

## A Roundabout Approach to Macroeconomics: Some Autobiographical Reflections\*

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### I. Introduction: Setting the Stage

"Roundaboutness" is a concept featured in Austrian capital theory. Homely stories about the bare-handed catching of fish are a prelude to a discussion of the economy's capital structure. The outputs of some stages of production become inputs to others. Production takes time. The capital structure, broadly conceived, has a temporal profile—one that can be modified in response to changes in intertemporal consumption preferences and resource constraints. This was the central message of Eugen von Böhm-Bawerk (1959).

Alfred Marshall, who theorized in terms of the short period and the long period, taught us that most problems in economics stem from the ever-critical time element. I think Marshall was right on time. But I also believe that a healthy understanding of some of those problems, particularly the ones in macroeconomics, is not best facilitated by his simple short-period/long-period distinction.

Adopting Marshallian methods, John Maynard Keynes dealt with the polar extremes in the quality of expectations, casting serious doubt about the viability of a market system. In the short run, the time element itself is no problem: Short-run expectations faithfully reflect reality. If the level of spending changes, the multiplier process plays itself out in a clockwork sequence of spending and earning, eventually achieving a new circular-flow equilibrium. Long-run expectations, however, are

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another matter. Here, the time element is a debilitating problem: These expectations, if you can call them that, are baseless. The future is shrouded in an impenetrable fog of uncertainty, leaving the current level of investment spending to be determined by unruly psychological factors—Keynes's infamous "animal spirits." The resultant circular flow will gush and ebb and even on average may not entail enough flow to fully employ the labor force.

The circular-flow framework, exercised in both its short-run and long-run modes, seems to me to be exactly the wrong framework for understanding and dealing with the time element in macroeconomics. Identifying the polar cases of "no problem" and "debilitating problem" doesn't get us any closer to a solution to all those intermediate cases lying between the poles. The tell-tale feature that inevitably characterizes this framework has been recognized in recent years by Robert Solow (1997)—namely the lack of any "real coupling" (Solow's term) between the short run and the long run. In Solow's reckoning, the two runs simply divide our discipline's subject matter into (1) the problem of maintaining full employment of existing resources and (2) the determinants of economic growth.

A viable alternative to the Keynesian circular flow framework is the Austrian means-ends framework. People employ means (investment) to achieve ends (consumption). In a capital-using economy, there is a significant time dimension separating means and ends. We realize that some production processes take more time than others. And, thinking in macroeconomic terms, we recognize that production time can increase or decrease for the economy as a whole as market conditions warrant. In the "medium run" (a term from Solow, 2000), the problem is one of adjusting production decisions to (intertemporal) consumption preferences—a problem that Axel Leijonhufvud (1998) urges us to put back on our macroeconomic agenda. The only solution available in a decentralized economy is one involving entrepreneurial responses to changing market signals and especially to changes in the rate of interest. The time element here (Böhm-Bawerk's "roundaboutness" of production activities) is a key variable in the system. Like other endogenous variables, it is subject to marginal adjustments in response to parametric changes. Of course, there is uncertainty. And the fog is more of a problem the farther into the future the entrepreneurs are trying to see. But still, what seems to be called for is an application of marginalism and not a contrasting of the polar cases.

At the very dawn of the Keynesian revolution, the circular-flow framework and the means-ends framework were seen as the two real live alternatives (Hicks, 1967). Marshall-cum-Keynes gets you one vision of the economy (plus a toolkit of policy options); Böhm-Bawerk-cum-Hayek gets you another vision of the economy. In the early 1930s, Friedrich A. Hayek was brought to the London School of Economics by Lionel Robbins precisely for the purpose of providing a counter to the ideas of Keynes. But Hayekian ideas proved ineffective against the Keynesian avalanche. Debate continues to this day about just why Hayek failed to review the *General Theory*

(Caldwell, 1998) and just how he lost out to Keynes.

At mid century, Hayek's business cycle theory, a theory that contained the seeds of a full-fledged capital-based macroeconomic framework, had sunk into oblivion. His books, including *Prices and Production*, which was the print version of the lectures he had delivered at LSE in 1931, were long out of print. Undergraduates of my generation—even economics majors—would never hear about Hayek, much less learn that his theory was once the principal rival of Keynes's. The index in Paul Samuelson's *Principles of Economics*, 7<sup>th</sup> ed. (1967), which dominated the field at the time, refers the reader to a single footnote in an appendix on commodity and factor pricing—where Hayek is portrayed, in effect, as being on the losing side of the socialist-calculation debate.

*Prices and Production* didn't get back into print until 1967, when it was offered, along with other Hayek books, by Augustus M. Kelley as part of a reprint series. But in that year, I was a long way from reading Hayek and, in fact, a long way from studying economics. The progression of my own studies from the sinusoidal rhythms of alternating current to the cyclical fluctuations of a mixed economy was a roundabout one—hence the double entendre of this essay's title.

## II. Flashback: Electrical Engineering and Engineering Econ

In January of 1967 I was completing the course requirements for a BS in Electrical Engineering. Few students could finish the course work in four years at the Missouri School of Mines and Metallurgy (now the University of Missouri at Rolla). Most took five years. I was lucky to get through in four-and-a-half—especially in view of my having completed the first two years at a junior college. Getting some early course work behind me at Joplin Junior College kept the cost of my education down. I could live at home and cover the out-of-pocket expenses with income from part-time and summer jobs. The junior college put me a little behind in terms of the applied engineering courses, but it put me way ahead in terms of mathematical skills. Martha McCormick was probably the best college-level professor of mathematics in the state.

Partly because of McCormick, along with some excellent junior high and high school math teachers, mathematics was my strong suit. But it was McCormick herself who steered me away from it. At the time, she knew more economics than I did. She knew that even an advanced math degree did not translate into attractive employment opportunities. Her suggested alternative was an applied field—engineering. She suggested electrical engineering because that specialty made use of the most sophisticated mathematical techniques.

I was very good at textbook engineering but was a virtual non-starter in the electronics labs. Given some output specifications, I could design a circuit and determine the power requirements and the parametric values of all the elements and active components with ease—so many ohms here, so many farads there, and so many watts. If my calculations called for a few 500-ohm resistors and a couple of 30-farad

capacitors, I was off to the supply room. It was a rude awakening—to say nothing of the embarrassment—to learn that a 30-farad capacitor would be about the size of a boxcar!

When I eventually acknowledged the constraints imposed by reality and could actually get the raw materials for a circuit, I had no trouble putting them all together—but only if the other students would leave me alone. Which is to say that I was no good at team work. Unfortunately, we usually had six hands working on one circuit. That was an exercise in frustration for me. Things were no better in the heavy-machinery lab, where we had only three hands working on the circuit. As a not-too-subtle safety reminder, the lab instructor had left one pair of sneakers welded to the floor—a result of an earlier student completing the circuit in an all-too-personal way. Each of us had to work with one hand behind his back. And by the way, “behind her back” is a phrase that had little application at Rolla in the 1960s. The female population at the Missouri School of Mines could have been measured in picofarads.

Whether involving individual effort or team effort, the technical courses at MSM were given precedence over other courses—few in number—that were outside the hard sciences. I took a one-semester course in economics (both micro and macro) to fulfill the social-science requirement. I remember the Keynesian diagrammatics and remember thinking that Samuelson's rendition of the circular-flow, complete with small drain spout that vented saving away from the investment pump, had something of a comic-book character to it. Neither the diagram nor the text inspired careful study of Keynes, and there was no hint of a more appealing alternative. The lectures on unemployment and business cycles were no help. I remember only that the professor ended almost every sentence with the words “as such.” And sometimes there were two back-to-back “as suches”—one deliberate, one subconscious.

The only other course that had any economics content at all was taught in the civil engineering department. It was called Engineering Economics—with the Economics always pronounced with a long e and usually shortened to “Econ.” I look back at that course now with both amusement and gratitude. What does a professor of Engineering Econ do with a bunch of students who (1) have highly honed math skills but (2) have little or no appreciation for the economic aspects of engineering? The answer: Confront them with a bunch of present-value problems in which the inputs and the outputs are described by high-order polynomials or by transcendental functions of one sort or another. Then, award them an A if they can do the math. The professor in that course played right into my hands.

I do remember noticing that in many of the problems the interest rate was given. I wondered just who gave it in the real world, but it never occurred to me to ask. The professor didn't so much as hint that there were important issues here. If the rate of interest reflects the give and take of the marketplace, the resulting resource allocations will have a certain internal consistency about them. But if the interest rate is given from on high by the policy committee of the central bank, the internal consistency is

in doubt and the economy may be moving along an unsustainable growth path. These issues were well beyond the scope of Engineering Econ.

I also recall that in some of the problems, the internal rate of return had to be determined. And for problems in which the given time profiles of outlays and revenues overlapped, there was an obvious possibility of multiple internal rates of return. It could turn out that the internal rates for such a project were 8% and 2%. Not much was made of such cases. But when I learned years-and-a-career-change later of the related possibility of “technique reswitching” and of the so-called Cambridge capital controversy, I found the phenomenon less than earth-shaking and the controversy almost wholly sterile.

Piero Sraffa (1960) had laid the groundwork in a book that he wrote in the 1920s but published decades later. Paul Samuelson offered a summing-up article in 1966: The relative profitabilities of two techniques (*A* and *B*) could change and then change back as the interest rate underwent a continuous decline. That is, during the decline, a profit-maximizing producer would switch from *A* to *B* and then back to *A*. The math was the same as in the case of multiple rates of return: Set the present values of the two techniques equal to one another to find the two rates—maybe that same 8% and 2%—where the switching and reswitching take place. One technique is the more profitable between the switch and reswitch points; the other is the more profitable at all other rates. It’s Engineering Econ. But the very possibility of technique reswitching, according to the Sraffians, casts grave doubts on the logical integrity of the neoclassical production function and on the validity of Böhm-Bawerk’s capital theory.

Eventually, Joan Robinson (1975) wrote an article on “The Unimportance of Reswitching”; Cohen and Harcourt (2003), who have recently wondered out loud “Whatever Happened to the Cambridge Capital Theory Controversies” still see deep and unresolved issues here. My own take on the controversy appears as “Waiting in Vienna” (1979).

### **III. Disrupting Events and Changing Interests**

During the summers of my undergraduate years, I had work experiences that gave me a taste of the real world of engineering. I worked one summer for the Eagle Picher Company in Galena, Kansas. The Galena facility, located along a stretch of the original Route 66, processed lead and zinc for the company’s battery division. The work there was a little too real-world for me. I don’t think I would have made a very good production engineer.

I worked another summer for the Kansas City Power and Light Company. There I learned first-hand about the perversities of rate-of-return regulation that had recently been dubbed the Averch-Johnson effect. This form of regulation creates incentives for the firm to pad its capital base so that higher total profits fall within the allowed rate of return. Most of my time at KCPL was spent doing Engineering Econ—using present-value calculations to show that a well-insulated all-electric home is more cost

efficient than the more conventional homes that used both electricity and natural gas. A low rate of interest favored the all-electric option because of its large up-front insulation costs.

After graduating from the Missouri School of Mines, neither my work experiences nor my one interview trip (to IBM in Poughkeepsie, New York) counted for anything. The Vietnam war was in full swing, and I was registered at one of Missouri’s more draconian draft boards. My older brother (and only sibling) had received his draft notice just before graduating from the Kansas City Art Institute. Jim was in a training trajectory that would eventually land him in Vietnam. My options were limited. MSM in the 1960s was no Berkeley. Engineering students tended to be neither protesters nor draft dodgers. But many of them, including me, were risk minimizers. I chose to take a commission in the Air Force, preferring four years in that branch of the service to two years in the Army. On receiving the commission in San Antonio, Texas in April of 1967, I was assigned to an electronics-countermeasures lab at Griffiss AFB in Rome, New York. I became a systems engineer, traveling to Texas, California, and elsewhere in the continental US to monitor contracts for the development of radar-jamming systems and the like.

At Griffiss there was some in-house capability, including a state-of-the-art mainframe computer. General Electric was still in the process of debugging the new computer and hence invited the researchers at Griffiss to use it for any and all purposes in order to identify still more bugs. Computer time became a free good, and given the demands of the limited war on military personnel in the Mohawk Valley, I had plenty of free time to exercise the computer. On my own I decided to learn to program in BASIC, which at the time was a new computer language. I was motivated by my interest in mathematical puzzles. An article on BASIC in *Aviation Week*, a very popular magazine at Air Force labs, claimed that the new language deserved credit for its user friendliness, but that it was not at all adept at “problem recognition.” For instance, BASIC could never be used, according to the article, to solve the problem known as the “Knight’s Tour of the Board.”

I had never heard of the Knight’s Tour, but my AW-reading colleagues had. They explained that the Knight must move around the chess board, starting where a Knight always starts, moving like a Knight always moves, and landing on each square once and only once. It sounded like a challenge. Could I write a program in BASIC to generate a successful Knight’s Tour? My attempt at a solution was literally my first program in BASIC. Undoubtedly I produced a very inefficient and graceless piece of programming. But I had the compensating advantage of unlimited computer time. I could set the program to running late on a Friday afternoon and return on Monday morning to see if a solution had emerged from my remote consol. Eventually, one did. The computer typed out an eight-by-eight matrix of numbers from 1 through 64, showing the trail of a successful tour. I streamlined the program and was able to get a solution in five-to-fifteen minutes. Before I grew tired of this puzzle, my program

found some twenty-odd solutions, never finding the same one twice. And it found one solution where the Knight's 64<sup>th</sup> position was within a Knight's move of its initial position—closing the path and giving me a Knight's Tour from any starting point. I don't know what the state of knowledge of the Knight's Tour was at the time, although the *Aviation Week* article indicated that there were only a handful of solutions. I never tried to publish the program to demonstrate the "problem-recognition" qualities of *basic*. For me solving a puzzle was an end in itself and a good way to deal with too much time.

Less than a year after I began my own four-year tour, Jim got orders sending him to Vietnam. He departed from an Army post in North Carolina, stopping in the Mohawk Valley before heading to the west coast and then to the war zone in Southeast Asia. His diversion from Army life had consisted of reading Ayn Rand. Having read a few of her books, he asked me to find and send him the others. Shortly after his departure, I found a bookstore in Syracuse that carried all of Rand's books—her novels and her collected essays on the philosophy of Objectivism. I bought two of each. Over the following weeks, I would send Jim a Rand book and then read the copy I'd bought for myself.

Objectivism is strong medicine, especially for people with a background in engineering. I had spent my college years avoiding courses in the social sciences because of the apparent lack of structure and reason. But Rand's *Capitalism: the Unknown Ideal* (1967) was full of structure and reason, and it provided a moral foundation for a free society. This collection of essays by Rand and others came with a list of recommended readings, including a number of books on economics. Rand identified the economists of the Austrian School as the most capable of showing just what is—or ought to be—sitting on her philosophical foundation. I soon discovered that Austrian economics is appealing to an engineering mind: basic principles, law-like propositions, unequivocal conclusions—all grounded in logic and applicable to the world as we know it. Among the authors flagged for further study were Eugen von Böhm-Bawerk and Ludwig von Mises as well as such popularizers as Frederic Bastiat and Henry Hazlitt.

The Rand volume itself contained two articles by Alan Greenspan, one exposing the fallacies of "Antitrust," the other establishing the connection between "Gold and Economic Freedom." And in an article by Nathaniel Branden dealing with the economics of depression, a memorable metaphor was attributed to Greenspan: Putting the government in charge of the money supply is like putting a penny in the fuse box. As recounted in a recent biography by Justin Martin (2000), Alan Greenspan was a member of Ayn Rand's inner circle.

All this was heady stuff to me. Just what was the role of gold in the economy? And what was the purpose of a central bank? Were depressions inherent in the nature of a market economy, or were they the result of ill-conceived stabilization policy? For me, these questions had the makings of a new puzzle. And this puzzle had a special

significance because of my awareness of the lingering effects of the Great Depression on my family. My parents were just reaching adulthood when the depression came. Their experiences in coping with deprivation during the 1930s set their attitudes about economic matters for life. They strongly distrusted banks, but they were always big savers—even though there was little income out of which to save. A large portion of my extended family (mostly on my mother's side) left Missouri in the 1940s and 1950s for California. (The economywide depression had gotten compounded in southwest Missouri by the depletion of quality ore in Missouri's lead belt.) I remember hearing an older cousin quip that many in our family were so poor they had nothing to lose by starting over in California and that the rest were so poor they couldn't afford the trip!

There was no coherence in political attitudes in my extended family—my father's family being Republican and my mother's family Democratic. I remember an aunt expressing her political views in an impish ungrammatical sing-song fashion: "Roosevelt was for poor people; I'm a poor people; so I was for Roosevelt." But I also remember the contempt in my father's voice when he recited the campaign slogan that had emerged in Roosevelt's 1936 reelection campaign: SPEND, SPEND, SPEND; ELECT, ELECT, ELECT!

In my immediate family, my father's political gene was dominant. My mother showed no signs of a cross-party marriage and even did duty, along with my father, as a grass-roots committee chairperson for the Republicans. They were comfortably on the right side of the issues of the day—but not on the religious right. In fact, my father's Ayn Rand was Robert Ingersoll, a prominent nineteenth-century free thinker. The touchstone for my parents' Republicanism was self reliance. In my father's eyes, Barry Goldwater personified the party's ideals.

I began reading from Rand's list of recommended books and soon came across *America's Great Depression* (1963) by Murray Rothbard, another erstwhile member of Rand's inner circle. I found Rothbard's account of boom and bust compelling and especially significant in light of the stark contrast between the views of the Austrian economists and those of my engineering cohorts. With a monopoly on money creation, the government could artificially cheapen credit and orchestrate an economic expansion—an artificial expansion that eventually and inevitably would collapse. Policies commonly defended in the name of stability and growth led instead to instability and decay. In later years, I would attach even more significance to this view of boom and bust—when I compared it to the curious and implausible accounts of the business cycles offered by other schools of thought.

During the second half of my military career, I immersed myself in economics—Austrian and otherwise. My brother had returned safely from Vietnam and regained his status as a civilian. He was beginning a career in graphic design in New York City. We had a number of opportunities to bounce ideas off one another, and on a couple of occasions, we met in Boston to hear Ayn Rand's annual Ford Hall Forum Lecture.

Though I was sympathetic to Rand's philosophy, I felt that I might actually have something to contribute on the economics front. The puzzles of boom and bust had an enduring appeal. Times were good in the 1920s and bad in the 1930s. How could times in general be either good or bad, and just what made them so? I jumped into the middle of the ongoing battle of ideas by reading Henry Hazlitt's *Failure of the "New Economics"* (1959). The first few chapters of this critique of Keynes's *General Theory* (1936) were enough to persuade me that I could not read Hazlitt's book with profit unless I first read Keynes's. I had no idea at the time what actually lay in store for me.

In his own preface, Keynes does warn the reader that his arguments are aimed at his fellow economists, but he invites interested others to eavesdrop. As it turned out, even the most careful reading of the *General Theory*'s 384 pages and the most intense pondering of its one solitary diagram were not enough to elevate me much beyond the status of eavesdropper. But Keynes made me feel that I was listening in on something important and mysterious. The ideas that investment is governed by "animal spirits" and that the use of savings is constricted by the "fetish of liquidity" do not integrate well with more sympathetic treatments of the free-enterprise system. Keynes's notion that the rate of interest could and should be driven to zero seemed puzzling, and his call for a "comprehensive socialization of investment" served as a red flag in more ways than one.

With Keynes's mode of argument—though not the full logic of his system—fresh in my mind, Hazlitt's book was intelligible, but his virtual page-by-page critique came across as the work of an unreceptive and hostile eavesdropper. Keynes's vision of the macroeconomy—in which the market tends toward depression and instability and in which the government assumes the role of stimulating and stabilizing it until social reform can replace it with something better—was never effectively countered. Not surprisingly, Hazlitt did point to the Austrian economists as the ones offering the most worthy alternative vision. There were a double handful of references to Hayek's writings and twice that many to Mises's.

After a diet of Keynes, contra-Keynes, and then Austrian economics, I returned to my old principles text to see how I had failed to come to any understanding at all during my undergraduate experience with macroeconomics. In Samuelson's chapters on the macroeconomy, I found a total eclipse of the most fundamental issues. The questions of whether, how, and in what institutional settings a market economy can be self-regulating were crowded out by a strong presumption that self regulation is not possible. The vision of an economy adrift was reinforced by simplistic exercises showing how in a failure-prone circular flow the extent of labor and resource idleness is related to the leakages from (and injections into) the economy's streams of spending.

#### **IV. From Engineering to Economics**

The engineering market was glutted in 1971 when Vietnam was winding down and I was making my exit from the military. Electrical Engineering was second only to

Aerospace in terms of the difficulty of making the transition from guns to butter. An electrical engineer with four-years' experience in warfare electronics could not compete effectively with an entry-level applicant with a fresh degree. One option popular among my peers was to work on an MBA degree while waiting for better times. An EE/MBA was considered to be a very marketable combination. I chose to pursue a masters in economics instead, knowing that the course work would be more interesting and thinking (erroneously) that an EE/MA combination would also be marketable. I applied for admission into the masters program at the University of Missouri at Kansas City. Attempting to compensate for having very little formal undergraduate training in economics, I attached to my application a list of books I had read in preparation for graduate school. The books ranged from Ludwig von Mises's *Theory of Money and Credit* (1912) to Shirley Schiebla's *Poverty is Where the Money Is* (1968). It is a wonder that I was admitted into the program.

At UMKC the most revered economists were the Institutionalists—Thorstein Veblen and Clarence Ayers. But the courses in macroeconomics offered a heavy doses of Keynesianism. Direct references to Keynes's *General Theory*, however, were rare. Instead, his analytical framework was presented in the conventional form of IS-LM analysis, those interlocking diagrams that jointly determine the equilibrium values for the economy's income and its interest rate. Gardner Ackley's *Macroeconomic Theory* (1961) was the assigned text. The substantial investment involved in mastering the diagrammatical technique seemed to give professors and students alike a special interest in defending Keynesian views. Further, the only alternative mentioned was the trumped-up classical model devised by Ackley himself as a foil for understanding and appreciating the revolutionary character of the Keynesian system.

I vividly remember reporting to my brother on the state of macroeconomic pedagogy. I explained how the Keynesians had a virtual lock on macroeconomics. The interlocking graphics yielded up answers to macroeconomic questions, and if the students didn't give those answers, they had no answers at all to give. Hayek was a genuine alternative to Keynes in the 1930s. Forty years later there was a glaring need, Jim and I agreed, for a genuine alternative to the Keynesian graphics.

In late 1972 I began to devise an Austrian counterpart to the Keynesian diagrams. Rothbard's *Man, Economy, and State* (1962) provided the primary source material. In the end, I was able to draw together individual diagrams taken from or inspired by Rothbard, Mises, Hayek, Böhm-Bawerk and Wicksell and show that they all fit together into a coherent story about boom and bust. I wrote a term paper titled "Austrian Macroeconomics: A Diagrammatical Exposition." My graphics were three-dimensional: the Austrian view was represented in one plane, the Keynesian view in another, orthogonal plane. This construction allowed me to show the definitional connections between the two views as well as the key substantive differences.

The professor gave me a high mark on the paper but confessed that he hadn't actually worked through the graphical analysis and wasn't familiar with Austrian

economics. To my surprise, though, he offered to arrange for me to present the paper at the Midwest Economic Association meetings to be held in Chicago in April 1973. With some urging from this professor, I agreed to go to Chicago. I soon realized, however, that neither he nor anyone else had provided me with critical feedback. No one had actually read the paper. And I was to present it to a professional audience in April! The one action item that occurred to me was to send the paper to Rothbard. Maybe he would respond in time to give me some confidence about Chicago—or to allow me to renege on my agreement to go.

About a week after mailing the paper, I got a phone call from Rothbard. He was clearly enthused about the diagrammatical exposition; he saw it as beating the Keynesians at their own game. “Would you be coming to New York anytime soon?” he asked. Although I had no plans whatever to go to New York, I managed to announce: “I’ll be there during spring break,” at which point he invited me for dinner and further discussion of the diagrams.

Dining with Murray and Joey Rothbard in their book-lined upper westside apartment was a pleasant and memorable experience. After dinner more guests arrived for a lively discussion of the paper. There were lots of good suggestions for revision and further development. In the short period between my March trip to New York and the April meeting in Chicago, I was able to make some improvements and gain some confidence.

The presentation in Chicago was a virtual non-event, which, as I soon learned, is typical of sessions at professional meetings. But that disappointment was overshadowed by the fact that Rothbard had invited me to attend a week-long conference on twentieth-century American economic history sponsored by the Institute for Humane Studies and scheduled in the summer at Cornell University. He and Forrest McDonald (now of the University of Alabama) were to lecture for a week to an audience consisting mainly of student historians.

With that conference, which came at the end of my studies at UMKC, I became comfortable with the fact that I had left engineering forever. I had spent the summer of 1972 (the Watergate summer) as an intern at the Bureau of Labor Statistics in Washington D.C. But on finishing the course work for the masters degree, I applied for employment in engineering as well as in economics. In the same week, I had interviews with the Bendix Corporation and with the Federal Reserve Bank of Kansas City. I discovered that an MA in economics was a poor substitute for an MBA in the eyes of engineering firms. With some philosophical reservations, I accepted the job at the Kansas City Fed, delaying my start date until after the Cornell conference. I learned much from the Rothbard-McDonald lectures, but more importantly, my name was added to the invitation list for subsequent conferences.

In June of the following year (1974) I spent a week at South Royalton, Vermont—at an IHS conference that came to be widely recognized as the take-off point for the Austrian Resurgence. There, Murray Rothbard, Israel Kirzner and Ludwig

Lachmann gave lectures dealing with method, theory, and policy, all published in due course as *The Foundations of Modern Austrian Economics* (Dolan, ed., 1976). Henry Hazlitt and Bill Hutt added much insight and perspective to the discussions. Milton Friedman was there for the opening banquet. His now-famous remark that “there is no Austrian economics—only good economics and bad economics” had a certain (but unintended) galvanizing effect on the conference. The list of participants, most meeting one another for the first time, now reads like a Who’s Who in Austrian Economics: Dom Armentano, Walter Block, John Blundell, Richard Ebeling, David Henderson, Jack High, Randall Holcombe, Don Lavoie, Larry Moss, Gary North, Gerald O’Driscoll, Mario Rizzo, Joe Salerno, Sudha Shenoy, Karen Vaughn. One purpose of that conference was to persuade Lachmann that there was sufficient interest in Austrian economics to justify his coming out of semi-retirement and teaching at New York University. By week’s end, the interest was not in doubt, and Lachmann soon began teaching at NYU.

There were two follow-on conferences held in successive years—with Hayek joining the original South Royalton faculty. In 1975 the Austrians met at the University of Hartford in Connecticut; in 1976 they met in England at Windsor Castle. At both conferences, papers by South Royalton participants were presented and discussed. The Windsor Castle papers, among which was my newly revised “Austrian Macroeconomics,” were eventually published as *New Directions in Austrian Economics* (Spadaro, ed., 1978). My diagrammatical exposition has had a limited but enduring success. It was published separately as a monograph by IHS and was excerpted extensively in Duncan Reekie’s *Markets, Entrepreneurs and Liberty: An Austrian View of Capitalism* (1984). It continues to appear on Austrian economics reading lists, was the basis for some discussion in an interview published in Snowdon et al. (1994), and tends to get mentioned in histories of the Austrian school, such as in Vaughn (1994), and in survey articles, such as in Kirzner (1997).

## V. Interlude: The Federal Reserve

Spending time at the Federal Reserve was an important part of my education. But apart from occasionally being on “burn detail”—destroying soiled or torn currency—I was not directly involved with monetary matters. I was in the Division of Bank Supervision and Structure, where applications for the acquisition of banks by bank holding companies were processed. In play here were the issues of industrial organization. The Federal Reserve, in effect, was serving as trust buster for the banking industry. The focus in each case was on market share—although there was no benchmark market share according to which some actual market share could be judged harmless or worrisome. And the numbers quantifying the change in market share were very sensitive to the definition of the market area, a definition that itself was almost wholly arbitrary.

Suppose that Missouri’s largest bank holding company, whose lead bank is in

Kansas City, wanted to acquire a small bank in nearby Blue Springs. The holding company would press for a definition of the relevant market area that minimizes the impact of the proposed acquisition. It would argue one of two extremes—that there would be no change in the market-share figures because the relevant market areas were so small that the Kansas City bank and the Blue Springs bank were in two different market areas; or that there would be only a minuscule change in the market-share figures because the lead bank and the target bank were located in a very large market area (the whole state of Missouri) in which many banks competed. The Federal Reserve would usually opt for some middling market-area definition—using rivers (despite bridges), county lines, and even highways as market-area boundaries. But I don't recall a case where the change in market share was judged to be significant. The Fed didn't bust many trusts.

Evaluating an application always entailed interviewing the CEOs of other small banks that competed with the target bank. Tellingly, the reactions of the target banks' competitors were either grave concern or seeming indifference: "How are we supposed to compete with the largest bank holding company in the state!?" Or, quite often: "The acquisition is fine with us; we'll compete"—which really meant that this bank was currently in the negotiation phase with the second largest bank holding company in the state. While traveling around the Federal Reserveâ™s tenth district for on-site visits (mostly in Missouri and Colorado), I was also reading Israel Kirzner's *Competition and Entrepreneurship* (1973) in preparation for attending the South Royalton conference. I had trouble, to say the least, trying to reconcile what I was reading with what I was doing.

In between applications, I had time to pursue my macroeconomic interests. Did the boom during the 1920s have a significant policy-driven component? There was no dispute about there being significant real growth during that decade. But what about money growth and its effects on prices and on interest rates—and hence on resource allocation? I juxtaposed two key books giving contrasting answers to these and related questions, both published in 1963: Friedman and Schwartz's *Monetary History of the United States* (1867-1960) and Rothbard's *America's Great Depression*. It became clear that, with minor variation, both books relied on the same data: Friedman and Schwartz reported the annual growth rate of the money supply for the 1920s and took it to be consistent with macroeconomic stability. Rothbard reported the cumulative growth of the money for an eight-year period (1921-1929) and saw it as alarmingly high.

Clearly, the difference between Friedman and Rothbard lay in their contrasting interpretations of Federal Reserve behavior during the 1920s. Friedman took the constant or near-constant price level that characterized the decade as evidence of monetary stability. Money creation was accommodating real growth. Rothbard looked at the effect of this accommodation on interest rates and hence on the intertemporal pattern of investment. In his judgment, the credit expansion resulted in widespread

intertemporal disequilibrium. I would later learn that Hayek, who was watching the Fed in the early 1920s, began work on an NYU Ph.D. thesis on the question "Is the function of money consistent with an artificial stabilization of purchasing power?"—a question that could only be puzzling to the monetarists.

The difference between these two books was a reflection of their competing analytical frameworks. But in each case the analytical framework was not well-defined. Friedman (1970) had attempted to set out the differences between himself and the Keynesians by expressing these two sets of ideas in the IS-LM framework. Was this Keynesian framework also the monetarist framework? If not, then just what—beyond the equation of exchange plus a money-demand equation—was the monetarist framework? The Austrian analytics seemed to revolve around the intertemporal structure of production as depicted by the Hayekian triangle, whose changing shape reflects marginal adjustments in the roundaboutness of the economy's production activities. Though vital, this little piece of pedagogy, by itself, hardly passed muster as an analytical framework.

Dating from the South Royalton conference, my interests in the analytical framework of Austrian macroeconomics grew. Lectures at that conference by Ludwig Lachmann, though somewhat cryptic, further convinced me that it was capital theory that made the difference between the Austrians and mainstream schools. And having listened to the lectures by Israel Kirzner, my interests in market areas and market shares were decidedly on the wane. Besides, with only a masters degree, I did not have a bright future with the Federal Reserve. It was time for me to go. I had been reluctant to continue my graduate studies mainly because I did not know which doctorate programs might be worthwhile. But I now knew of three possibilities—all suggested to me by Rothbard: New York University, UCLA, and the University of Virginia.

Other events and opportunities at the time spurred me on. Sudha Shenoy, whom I had met at South Royalton, arranged for me to attend the Hayek-led Mont Pélerin Society meetings in September of that year. Those meetings were held in Brussels, just three months after South Royalton. And in the following month, Hayek was awarded the Nobel Prize for his early work in industrial fluctuations. In November I attended the Southern Economic Association meetings in Atlanta in order to discuss with George Pearson a possible fellowship arrangement. If I was inclined to leave the Federal Reserve and resume graduate studies, the Institute for Humane Studies, then in Menlo Park, California, would accept me as a resident fellow during the summers. With that opportunity, I became a short-timer in Kansas City. I did no search for other graduate programs beyond the three that Rothbard had mentioned. But having no strong desire to live in either New York City or Los Angeles, I headed for Charlottesville.

## **VI. Virginia: Preparing for an Academic Career**

Virginia's economics department in the mid seventies was in decline. Jim Buchanan

and Gordon Tullock had left. Warren Nutter had fallen ill, and the few others to whom UVA owed its good reputation (for its adherence to the principles of classical liberalism) were on the political outs. The department was beginning to turn towards mathematical economics—though not as much so as was the general trend in those years. In the field of macroeconomics, there wasn't much sympathy for Keynesianism, except in the first-semester core course. That course was taught by a visiting professor who was so inept and narrow-minded as to make Keynesian constructions monumentally unappealing. Monetarism and its offshoots (new classicism and rational expectations) were considered to be the mainstream and the cutting edge of monetary theory.

The propensity to mathematize was eroding the Virginia tradition. The math never got in my way, thanks to my engineering background, but I had no inclination to take a purely mathematical approach to economics. Having learned Lagrangian multipliers and the like separately from the substantive content of economics had a definite payoff. That training sequence keeps the mathematics in perspective—which, in many cases, is to say that it keeps the math at bay. In macroeconomics, what often matters most are the institutional arrangements, such as the fact that new money enters the economy through credit markets and hence impinges in the first instance on interest rates. And changes in the interest rate have a predictable effect on the pattern of investment—as can be demonstrated by those present-value calculations I learned in Engineering Econ.

For me the shining light at Virginia was Leland Yeager, whose monetary disequilibrium theory (Yeager, 1997) derives from the pre-Friedman monetarism of Clark Warburton. Yeager taught a course in monetary theory using Don Patinkin's *Money, Interest, and Prices* (1965), a book well anchored in name and in deed to Knut Wicksell's *Interest and Prices* (1898). It was the Wicksell-Patinkin connection that inspired my dissertation. Wicksell had called attention to the “cumulative process” that is set into motion by a bank rate of interest held below the market, or natural, rate. I could trace the Wicksellian insights in two different directions—to Patinkin and to Hayek—showing how one direction of development emphasized the sense in which money is neutral while the other emphasized the inherent non-neutrality of the process through which the economy adjusts to a monetary expansion. The key difference between the two different directions of development was capital theory—its wholesale neglect in Patinkin's framework and its centrality in Hayek's.

While in residence at the University of Virginia, I had the good fortune to serve as a Teaching Assistant to Kenneth Elzinga and Bill Breit, a peerless duo who taught Virginia's two-semester sequence of microeconomics and macroeconomics. Their performances in the undergraduate classroom, together with Yeager's performance at the graduate level, were a virtual how-to course for budding academics who themselves would soon be teaching.

Of course, making the transition from graduate student to professor required actually completing the dissertation. I was able to work on mine at the Institute for

Humane Studies in 1977 and 1978 when, as luck would have it, Hayek was in residence there, too. After having been occupied with the broader issues of political science for more than three decades, Hayek was beginning to think again about the issues of money and macroeconomics. He had just published his *Choice in Currency* (1976) and had expanded it into the *Denationalization of Money* (1976).

Participating in the UCLA Oral History Program, Hayek had reaffirmed his early views about the nature of business cycles. And he wrote an inspiring Foreword to Gerald O'Driscoll's *Economics as a Coordination Problem: The Contribution of Friedrich A. Hayek* (1977). O'Driscoll, a UCLA graduate and IHS resident fellow, had demonstrated in his dissertation the unity of Hayek's work. Hayek found the demonstration satisfying and even moving: “That it seems in principle possible to recast a great part of economic theory in terms of the approach which I had found useful in dealing with such different problems as those of industrial fluctuations and the running of a socialist economy [is] gratifying to me.... Professor O'Driscoll has almost persuaded me that I ought to have continued with the work I had been doing in the 1930s and 1940s rather than let myself be drawn away to other problems which I felt to be more important.” Hayek was an inspiration for me to finish the dissertation and to press on with the research agenda that had occupied him during his early LSE years.

IHS provided opportunities to teach Austrian macroeconomics. Starting the year of the Windsor Castle conference, IHS ran a teaching conference almost every year—at the University of Delaware (1976), Mills College of Oakland, California (1977), the University of Colorado (1978-1980), and Marquette University (1982, 1984, and 1986). I was a Teaching Assistant at the Delaware conference and was on the faculty at all the others—along with Israel Kirzner, Jerry O'Driscoll, Mario Rizzo, and a number of guest lecturers.

## VII. Auburn: A Base of Operations

In the late seventies the Department of Economics at Auburn University was in a building phase. The strategy was to attract low-tech market-oriented economists. The department attracted me in 1978. “Market orientation,” it turned out, got translated into “not much macroeconomics.” The department proudly described itself as a place to learn low-tech applied microeconomics. In my early Auburn years I taught Principles of Microeconomics and History of Economic Thought as well as Business Conditions Analysis in the MBA program. But soon enough, as one of the few macroeconomists on the faculty, I saw my teaching repertoire narrow to macroeconomics at all levels—principles, upper-level undergraduate, and graduate.

In 1981 I took leave from Auburn to spend a semester at New York University as a Visiting Fellow. The attraction there was the weekly Austrian Economics Colloquium, the active participants including Israel Kirzner, Ludwig Lachmann, Jerry O'Driscoll, and Mario Rizzo. It was during that visit that I began writing an article that

eventually appeared as “Time and Money: The Universals of Macroeconomic Theorizing” (1984). Capital-based macroeconomics integrates capital theory, which captures the critical time element in the macroeconomy, with monetary disequilibrium theory, which identifies the source of macroeconomic problems. Precisely because of the time element, disequilibrium affecting the economy’s capital structure can persist. Problems can fester until the disequilibrium begets a crisis and downturn. An aphorism emerged that captures the idea: Capital gives money time to cause trouble. The article is widely cited (in Austrian circles) and has been reprinted in various volumes of collected essays.

The Ludwig von Mises Institute attached itself to Auburn University in 1983 and soon enticed Leland Yeager to leave Virginia to become Auburn’s Ludwig von Mises Professor. Though critical of those very aspects of the Austrian theory that I have always found most attractive, Yeager has been a continuing and significant force in shaping my ideas. My research and writing in the 1980s and 1990s can be characterized as a piecemeal approach to resolving the key issues separating the various schools of macroeconomic thought.

In one instance, I collaborated with an Auburn colleague to pit Hayek against Friedman. Don Bellante, who was also my immediate successor as an NYU Visiting Fellow, is a labor economist with a macroeconomics orientation. He and I had similar views about the disruptive effects of monetary expansion. Our separate strengths were very much complementary—his in critically expositing the dynamics of the Phillips curve, mine in expositing the dynamics of the Hayekian triangle. We conspired to write an article titled, “Phillips Curves and Hayekian Triangles: Two Perspectives on Monetary Dynamics” (1988). In terms of both logic and implications, we found Austrianism to be superior to monetarism. (This publication represents one of the few instances where co-authoring seemed to work for me. Generally, I prefer to work on my own, my aversion to co-authorship being similar to my aversion to teamwork in the undergraduate engineering labs at the Missouri School of Mines.)

Some years later, I put the dynamics of the Hayekian triangle in play in my Comment on Friedman’s ‘Plucking Model’” (1996). Curiously, Friedman (1993) had taken the observed temporal pattern of total output to be a basis for declaring irrelevant all theories that suggest a cause-and-effect connection between boom and subsequent bust. His critical remarks, explicitly directed at the Austrian theory, would be equally telling, of course, against his own short-run/long-run Phillips curve analysis.

In another critical article, “The Persistence of Keynesian Myths: A Report at Six Decades” (1995b), I employed a production possibilities frontier (PPF) to highlight the essential difference between Keynesian and Austrian constructions. I discovered later that Abba Lerner (in Colander and Landreth, 1996) had made the same distinction early on: For Keynes, consumption and investment are magnitudes that move up and down together, their path of possible movements intersecting the PPF; for the classical economists, including Hayek, those two magnitudes can be traded off against one

another—as can be depicted as movements along a PPF.

In an encyclopedia entry on the “The Austrian Theory of the Business Cycle” (Garrison, 1997a), I focused on the market for loanable funds to make the key Austrian distinction between sustainable and unsustainable growth. An increase in saving shifts the supply of loanable funds rightward, lowers the rate of interest, and sets the economy off on a steeper growth path. By contrast, credit expansion pads the supply of loanable funds, drives a wedge between saving and investment, and sets the economy off on an unsustainable growth path.

These three simple graphs—the Hayekian triangle, the production possibilities frontier, and the loanable-funds market—all came together for me in the classroom at Auburn. They jelled into a capital-based macroeconomic framework that was much superior to my earlier diagrammatic exposition. The 1984 article lends its title to my book-length treatment of the issues: *Time and Money: The Macroeconomics of Capital Structure* (2001). The analytical framework of interlocking graphs, first assembled to demonstrate the logical coherence of Hayek’s business cycle theory, turned out to have much broader application—in analyzing the fiscal issues such as deficit finance and tax reform and in sharpening the contrast between the Austrians’ capital-based theory and Keynes’s labor-based theory.

### VIII. Capital-Based Macroeconomics on Tour

I took a sabbatical in the fall of 1997 to speed the progress on the book. Coincidentally, I was invited to deliver the Ludwig Lachmann Memorial Lecture at the University of the Witwatersrand in Johannesburg, South Africa in late August of that year. The trip to South Africa quickly developed into a lecture tour with stops at the University of Capetown and the Reserve Bank in Pretoria. I used the Lachmann lecture to deal with the issue of expectations in macroeconomics. This was always Lachmann’s favorite topic—and in light of the dominance of rational-expectation models in mainstream theorizing, one that had to be addressed head-on. That lecture (Garrison, 1997b) became Chapter 2 in my book. At Capetown, I presented the capital-based framework to a very receptive audience and put it through its paces in the context of boom and bust.

The visit to the central bank in Pretoria, which was arranged by South Africa’s Free Market Foundation, was an occasion to make the distinction between a Fed-led boom and a debt-led boom. The economic expansion that began during the Reagan administration was spurred on by a Treasury that was borrowing money rather than by a central bank that was creating money. Still, the boom was an unsustainable one, though the dynamics were different. Investments become excessively speculative rather than excessively roundabout. The Free Market Foundation asked me to write up the lecture for publication as a monograph. My *Chronically Large Federal Budget Deficits: The American Experience* (1998) appeared in print just as the Clinton

administration's Fed-led boom turned the budget from deficit to surplus. The timing dampened interest in the monograph, but the arguments were incorporated into Chapter 6 of my book.

For me, the most satisfying experience came by way of an invitation to be the first Hayek Visiting Fellow at the London School of Economics. The visit, during May/June 2003, was arranged by Toby Baxendale, a London businessman, and was sponsored by LSE and the Mises Institute. The highlight of that five-week visit was a well-attended public lecture in LSE's Old Auditorium. Seventy-two years after Hayek gave the lectures that became *Prices and Production*, I was able to give a lecture that was inspired by that book, telling the story of the business cycle with the aid of a capital-based analytical framework and putting Keynes and Hayek head-to-head in a way that had not been done before.

**IX. Prospects and Perspective** The Mises Institute has given me many opportunities to fine-tune the pedagogy in presenting the Austrian theory of the business cycle and, more generally, capital-based macroeconomics. Starting in the mid 1980s with a pilot program in Auburn, the Institute has organized teaching conferences—first at Stanford University and Claremont University in California and, since the mid 1990s, at Auburn University. Those conferences, as well as the Institute's annual Austrian Scholars Conference, have been important in developing the theory and pedagogy that appears in *Time and Money*. Similar benefits have come from my participation in the teaching conferences organized by New York University and, more recently, by George Mason University and held each year at the Foundation for Economic Education in Irvington-on-Hudson, New York.

*Time and Money* continues to be a source of ideas for development for me and for others. I have become intrigued with the puzzles of the associated doctrinal history, such as reconciling the concepts of "Forced Saving and Overconsumption in the Mises-Hayek Theory of the Business Cycle" (2004). And I note with some satisfaction that the term capital-based macroeconomics seems to have found its way into the lexicon of macroeconomics as a more substantive name for analytical framework that was once identified only by its country of origin.

The story told in this essay, though involving a roundabout progression, seems to entail a greater linearity and singularity of purpose than was ever actually evident along the way. And although it is true that, when in an academic frame of mind, my thoughts are dominated by time, money, capital, and interest, these macroeconomic concepts and the theories we can build with them are kept in perspective by more worldly matters—by the memory of my parents, by conversations with my brother, and by spending time with my twelve-year-old son Jim and with my wife Karen, who is also my best friend.

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