous that M.I.T. should have an economic department at all, much less a good one. Its existence is only partly accident. Francis A. Walker, a distinguished economist of the nineteenth century, had been president of M.I.T. Davis R. Dewey, brother of John Dewey, was for forty years secretary-treasurer (and one year president) of the American Economic Association, which he ran from his M.I.T. office. It was thought that most engineering students went into business, and that they should know something of industrial relations. Many state certifying agencies in engineering required some exposure to economics. At one time the department of economics was half devoted to labor economics, the other half to everything else. Then it was able to recruit Paul Samuelson. His availability rested first on the resolve of a tiny minority of Harvard faculty not to hire him, because he made them uncomfortable, and second, his interest in staying in Cambridge. With his appointment, a positive feedback process set in, and economics at M.I.T. grew by itself.

M.I.T. turned out to be a superb place to spend the following 30 years. Its micro-sociology was highly effective. The economics department was housed on two floors in the same building with the Sloan School of Management and the Faculty Club. There was easy and informal intercourse within the department, at lunch where members without engagements filled up the same round table each day, and with economists in the Sloan School, some of whom had joint appointments with economics. The faculty was relaxed, tolerant, supportive, as opposed to some departments in other universities, which are divided and antagonistic. Many of us take on the coloration of our environment. If one goes to a third-rate place where few people do research, one is unlikely to

do research. At M.I.T. everyone worked hard, stayed on the campus in the office, accessible to colleagues and students.

There is a possibility I made a mistake in the 1950's, by not seeking to repair omissions in theory, mathematics, and later econometrics that were produced by weak training in the 1930's, and twelve years away from the academy. It is true that the journals were not full during the war years, but I had not been keeping up — as few in government service do — before and after the war. One colleague reassured me, however, saying that is was a mistake to convert a literary economist into a second-rate mathematical one. Moreover, I began to be pushed into economic history by the chance of agreeing to give a year's course on the "Economy of Europe" at Columbia in New York, one day a week, a rare assignment in the United States, though ordinary in France and Italy. I had no trouble preparing material for the second term; that came out of State Department experience. For the first term I needed a running start, and worked up a little history. One by-product was the 1951 *Journal of Political Economy* article on how differently various countries in Europe responded to the fall in the price of wheat in the 1880's. From then on economic history took more and more time.

Thirty years at M.I.T. have produced books rather than articles. What is irritating, however, is a reputation based on a moderately successful textbook, written to help educate a large family, rather than on original work. Textbooks are syntheses of work of others.

M.I.T. has been a splendid place to teach not alone because of the tolerance and support of one's colleagues. The students are of very high quality. Graduate admission is highly competitive, with perhaps 300 to 400 applications each year for openings that the department intends to hold down to 25, but which seem inescapably to inch up to nearer 35. Micro-sociology appears to decree that the optimum size of a scholarly department is on the order of 100 graduate students and 25 to 30 faculty. Increasing economies to scale obtain below these levels; diseconomies above. There is always a temptation to yield to pressure and to grow a little. But beyond a certain size, students do not know each other, faculty has less frequent informal contact.

I am puzzled by the outcome of my teaching. M.I.T. graduates in international economics include Jagdish Bhawati, William Branson, Carlos Diaz Alejandro, Miltiades Chacholiades, Ronald

Findlay, Ronald Jones, Stephen Hymer, Stephen Magee, Robert Mundell, Egon Sohmen, Jaroslav Vanek. It is hubris perhaps to claim Bhagwati and Mundell for M.I.T. since they both studied widely elsewhere, Bhagwati at Cambridge, Oxford, and Chicago under Harry Johnson; Mundell at Washington, London under James Meade, Chicago under Harry Johnson. This sort of peripatetic education, seeking out great men to work under is surely the best possible. M.I.T. has had its share of whom one of the more original was Staffan Burenstam Linder. The distinction of M.I.T. graduates in international economics reflects the fact that students educate each other rather than learn from faculty. If faculty can communicate enthusiasm for the subject which incites the students to work on their own and with each other, little more can be contributed. But I have a very pungent recollection of the remark of an M.I.T. graduate, not my student, saying to me after a party, and somewhat in his cups, that M.I.T. had done so well in international economics because my old-fashioned approach confused the students who were forced to work the subject out independently for themselves.

In the course of thirty-two years, my wife and I have had three sabbaticals abroad; in Geneva, Oxford and Paris, Kiel and Rome. Each carried research deeper into European economic history and further from the pure theory of trade. International money represented a compromise and a continuing interest. Preoccupation with history led further from pure theory in a seminar at Harvard that lasted two years and produced a book, *In Search of France*, written with two political scientists, two sociologists, and an historian. Interdisciplinary research, I concluded from that exercise, is hard work, productive, but should be undertaken later rather than sooner, after one has spent years within one's own discipline. Departmental divisions strongly discourage such research. It is erroneously thought by many that the best explanation is the one that manages to stay within a given discipline, and indeed within a given technique. I once asked a visiting European scholar how he liked teaching at Not much, he said. All the students reason like Professor At Chicago, he went on, everyone is a Becker, looking for economic explanations of all sorts of phenomena that are normally thought to lie outside the purview of that subject. And as for the M.I.T. "fryer," referring to chickens

available in American supermarkets that are exactly alike in size, weight, taste, they are superb at taking a model, manipulating it mathematically, then testing it with econometrics, but they are all identical.

Literary economic history, or historical economics as I sometimes call it, is not the fashion of the on-going M.I.T., but it is a splendid way to occupy one's retirement.

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