Professional Specialization: Why Labour Market Changes Have Undermined the Role of Economic History in Economics Departments

A Paper to Be Delivered to the “Future of Economic History” Conference, University of Guelph, October 18, 2003

Carl Mosk *
Department of Economics
University of Victoria

* e-mail: mosk@uvic.ca
homepage: web.uvic.ca/~mosk/mosk-hp.html

I am grateful to Malcolm Rutherford for his guidance concerning the history of economic doctrine and the place of economic history in that history.
I The Dismal Economics of the Decline of Economic History

Labour market specialization – in the market for academics, in the market for college and university graduates – has steadfastly rendered the position of economic history within economics departments precarious. A powerful swell of professionalism, especially pronounced in the aftermath of World War II, is swamping the intellectual diversity of economics departments in its mighty wake, submerging economic history and the history of economic thought.

As a practicing economic historian with manifest self-interest in the future of economic history I feel compelled to decry this tendency. But – cold comfort indeed – I can take refuge in labour economics. As an economist who has examined the sway labour markets hold over higher education, I can understand, appreciate but not applaud, why it is happening. [1]

That I not be accused of being overly dramatic in asserting that economic history is being marginalized in economics let me be clear about my reasoning. I am not saying that economic history is being eliminated from the curricula of economics departments; I am not saying that publication in economic history journals by members of economics departments is disappearing.

While it is true that some departments have phased out economic history as a field, a number of the major Ph.D. granting departments in North America and the United Kingdom continue to graduate doctoral candidates who work in the field (for instance, Harvard, several campuses of the University of California, Northwestern University.)

Economists specializing in the field continue to publish in journals like the Journal of Economic History, Explorations in Economic History, and the Economic
History Review. The Economic History Association holds an annual meeting; Cliometrics has a special session in the Allied Social Sciences Association annual meeting; the annual gathering of the Social Sciences History Association is an umbrella under which economic historians gather; and conferences focused on particular themes, like this one, take place regularly. Two economic historians, Robert Fogel and Douglass North, shared a Nobel Prize in Economics, and in recent years Fogel was the president of the American Economic Association.

What I am talking about is perhaps less tangible but nonetheless important: being taken seriously by the profession of economists. Despite assertions by economists that economic history continues to be an important part of the training and reasoning of economists, I sincerely doubt that it is. True, as I shall point out in the next section, in assembling long-run time series, empirical economists, especially macroeconomists and applied econometricians, draw upon the literature of economic history, mainly in putting together data sets for testing models. But economic history brings to the table more than data, more than a focus on particular issues of interest to economists like the history of the gold standard or the causes of the great Depression. By its very nature as a field that focuses upon particular historical events or sequences of events, economic history is eclectic in focus and inductive in methodology. It brings to the table a way of thinking, one that is not consistent with the way of thinking that increasingly dominates economic departments in North America, the United Kingdom and Australasia.

Differentiating itself from other fields by the drive to erect intellectual walls that define and delineate the professional economist, the mindset of the economist (the way of
looking at the world in a very specific way) has narrowed. Thinking like an historian is not longer part of the program. At one time it was; but no longer.

Arguing this point involves establishing several propositions about the intellectual history of the discipline of economics. Showing that a number of illustrious economists from the past made heavy use of the historian’s mode of thinking is one piece of my argument. Demonstrating that this way of thinking is being driven out of the mainstream journals by mathematical modeling cum econometrics is a second part of my argument. Finally, showing that these changes can be attributed to professional specialization, in publishing and research, and in teaching and curriculum development, constitutes the third component of this analysis.

In thinking about the third component of my argument it is useful to conceptualize what I am saying in terms of the supply of academic economists (trained in a certain way on the basis of what key journals in the field define as acceptable research), and the demand for their services as teachers. To some extent this distinction between supply and demand is artificial. After all, at one time every professor was an undergraduate student, so his or her experiences as a student influenced his or her attitudes toward professional specialization later on. Still, making the distinction facilitates my discussion. So I stick to it.

II What Does Economic History Bring to the Table?

To many contemporary economists, economic history is simply one of the applied sub-fields of the discipline. Every sub-field has its own little quirks. In the eyes of many professional economists, the quirks of economic history are an emphasis on historical continuity (known as “path dependence”), a focus on applying mainstream theory to
questions addressed by historians, concern with the quality of data in general and historical data in particular, an appreciation of the various reasons why records of events are unreliable, and an abiding interest in long-run social evolution, therefore a fascination with evolutionary models that they share with followers of Institutionalism, past and present.

To an important degree economic historians have only themselves to blame for this state of affairs. The new economic history, especially Cliometrics, was a program of research and teaching launched in the 1950s by economists concerned with showing that their training as professional economists, versed in econometrics and economic theory, would and could revolutionize history. The key point is that they were nervously looking over their shoulders: will my colleagues think that I am “real economist, a true professional”? To be a professional economist I need to use the latest intellectual tools of the profession narrowly defined.

But historical thinking is something altogether different. It is eclectic. Any consumer of history well written appreciates this point. It looks at particular events, people or practices out of the past in a rounded, holistic fashion. What did people eat? What did they read? How did they speak? What ideologies motivated their behavior? Was religion important to their worldview, and if so, how and why?

It is also inductive. A history well crafted builds through an accretion of facts carefully assembled. A stonewall made up of hundreds of small separate rocks, the carefully fashioned history is a construct of many small things that are orchestrated in an assemblage greater than the sum of the parts. But the parts matter, the smaller and more variegated the better. Big points, grand themes, are won out of a painstaking sifting
through masses of detail—oral accounts or written down stories, numbers recorded in business accounts and scribbles in farmer’s almanacs, diaries, literary sources, posters, drawings—these are the grist of the historian’s imagination. To grasp the historical uniqueness of particular events, lives lived, books written, music composed—to capture historical essence—that is the point.

Some economists, but not most of the new economic historians, get the point. One of the most celebrated theorists of the post-World War II period Kenneth Arrow (1985) admits to an abiding interest in reading history, and writes of the impression that a lecture by a distinguished historian Leonard Krieger made on him:

“…[According to Krieger] history could not be regarded as simply a branch of social science. Its aims were different. It sought to study the individual case, while social science aimed at general principles.” [4]

As Arrow notes in his article, economics and history are like Maine and Texas connected by a telegraph: connected perhaps, but “does Maine have anything to say to Texas?” Most contemporary economists do not care about communicating with Maine. Safely ensconced in Texas “the most insular of the social sciences,” they are content to import tools and concepts from mathematics and classical physics on occasion, but are mainly content to keep Texas pure and unsullied, certainly uncontaminated by “Maine talk.” [5]

This was not always true. It is the thrust of the next section, that many of the great economists of the past, including the relatively recent past, took history seriously in doing their research, and writing up their results. The divorce of the economic and historical mindsets is of relatively recent vintage.
III The Intellectual Fate of Four Famous Economists Who Took History Seriously

In this section I examine the distribution of post-1950 citations in a subset of economics journals to four famous economists (all deceased now) who took historical methodology and historical thinking seriously. These four economists are: Joseph Schumpeter, Simon Kuznets, Alexander Gerschenkron and Charles Kindleberger. The point I will attempt to convince you of is that there is a secular drift in the citations between the 1950s and the 1990s. During the 1950s, citations to these four economists tended to be concentrated in the four most prestigious journals: the American Economic Review (A.E.R.), the Quarterly Journal of Economics (Q.J.E.), the Journal of Political Economy (J.P.E.), and the Economic Journal (E.J.). By the 1990s, relatively few of the citations appeared in these four journals.

Why does this matter? It matters because these journals are read by a large proportion of the community of professional economists working in the English language. It matters because other economics journals are not widely consumed by the general audience of professional economists. Citations in the four prestige journals direct economists, budding and fully budded, to the views of predecessors especially meriting attention in the eyes of the protectors and purveyors of professional identity. [6]

Before jumping into my analysis of the secular shift, something needs to be said about each of my prominent economists and why and how they were steeped in the historical imagination. [7] It is my contention that all four scholars fully imbibed the historical approach in its dense complexity. All four worked inductively; all four appreciated the importance of historical turning points, secular shifts in the historical
process; all four appreciated how the “laws” of economics were shaped by specific historical situations.

While all four scholars I discuss are notable figures in the field and therefore unique in their own ways, their use of history is in keeping with their times. Prior to the 1950s many academic economists taught in departments that included historians and political scientists. Not surprisingly, they were influenced by the ideas and approaches of their colleagues. After all, they shared space and conversations around the water cooler, and had joint responsibility for creating a common curriculum for their departments.

David Dewey, the first editor of the *American Economic Review*, was not an economic theorist. His main scholarly contribution was writing a *Financial History of the United States*.[8] One of the founding fathers of the National Bureau of Economic Research was Wesley Clair Mitchell, a major proponent of Institutionalism and a staunch believer in induction rather than deduction for empirical analysis.[9] Analyzing the academic scene in Germany and the United Kingdom from the 1850s to the 1970s, Hartwell (1973) argues that many of the leading figures of European economics (Hildebrand, Schmoller, Sombart, Unwin, Clapham, and Tawney) were deeply steeped in history, building their theories of economic activity on the basis of historical evidence.

Proceeding in chronological order (by date of birth), consider Schumpeter.[10] Most famous for his “entrepreneurial” theory of economic change through a four-step process of diffusion of new technologies and organization forms – invention, innovation, imitation and creative destruction – Schumpeter advanced an influential theory of industrial organization that justified oligopolies, monopolies and even socialism managed by technocrats. Schumpeter saw the dynamic of capitalism as something relatively new in
history, its dawn associated with the first Kondratieff wave in England, something that might well have a sunset, giving way to socialism.

That Schumpeter amassed immense amounts of historical evidence in defense of his theories is well known to the readers of *Business Cycles* and *Capitalism, Socialism and Democracy*. Indeed, in his appreciation of Schumpeter, Haberler (1950: pg. 333) writes:

“He himself used to say that an economist who is not also a mathematician, a statistician, and most of all a historian, is not properly qualified for his profession. He was all these and more besides: he had an encyclopaedic [sic] knowledge not only of the history of economic doctrines, which was one of his special fields, but also of the history of economic facts and institutions and of general political and social history.”

One of the greatest economists of the twentieth century was historian first, statistician second, and mathematician third. This should not be forgotten.

My second illustration is Simon Kuznets (1901-1985), famed for his pioneering work in national income accounting (carried out for the National Bureau of Economic Research under the direction of Mitchell), and for his development of the theory of Modern Economic Growth that has profoundly shaped the field of economic development in the post-1950 period. All who have read the works of Kuznets closely, books like *Secular Movements in Production and Prices*, *Modern Economic Growth* or *Economic Growth of Nations*, will be aware of his emphasis on the inductive method, and the careful unrelenting probing of the data upon which generalizations about the universality of the “great constants and laws” of economics have been erected.
Indeed, the critics of Kuznets constantly referred to his emphasis on “facts without theories;” his rejoinder was that economics, especially post-1950 economics, was becoming “theories without facts.” [12]

A second great figure of twentieth century economics, Kuznets, like Schumpeter, was more historian than theorist. Indeed, despite being president of the American Economic Association and winner of the Nobel Prize in Economics, in the eyes of many economists, he is the arch “anti-theorist” of economics, not really an economist at all.

So goes economics: heaven forbid Texas should talk to Maine.

Perhaps my other two examples – Gerschenkron and Kindleberger – are better known more for their contributions to economic history than to economics broadly defined. Nevertheless, as I shall show, their views were well received in the four prestige journals of economics during the 1950s and the 1960s.

Alexander Gerschenkron (1904-1978), who taught European Economic History at Harvard from 1948 until the mid-1970s, was mainly known for his theory of economic backwardness, the “rubber-band” thesis that argues that there is a tension between what economic development offers and the economic backwardness of less developed countries, one that may be released in a great “growth spurt.” [13] Motivated by a strong distaste for stage theories like those of Marx implying that history moves in linear teleological fashion, the economic advance of late developers mimicking the patterns of early industrializing powers like the United Kingdom, Gerschenkron emphasized the importance of a pattern of substitutes for the missing “primitive accumulation of capital” that occurred in England and was a keystone of Marx’s Capital. In Germany, financing by the big three investment banks substituted for accumulation by merchants and factory
owners; in Czarist Russia under Finance Minister Witte, government played a crucial role in early capital accumulation. In generalizing from the England-Germany-Russia triumvirate, Gerschenkron was using the sequencing of history to build a general theory of economic development.

There is no doubt that Gerschenkron was a formidable scholar, steeped in foreign languages, enjoying an intimate knowledge of literature and history (and baseball!), rivaling Schumpeter. Rosovsky (1979: page 1013) employs Gerschenkron’s own words, penned for an introduction to a translation of Eli Heckscher’s Economic History of Sweden, to eulogize Gerschenkron:

“His immense erudition, his classical background, his modesty, his fierce independence, his willingness at all times, in the words of his beloved Horace, to step on the treacherous ashes covering the smoldering fire of conflict and controversy, and above all, his supreme sense of duty – these qualities of a very great scholar are less readily pronounced by our age of anxiety and instability.”

There is no doubt that Gerschenkron was historian and economist, a scholar able to integrate the two fields, generating compelling theoretical propositions from the juxtaposing of Maine and Texas.

My last illustration of the economist who fully grappled with history is Charles Kindleberger who taught at the Massachusetts Institute of Technology for many years, remaining active as a scholar until his recent demise. Kindleberger is probably best known amongst economists for authoring/co-authoring a widely used international trade textbook, and for his contributions to the theory and empirical analysis of trade and the role of multinational corporations in promoting trade in the post-1950 era. Perhaps less
well known is his theory of why hegemonic power is necessary for fashioning and maintaining a relatively stable international economy order, and his cogent dissection of manias and bubbles. Kindleberger’s writings – The World in Depression, 1929-1939 is a prime example – sparkle with the kind of minute historical details that historians delight in. More important, he emphasized the importance of ephemeral and particular conditions like the political groundswell for Free Trade in England in the mid-nineteenth century for the promotion of anti-tariff ideology in Western Europe in the period 1850-1875.

Although fully schooled in the theory of comparative advantage and its various reworking as Heckscher-Ohlin theory and later Samuelson-Stolper theory, Kindleberger never lost sight of the particular, the chance combination of events shaping the evolving chronology of economic events, the evolving structure of international trade and the ups and downs of asset markets.

To reiterate: the four economists I focus upon made major contributions to economics in the English speaking world on the basis of worldviews thoroughly informed by the historical imagination. How have their writings and ideas influenced the mainstream economist in that world?

To measure this influence, I look at the distribution of citations to these four economists in the 26 journals – 27 if one counts Brookings Papers on Economic Activity as a listing distinct from Brookings Papers on Economic Activity – given in the JSTOR archives for economics journals (website: http://www.jstor.org). I compute the percentage of the citations appearing in the four most prestigious journals (A.E.R., Q.J.E., J.P.E., and the E.J.), the percentages appearing in the two economic history journals archived in JSTOR (Journal of Economic History...
and the *Economic History Review*), and the percentages in “other” (the remaining 20 or 21 journals in JSTOR.) The question I address with my classification of citations is simple: has the “influence” of these four prominent economists who were strongly influenced by history and its methodology changed over time, shifting away from the mainstream publications, shifting towards economic history journals in particular?

Speaking personally and polemically, I think this procedure is preferable to the approach employed by Bronfenbrenner (1966) and Coats (1971, 1993) that rely upon the American Economic Association’s (A.E.A.) classification of professional economics articles appearing in its *Index of Economic Journals*. The problem with the A.E.A.’s classification is that it is discrete, “zero-one” as it were. An article must be fit into just one box, even if its content and methodology spills over into many boxes. My method, arbitrary because it is based upon my particular selection of economists, has the virtue of measuring the “influence” of historical thinking in a more subtle, less classification driven, manner. Of course, every choice has a cost associated with it. Many of the citations I count here may be ideas that have little or nothing to do with historical reasoning per se. It would be stupid to deny this.

My results appear in Table 1. [Table 1 about here.] As the reader can glean from the percentages, there is a tale told by the numbers. During the 1950s, over half of the citations to the works of the four authors (including articles by the author himself) appeared in the four prestige journals. Over time, this percentage fell, by the 1990s hovering around 32% for Schumpeter and Kuznets; around 20% for Kindleberger and Gerschenkron. In the case of all four economists, the percentage of citations appearing in the two economic history journals tends to increase through the 1980s, then fall-off
somewhat during the 1990s for Schumpeter, Kuznets, and Gerschenkron. Not surprisingly, the trend toward citing in economic history publications is the most pronounced for Gerschenkron, whose career was most strongly wedded to the field.

Still, despite differences between the individual scholars, what is remarkable is the similarity in the trends. Using the word “marginalizing” to describe what happened is too strong. Indeed, citations to these four economists held up quite well over-all, less in the case of Gerschenkron, most in the case of Schumpeter. It is difficult to avoid the conclusion that the historical imagination is diminishing in influence, especially in the mainstream journals that are the touchstone of the field of economics, the standard-bearers for professional publication, the models for prestigious placement of academic writing.

Demonstrating this is not difficult. But how do we understand why it has occurred? It is the burden of the remainder of this paper that the reasons involve professional specialization: specialization within the community of academic scholars; specialization in the community of students enrolling in courses that academic economists teach. Let us turn to each of these factors in turn.

IV Keeping the Barbarians from the Inner Sanctums: Professionalism and Professional Expansion

Becoming an academic profession means building walls, staking out intellectual territory, establishing clear and enforceable criteria for credentialing new entrants. Economics, like sociology, history, political science and philosophy has found itself pulling in its boundaries, narrowing its scope, and reducing the territory it claims hegemony over, even as some of its most celebrated members – Gary Becker’s
“hegemonic” views will be discussed below – crow about a universality for the
“economic approach” that it has no right or credibility to claim. \[14\]

The narrowing of economics from the viewpoint of academic publishing is most
prominently associated with the mathematization of economic theory and the blossoming
of econometrics as a technical field establishing statistical techniques appropriate for
analyzing economic data sets, especially time-series and panel data. Prior to World War
II, the drift toward mathematical presentation of economic reasoning was relatively
modest. After the war it accelerated dramatically. \[15\]

Why does the war represent such a clear structural break? The war itself played a
role. As never before, American economists went to work for the federal government,
and British economists for Whitehall. In that work they were increasingly drawn into
using their analytical skills to work on economic problems, ranging from projecting
national income with statistical techniques to developing the field of linear programming
and operations research with algorithms that could be applied to efficient allocation of
gasoline, airplanes and pilots for bombing campaigns. The technical complexity of global
warfare accelerated rapidly after 1939, and economists were increasingly caught up in
waging it with a dizzying array of techniques, a glittering plethora of toolkits. The
technocratic impulse was given a strong fillip during the war.

The growth of the profession was a factor. Using figures for membership in the
A.E.A. taken from various years of the Handbook of the American Economic Association
and from the American Economic Review (for December 1985, December 1993 and
December 1997), I have computed the following index \(1969 = 100\) for A.E.A.
membership: 1938 (14.7); 1942 (19.1); 1948 (29.9); 1956 (44.0); 1964 (59.2); 1966
From these figures it is apparent that the ranks of the profession grow extremely rapidly during the 1950s and 1960s, the growth tapering off after 1970 or so.

Why is growth important? Growth is important because as the number of professional economists increased, especially in academic positions (around 70% economist jobs during the 1980s and 1990s were in academia), the pressure to increase new journals increased. After all, if there were only a handful of journals, where were aspiring scholars going to place their articles? Proliferation of specialized journals servicing sub-fields of the profession allowed the mainstream journals to homogenize their product, focusing on developing a standardized model for the “mainstream academic article in economics.”

Consider the following dates at which the 27 economics journals listed in JSTOR were established. Of these 27 journals, thirteen (about half) were founded during the 1930s or earlier; during the 1940s two were created; in the 1950s, one; during the 1960s, five; in the 1970s, two; and in the 1980s four (none were created during the 1990s.) The point is that the 1960s, when the profession was growing by leaps and bounds, was a particularly prolific era in spawning new journals.

Not only was the number of academic jobs booming. Colleges were upgrading into universities. Becker (1997) who does an excellent job of tracing this change gives the example of the College of Saint Thomas, which morphed into the University of Saint Thomas shifting from being a liberal arts institution into an organization lavishing its attention and resources on developing graduate and professional schools. As a result of
this structural shift, the generalist scholar who published a few articles at best and then turned to teaching gave way to the professional who concentrated on writing books and articles.

The structural shift within academic employment during the 1960s and 1970s increased the demand for new specialized journals within economics beyond the increase implied by the index of membership in the A.E.A. that I have just mentioned. Growth – in the ranks of economists, especially those viewing themselves as professionals narrowly defined – caused a “knowledge explosion,” especially during the 1960s. This “knowledge explosion” promoted growing homogeneity in the major journals within the field.

The price of the growing professionalism associated with mathematization was growing irrelevance to scholars in most of the other social sciences. Mathematical economics rooted in deductive logic walked away from political science, geography, psychology, and sociology. Even more, it parted company with history.

But were economists willing to be irrelevant? To nobody’s surprise, “ideological imperialists” beating the drums for the power of economic logic emerged, most notably Gary Becker. In Becker (1976: page 5) the following proposition is advanced:

“The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach ….”

Becker (1976: page 14) goes on to argue:

“I am saying that the economic behavior provided a valuable unified framework for understanding all [his emphasis] human behavior, although I recognize, of course, that much behavior is not yet understood, and that non-economic variables
and the techniques and findings from other fields contribute significantly to the understanding of human behavior.”

It is a good thing Becker included the caveats. After all, as a good income-maximizing economist, he had manifest self-interest in selling books to anthropologists, sociologists, and geographers. However to be honest, I fail to detect conviction or sincerity in Becker’s caveats, and I expect most other readers of his manifesto would agree with me.

Indeed, things went so far that an economist, and economic historian at that, actually titled an article “Is It Kosher to Talk About Culture?” [16]

In promoting a highly stylized mathematized model of *homo economicus* economists were building walls of professionalism that clearly demarcated their approach from that used in other fields. In doing so, in throwing up the stockade as it were, the committed professionals within the field had a strong interest in driving out adherents of renegade approaches, like psychology, sociology and history.

The new economic historians responded by crawling into the stockade, taking Becker’s program and econometrics, applying them willy-nilly to “revolutionize” history. Once ensconced in the stockade, they could gather behind Becker’s imperialist rhetoric, claiming that their techniques and viewpoints would sweep away the history as it was written in the past. Not surprisingly, the economic profession bestowed the Nobel Prize for Economics on two of the most vocal advocates of this approach, Robert Fogel and Douglass North.

Keeping the profession well defined in terms of acceptable standards for journal publication was one thing. Increasingly mathematical the four prestige journals staked out a relatively “homogeneous” product that economists and non-economists could instantly
recognize. As Becker said: toss together some constrained maximization, a dose of market equilibrium with carefully sculpted preferences and you have it.

Branding dissidents as radical economists, Marxists, sociologists, anthropologists and the like - purifying the product by standardizing it and inviting heretics to leave - kept barbarian ideas at bay amongst the community of academic economist professionals. As a professional guild economists became increasingly successful in standardizing the rules and rites of membership through the standardization of the “acceptable” journal article. But academic economists, or at least most of them, also taught. How did the increasingly narrow professional identification of economists interact with the demand for their services in the classroom?

V Is the Business School Tail Wagging the Economics Dog?

In the classroom the professor of economics found him/herself concentrating more and more on presenting the contents of toolkits. Consider the views of Bernard Saffron of Swarthmore College enunciated in an A.E.A. roundtable on the teaching of economics in 1981/1982: [17]

“If our students are going to be able to develop analytical and critical skills, read the economics literature, or attempt to do independent work in the field, they need a variety of toolkit courses including calculus, statistics, and intermediate theory. Many of them would also find it useful to take courses in econometrics, accounting, operations research, or historical techniques.”

The task of the economist qua teacher was to “tool” the student. But what were the members of the student audience looking for as consumers of the training? Why was learning about the toolkits valuable to them? What did they intend to do with the tools –
or with a certification that indicated that they had mastered the elements of the toolkits - upon graduation?

Moving to a toolkit approach to teaching economics was not an easy task. Key was turning the introductory course in economics into a prototype for the new professionalism. Reflecting the state of the discipline up through the 1940s, introductory textbooks in economics were eclectic, a point made by Bronfenbrenner (1942: page 557):

“In attacking the essentially institutional introductory course, Messrs Clemence and Doody do not lay sufficient stress, it seems to me, on three of the most telling objections ….. Second, …. The absence of any standards of theoretical reasoning, serves to strengthen the student’s initial appraisal of economics as a branch of the forensic art, in which any opinion is as good as any other …. The effect of this belief on the prestige and usefulness of professional economists requires no comment.” [My italics]

Appreciating the overriding importance of the debate over the content of courses in elementary economics is essential to grasping the significance of Paul Samuelson’s basic principles text, Economics, the first edition appearing in 1948. [18]

Samuelson’s text, which went into fifteen editions – sales peaking at over 440 thousand copies sold with the sixth edition that was first issued in 1964 – is usually credited with promoting the neoclassical synthesis that sandwiched together Keynesian theory in the guise of the aggregate demand multiplier model with the foundations of the constrained maximizing theory of behavior and the concept of market equilibrium. For this reason many historians of economic thought believe that it was instrumental in promoting the Keynesian revolution in economics. In my opinion its greatest significance
lies elsewhere: its main impact was on pushing the toolkit approach to economics, dressed up in deductive mathematical logic.

Once the elementary course in economics was defined in these terms, the identification of an economist as a professional defined in terms of toolkits was well underway. Students entered into the kingdom of economics through the gateway of a principles text that set the table for the rest of the toolkit-oriented curriculum in the major. [19]

To appreciate why a particular type of student wanted to acquire a particular set of tools, or at least a certificate indicating some level of mastery of the contents of the toolkits, it is important to realize that there is a premium for possessing mathematical and quantitative skills in the business community, white-collar employment and in factory work. This premium shows up in statistical studies of wage determination in the American labour market. [20] The undergraduate eyeing a career in banking or management, contemplating acquiring a Masters of Business Administration, is naturally attracted to the fields of business administration, commerce, or economics. This is particularly true of the undergraduate with an aptitude for mathematics and quantitative analysis. [21]

Either a desire to send signals to prospective employees regarding mathematical and quantitative abilities, or a genuine need to acquire particular skills that may be of use for a career in industry and commerce or government, informs the demand of most undergraduate students majoring in economics. To be sure, some of the students enrolling in the economics curriculum do intend to become professional economists, academic economists in particular. But many do not: they would be equally well, perhaps better,
served by programs in commerce or business administration, or by programs in public policy.

This is the reasoning of the “discouraged-business-major” hypothesis that has been used to explain why enrollments in undergraduate programs in economics plummeted during the 1990s. The idea is that there was a surge of students trying to get into commerce or business administration programs during the 1980s, many being turned away in the shuffle. A large number of these “discouraged-business-majors” ended up choosing an economics major as a second best option. [22] When the demand for slots in commerce and business administration programs fell off, so did the demand for slots in economics departments.

Why is an economics department an attractive option for discouraged-business-majors? The answer is simple: the toolkits offered by economics departments overlap heavily with the toolkits that business and commerce programs offer in their curricula.

Over the course of the post-1950 period the curricula of business schools has evolved. By the mid-1970s a standard business school model had emerged. It consisted of five components: accounting, finance, management science/operations research, marketing and organization theory and behavior. [23]

Many facets of this standard business school program involved the very skills offered in the standard economics department toolkit. [24] A particular striking example is operations research. [25] From my own personal experience as a graduate student taking a course in basic graduate economic theory at Harvard from Dale Jorgenson during the early 1970s – the package of notes that he distributed had been worked up at Berkeley in the years before Jorgenson joined the Harvard department – I can attest to the fact that
linear programming and other tools of operations research were being directly incorporated into economics at the time. Indeed, a number of students in the program I was taking were in a joint program in Business and Economics. They were attracted in this joint program by the strong overlap between the two fields.

Economics, once a field exemplified at his best by the three scholarly traditions that Schumpeter argued were essential for the economist – a knowledge of history, statistics and theory – was rapidly becoming a toolkit field, favored at the undergraduate level by mathematically inclined students who could not cram into the technically oriented commerce and business administration programs flourishing in the 1960s and 1970s and 1980s.

VI The McEC Meal: From the Ivory Tower to the Golden Arches

One of the consequences of professional specialization in academia is the creation of standardized products. Hayes and Wynyard (1998) provide a nice analogy for this drive to standardization: the McDonaldization of higher education.

The logic of the argument I advance in this paper is that economics has become more and more standardized. Indeed, in comparison to fields like sociology and geography that are still relatively interdisciplinary, economics is homogeneous, highly standardized.

With this in mind, I would like to close this paper by considering the McEC meal at Drive-Through-U.

Based upon the gist of my argument, I doubt any economics department sincerely sees economic history as an essential component of the McEC Meal. Rather I think the standard menu would look like this:
• The microeconomic toolkit: constrained maximization, the consumer and the firm. Market equilibrium and the mathematical properties of supply and demand curves. Game theory and strategic behavior.

• The macroeconomic toolkit: Multiplier and IS/LM analysis; aggregate supply; overlapping-generations models and growth models, exogenous and endogenous.

• Econometrics.

These are the basics components, the burger, the veggies on the burger, and the fries.

This is where you get the protein and calories, the starch. Then there are the condiments, the upper division electives, all wrapped up in neat little plastic bags:

• Labour economics

• Money and banking, finance

• Managerial economics

• Industrial organization.

Take the toolkits that you got with the protein and starch, burnish them up a bit, and you apply them in the electives. Load on that mustard and relish!

    The McEC meal – doesn’t it sound delicious? Can’t you see the students driving their cars through the queue at the drive-through anticipating the taste? … “Yum, yum…. What a Mc Deal! I think I’ll take one, condiments and all…. better yet: Supersize it!”
Footnotes

[1] See Mosk and Nakata (1992) and Chapter 4 of Mosk (1995.)

[2] See Romer (1994). It is worth noting that Romer works in macroeconomics, and the main thrust of her remarks has to do with the importance of American economic history for the testing of macroeconomic propositions about the American economy.

[3] See Crafts (1987), Fogel (1964) and North (1963). The useful survey of contemporary economic history thinking by Whaples (1995) shares this bias. The list of issues that he thinks are crucial for the interpretation of the economic history of the United States are heavily weighted towards issues that the new economic historians have lavished their greatest attention upon. That said, it is worth pointing out that Whaples (1995) does point to intellectual differences between historian and economics members of the Economic History Association. The new economic historians were certainly successful in promoting their view that they were bringing a breath of fresh air into a field of enquiry that had stagnated. As Aitken (1963) shows, membership in the Economic History Association jumped by leaps and bounds in the 1950s and 1960s, precisely when the new economic history was making its first sustained intellectual splash.


[5] See, for instance, Schmalensee (1991), especially page 119. Like economics, most
contemporary sociologists and organization theorists have also turned their back
on the historical focus of an earlier generation of scholars, like Max Weber. See,
for instance, Kieser (1994).

am grateful to Malcolm Rutherford for stressing the overwhelming importance of
these four journals within the English reading community of economists.

[7] I must confess to a personal bias in selecting these four names. They have all
influenced me, either directly or indirectly. I was a student of Gerschenkron’s at
Harvard, and had the fortune to interact with Kuznets, even though he was only
emeritus at Harvard. I have found Schumpeter’s theories particularly helpful in
developing my own views about Japan’s economic history, and for a recent
research project have found Kindleberger’s views about hegemony and
international trade exceptionally enlightening.


[10] Schumpeter was born in 1883 and died in 1950. For an useful account of his life
and contributions to economics see Haberler (1950.)

[11] On the career and biography of Kuznets, see Kapuria-Foreman and Perlman

[12] For the methodological views of Kuznets, see Kuznets (1957.)

[13] For a biography of Gerschenkron and an account of his theory of economic
backwardness in historical perspective, see Rosovsky (1979.)
On academic professionalism in general, see Robbins (1993) and Wilshire (1990). For professionalism within economics as seen from a sociological and historical perspective, see Coats (1985, 1993.)

See Backhouse (1998) and Debreu (1991). For the almost complete absence of mathematics in the early professionalization of economics – which occurred during the 1880s – see Baumol (1985).

Temin (1997).

See Solow, Johnson, Lampman, and Saffron (1982.)

For a brief account of the changing content of, and sales figures for, Samuelson’s principals text, Economics, see Skousen (1997). Beginning with the twelfth edition of the text, first printed in 1985, Nordhaus joined Samuelson as co-author.

For the curricula of economics departments circa 1980, see Siegfried and Wilkinson (1980.) For the role of the A.E.A. Committee on Economic Education in shaping the curricula of departments, see Hinshaw and Siegfried (1991.)

See Murmane, Willet and Levy (1995) and Rosenbaum and Binder (1997.)

See Fiorito and Dauffenbach (1982.)

For the discouraged-business-major hypothesis, see Salemi and Eubanks (1996). Willis and Pieper (1996) consider additional factors – increased competition from public policy programs and a declining rate of return to majoring in economics – in accounting for a decline in demand for undergraduate economics programs during the 1990s. Margo and Siegfried (1996) argue that much of the decline during the 1990s is explicable in terms of “mean reversion” to a constant share of undergraduate degrees enjoyed by economics department baccalaureates.
See Brooker and Shinoda (1976). For the state of business school programs in the United States in the 1950s, see Gordon and Howell (1959); von Zur-Muehlen (1971) considers the expansion of business education in Canada. Jenks (1960) provides a useful historical overview of management movement, arguing that it arose out of the profession of industrial engineering, especially Taylorism. He argues that the demand for technical specialized personnel equipped with mathematical and quantitative toolkits was partly due to the growth of employment in large companies. Because employment in large companies has recently declined as a share of the total labor force in many of the high-income countries, the demand for business school skills may have fallen off during the last decade. This may account for the recent decline in demand for slots in commerce and business school slots, and in economics department programs.

On the growing use of mathematics in insurance and risk management, see Greene (1961). For the heavy reliance on mathematical modeling in management science, see Institute of Management Sciences (1955.)

On the rapid rise of operations research in the post-1950 period, and the tapering off of the field during the 1970s, see Ackoff (1979), Balut and Armacost (1986), Walsh (1968) and Zimmerman (1982). Cooper (1958) looks at the overlap of operations research and economics.
Bibliography


Table 1: Distribution of Citations to Four Notable Economic Historians 1950-2000 in JSTOR Listed Economics Journals \[a\]

<table>
<thead>
<tr>
<th>Type of Journal [b]</th>
<th>1950s</th>
<th>1960s</th>
<th>1970s</th>
<th>1980s</th>
<th>1990s [c]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Joseph Schumpeter</strong> [d]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citations</td>
<td>370</td>
<td>287</td>
<td>212</td>
<td>241</td>
<td>257</td>
</tr>
<tr>
<td>Mainstream</td>
<td>53.2%</td>
<td>51.9%</td>
<td>37.3%</td>
<td>34.9%</td>
<td>31.5%</td>
</tr>
<tr>
<td>Economic History</td>
<td>12.7</td>
<td>17.4</td>
<td>23.6</td>
<td>18.3</td>
<td>16.0</td>
</tr>
<tr>
<td>Other</td>
<td>34.1</td>
<td>30.7</td>
<td>39.1</td>
<td>46.8</td>
<td>52.5</td>
</tr>
<tr>
<td><strong>Simon Kuznets</strong> [e]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citations</td>
<td>291</td>
<td>316</td>
<td>305</td>
<td>208</td>
<td>214</td>
</tr>
<tr>
<td>Mainstream</td>
<td>50.9</td>
<td>47.2</td>
<td>35.4</td>
<td>29.3</td>
<td>31.8</td>
</tr>
<tr>
<td>Economic History</td>
<td>10.3</td>
<td>15.8</td>
<td>24.6</td>
<td>31.7</td>
<td>16.4</td>
</tr>
<tr>
<td>Other</td>
<td>38.8</td>
<td>37.0</td>
<td>40.0</td>
<td>39.0</td>
<td>51.8</td>
</tr>
<tr>
<td><strong>Charles Kindleberger</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citations</td>
<td>66</td>
<td>99</td>
<td>173</td>
<td>182</td>
<td>123</td>
</tr>
<tr>
<td>Mainstream</td>
<td>54.6</td>
<td>51.5</td>
<td>37.6</td>
<td>28.6</td>
<td>18.7</td>
</tr>
<tr>
<td>Economic History</td>
<td>7.6</td>
<td>11.1</td>
<td>20.8</td>
<td>28.0</td>
<td>30.1</td>
</tr>
<tr>
<td>Other</td>
<td>37.8</td>
<td>37.4</td>
<td>41.6</td>
<td>43.4</td>
<td>51.2</td>
</tr>
<tr>
<td><strong>Alexander Gerschenkron</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citations</td>
<td>58</td>
<td>70</td>
<td>94</td>
<td>44</td>
<td>67</td>
</tr>
<tr>
<td>Mainstream</td>
<td>50.0</td>
<td>40.0</td>
<td>19.2</td>
<td>15.9</td>
<td>20.9</td>
</tr>
<tr>
<td>Economic History</td>
<td>19.0</td>
<td>40.0</td>
<td>64.9</td>
<td>63.6</td>
<td>41.8</td>
</tr>
<tr>
<td>Other</td>
<td>31.0</td>
<td>20.0</td>
<td>15.9</td>
<td>20.5</td>
<td>37.3</td>
</tr>
</tbody>
</table>

**Notes:**
Table 1 [Continued]


[b] The term “mainstream” refers to four journals, the American Economic Review, the Economic Journal, the Journal of Political Economy and the Quarterly Journal of Economics. The term “economic history” refers to two journals, the Economic History Review and the Journal of Economic History.

[c] For Schumpeter, citations in the year 2000 are included in this category; for Kuznets, citations in the years 2000 and 2001 are included in this category; for Kindleberger, citations in the year 2000 are included in this category.

[d] Citations to “Schumpeter” may include a few citations to Elizabeth Boody Schumpeter.

[e] Citations to “Kuznets” may include a few citations to George Kuznets and Paul Kuznets.

Sources: Website http://www.jstor.org.