real-world economics review, issue no. 74
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History as a source of economic policy

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In a debate about whether economics is a science, "Nobel Prize" winner in economics Robert J. Shiller observes that economics differs from fields in natural science in one important respect:

> "Economics is necessarily focused on policy, rather than discovery of fundamentals. Nobody really cares much about economic data except as a guide to policy: economic phenomena do not have the same intrinsic fascination for us as the internal resonances of the atom or the functioning of the vesicles and other organelles of a living cell. We judge economics by what it can produce. As such, economics is rather more like engineering than physics, more practical than spiritual."

To this difference in purpose, Alan Y. Wang, joining the debate at Harvard, added that economics also differs from fields in natural science as a subject of inquiry:

> "Merriam-Webster's definition of science is 'a study of the natural world based on facts learned through experiments and observation.' What physics and chemistry and molecular biology have in common is that the building blocks of what they observe and experiment with don't change. Such is the natural world. But what is the building block of economics? People. Economics does not study any unit smaller than a collection of people. And human behavior can never be absolutely predicted or explained—not if we wish to believe in free will, at any rate." ("No, Economics Is Not a Science.")

Shiller commented in the same vein:

> "[M]odels describe people rather than magnetic resonances or fundamental particles. People can just change their minds and behave completely differently. They even have neuroses and identity problems, complex phenomena that the field of behavioral economics is finding relevant to understanding economic outcomes."

And he could have added that these behaviors are often projected irrationally as well as rationally in different places and times in their specificities into the present from the past.

Historians agree with the subject and aim of economic inquiry as Shiller and Wang state them, because they discuss people and policy in their work. Orthodox economists have excluded historians from the study of economics, because their work is not "scientific." Here is how Egmont Kakarot-Handtke phrased the objection:

> "Heterodox history should not be confounded with heterodox economics. Heterodox economics can tell historians … that they have no idea of the

> fundamental Law of Profit as they have no idea of the Laws of energy transformation. Economics is neither psychology, nor sociology, nor history. Economics is the science which studies how the monetary economy works. Science looks for what remains unchanged in time, i.e. 'eternal' laws, history looks at what changes over time. 'That is why Descartes said that history was not a science – because there were no general laws which could be applied to history."

Most non-historians have a simplistic view of how historians do history: History is something that exists "out there" to be observed and explained by historians based on data, printed matter, and archival sources, the debris of history that historians dredge up in their research. History, however, no less than natural science or economics, does not exist "out there" but in the mind of the historian, as individual and guild. (Capra, 1982)

Wilhelm Dilthey, who wrote extensively on the subject, also stressed that unlike natural science, which looks for law-based explanations, the task of the humanities is to "understand" human and historical life:

> "The way the historical world is represented and explained must in some way reflect the way history has been lived and understood. Understanding (*Verstehen*) for Dilthey is a process that employs all our capacities (thinking, feeling, willing of life's experiences) and is to be distinguished from pure intellectual understanding (*Verstand*)" (Dilthey, Stanford Encyclopedia of Philosophy).

Accordingly, historians comprehend an economic-influencing occurrence differently from orthodox economists who observe and explain (*Verstand)*. Just as a person understands what is meant when he/she hears a person is in passionate love, not because he/she observes the symptoms following some natural law but because he/she has been in passionate love and has felt its force, an historian understands the power of self-preservation of an interest group (like neo-classical economists) because he has experienced the hold that self-preservation has over people. The historian's understanding of people does not come through "objective concepts," but only from past views, interpretations, and a shared world, that "springs out of the depth of his own experience" (Dilthey, *Einleitung in die Geisteswissenschaften*, p. 1).

That understanding comes to a great extent out of an inherited culture which historians learn about from the sources and create in their debates. Historians have a heightened sense of the specious present – the duration that is perceived as present and as extended in time. They have more of the past in it because they perceive it as still active in the now.

The question is, then, do the results of the intellectual work Egmont Kakarot-Handtke claims economists do in their search for laws provide a clearer guide for policy making that Shiller says is the aim of economics than the understanding historians achieve in their research methodology and interpretation of the specious present?

The scientific paradigm as an instrument of policy

Stanley Jevons, inventor with Karl Menger of marginal-utility analysis, described the first step that neoclassical economists had to make in order to turn their subject into a scientific object:

> "…all branches and divisions of economic science must be pervaded by certain general principles. It is to the investigation of such principles – to the tracing out of the mechanics of self-interest and utility, that [economics] is devoted" (Jevons quoted in Fullbrook, 2006, p. 2).

Fullbrook describes how the architects of the new discipline mapped it isomorphically with Newtonian mechanics in order to enhance its scientific stature:

> "In Neoclassical economics, 'bodies' translates 'individuals' or agents,' 'motions' translates 'exchange of goods,' 'forces' translates 'desires' or 'preferences,' which when summed become 'supply and demand, 'mechanical equilibrium' becomes 'market equilibrium,' this being when the difference between supply and demand is zero, and 'physical systems' translates 'markets.' …All exchanges were said to magically take place at the prices that equated demand and supply" (Fullbrook, 2006, p. 2).

No viable science means much to the neoclassical economist unless it can be expressed mathematically. That was the glory of Newtonian mechanics. In the second half of the nineteenth century Léon Walras, preceded by some French economic-engineers (Locke, 1989, pp. 124-26), mathematized neo-classical economics. With this achievement he stated in 1884 in his *Elements of Pure Economics that* economics has become a "science, which resembles the physico-mathematical sciences in every respect" (Quoted in Fullbrook, 2006, p. 3).

A claim, however, is not a reality. Seventy years after Walras' achievement, John von Neumann and Oskar Morgenstern questioned the prescriptive power of mathematical economics. In the forward to their remarkable book, *Theory of Games and Economic Behavior (1944)*, they wrote: "The concepts of economics are fuzzy but even in those parts of economics where the descriptive problem has been handled more satisfactorily, mathematical tools have seldom been used appropriately. Mathematical economics has not achieved very much."

During WWII and the Cold War, scientists and engineers working on operations research problems, developed analytical tools that people thought finally would turn management and economics into prescriptive management sciences. The work of George B. Dantzig and his associates at the Rand Corporation on the linear programming algorithm used in decision making drew the most attention. The procedure utilized modern mathematics (vector algebra, matrix theory, symbolic logic) and statistical techniques in an effort to take the guesswork out of management. The US Air Force, for which Rand worked on contract, used it logistically in the Berlin Airlift and to maximize the effectiveness of bombing patterns in the Korean War. The operations research methods were then proselytized among US NATO allies in seminars and symposiums held in America or abroad and by management consultancies in the broader business and corporate world.

Prescient neoclassical economists latched on to the new methods; while at Rand in 1948, the economist Kenneth Arrow used Dantzig's toolkit in his work on Rational Choice Theory. His book *Social Choice and Individual Value* (1951) was the "first real classic" on what, Robert N. Bellah remarked in 2000, "is now taken as a given in economics and has spread out into many neighboring disciplines." (Bellah, 2000, p. 7; Amadae, 2003) The neoclassical economists Joseph Dorfman, Paul Samuelson, and Robert Solow applied linear programming to their subject in *Programming and Economic Analysis* (1958).

This enthusiasm for scientific decision making prompted the founding of and permeated the thinking in postwar operation research groups, and by extension into economics. The British *Operational Research Quarterly* printed their scientific credo on its masthead:

> "Operational Research is the application of the methods of science to complex problems arising in the direction and management of large systems of men, machines, materials and money, in industry, business and defence. The distinctive approach is to develop a scientific model of the system, incorporating measurement of the factors such as choice and risk, with which to predict and compare the outcomes of alternate decisions strategies or controls. The purpose is to help management determine its policies and actions scientifically" (see any copy of the journal).

The simplex linear programming decision-making algorithm that influenced thinking in neoclassical economics is an example of this scientific model building. The procedure, which provided for comparisons of sets of ratios existing among sets of consumption of various inputs and rates of production of various outputs, utilized modern mathematical and statistical techniques. The algorithm was based on the following assumptions:

1. Objectives can be stated mathematically
2. Resources can be stated mathematically
3. Alternative courses of action are too numerous for discussion by older methods
4. Variables had to be related linearly, i.e., when two variables are linearly related this means that a change in one causes exactly proportionate change in the other.

Up to 1968 "optimism about the future of OR" reigned in the pages of this quarterly; there was "almost a total lack of criticism and debate" (Dando and Sharp, pp. 93-94). By 1973 the papers reflected considerable doubt about the practical effectiveness of OR, a doubt which by 1978 was being voiced in about a quarter of the major papers appearing in the journal. Essays in the late 1970s, therefore, expressed a decade of ever-increasing doubt at the center of the OR paradigm about the effectiveness of decision science models.

Models of macroeconomic prognostication were also scrutinized critically. Such models were relatively successful as long as the future resembled the past, but when the future did not resemble the past, spectacular failure to predict rate of economic growth, business profitability, inflation rates, private consumption levels, employment, etc. ensued. The conclusions Frederichs and Kübler reached about the reliability of German econometric models seemed to apply to the entire macroeconomic exercise in model building: "Neither the econometric, nor the naïve prognosis, nor the judgmental forecasts could satisfactorily predict future economic development" (p. 814). It was difficult to formulate useful economic policy recommendations with mathematical models. A well-known economist Kenneth Boulding, called the whole mathematical enterprise a mistake: "Perhaps the real villain," he wrote, "is

the discovery of seventeenth century mathematics two hundred years later by Cournot, Jevons, and most of all Walras, whose influence and brilliance set economics on a path that increasingly has become a dead end" (Boulding, p. 5).

Clearly there are fatal deficiencies in modeling; in the case of the linear programming decision model the requirement that objectives and resources be stated mathematically is one of them. At the individual manager's decision making level, the operations research scientists like to think that the manager's job is to plan, organize, co-ordinate and control, in order to optimize. These are rational, analytical acts, which seem well suited for scientific operations research techniques. But as Ian Glover and Michael Fores (p. 121), noted the manager's job is actually

> "…of an unprogrammed character; he… is not much concerned with the flow and use of 'hard' information, his information is distorted, incomplete, his job is ambiguous. He is not primarily a decision-maker, a planner, but an 'inspirer,' a fire fighter, and a rationalizer after the fact." As Glover phrased it, "attempts to 'study' decision making are overly academic; attempts at 'programming' it all seems to be like the search for fool's gold, confusing the academic ballgame of analysis with the executive task of synthesis" (Glover and Fores, p. 118).

Glover and Fores highlight variables that are important to success that cannot be stated mathematically. Charisma or leadership qualities in a manager are often among these variables. Clausewitz in his famous work, *On War* could and did measure the material resources of an army when estimating its strength, but in the fog of war he knew that successful outcomes depend much on the chief executive's genius. Napoleon was famous for his rhetoric; other great commanders, including Napoleon himself, had charisma. Nobody could predict the appearance of these irrational traits in commanders or define what precisely they are, or state them mathematically, but they are palpable and at times event-deciders (Locke, 2012).

Similar points can be made when distinguishing between managers and entrepreneurs. Friedrich Nietzsche's mythopoetic vision of Apollo and Dionysus can be used to sum up the difference. In his study of *Young Nietzsche* Carl Pletsch observed that for Nietzsche, the Apollonian "is the principle of clearly delineated images, permanence, optimism, individualization, and rationality. It is striving for clarity." This is the ethos of analytical formalism and mathematical methods. On the other hand, for Nietzsche the Dionysian expresses "the principle of flux, impermanence, suffering and pessimism…an irrational force, wild impulsive, and instinctive." This is the creative power of the great entrepreneur. Whereas the Apollonian vision is timeless and "responsible for the constant formulation and reformulation of the forms of knowledge and rationality that order our everyday life," the Dionysian urge, which is "momentary, exceptional, and counter-intuitive," is "dangerous to any structure of reality; it is the maelstrom of any impulse caught in the flux of time," unknowable and unpredictable (Pletsch, pp. 131-32, Locke and Schöne, pp. 2-5).

The economist Gunnar Eliasson, struggling to come to terms with entrepreneurialism and intrapreneurism within firms in the late 20th century, frustratingly commented about the inadequacies of the economist's scientific toolbox.

> "Management teachers as well as economic theorist need a realistic model to support teaching and thinking. Since no realistic teaching of dynamic markets

> exists, no good theory of the firm has been created. The moral, hence, is that so far we have excellent firms, not thanks to but despite management teaching" (Eliasson, 1997, p.12).

Perhaps for historians the most convincing evidence of the failure of this new scientific paradigm is in apostasy. Two examples of personal turnabouts are briefly outlined here. One is the operations research pioneer Russell Ackoff. He developed operations research at the Case Institute of Technology, Cleveland, before moving to the Wharton School of Finance, where OR techniques could be applied to a broader range of subjects in finance and business than just industrial administration. He is also credited with introducing more rigorous scientific operation research methods into British academia. K. Brian Haley notes that Russell Ackoff's coming to the University of Birmingham, as Joseph Lucas Visiting Professor, in 1961 was a signal event. "His presence had a major impact on the whole of the UK educational scene, inspired a number of initiatives in the way the subject was viewed in industry, and was one of the prime movers in the establishment of the Institute for Operational Research" (Haley, 2002, p. 85). The University of Birmingham, which had invited Ackoff, had instituted a master's in OR in 1958; his presence seemed to stimulate the development of academic OR in the UK, with masters' in OR initiated at Imperial College London and at Cranfield in 1961 and a master's in the subject started at the University of Hull in 1962.

By the 1970s Ackoff began to have misgivings about operations research as a scientific endeavor. He especially attacked the use of mathematics in modeling. In a remarkable essay, "The Future of Operational Research is Past," in 1979 (JORS 30, pp. 93-104), he accused operational research scientists in rather crude phraseology of engaging in "mathematical masturbation without substantive knowledge of organizations, institutions, or their management" (p.97). "OR problems," he concluded, "can never be a perfect representation of a problem. They leave out the human dimension, the motivational one;" indeed, he affirmed that the successful treatment of managerial problems deserves "the application not only of science with a capital S but, also, all the arts and humanities we can command" (p.102).

The second apostate is H. Thomas Johnson. He taught management accounting at a respectable university in the 1970s, where he took an interest in the financial accounting systems introduced in the 1920s into US multi-national automobile firms (Johnson, 1978). Then in the 1980s Johnson embarked on a twenty year journey of apostasy during which he studied, among other things, W. Edwards Deming on quality management and conducted a decade-long inquiry into the Toyota Management System at its Georgetown, Kentucky plant (Locke, 1996, pp. 176-77). Johnson is associated with the group in US manufacturing that developed the idea of lean management – to which the name of Mike Rother can also be attached. They became sharp critics of the command and control management culture, what Johnson called "management by results," that were set up to run mass production factories in America, which by the 1980s were failing to meet the organizational challenge posed by Japanese manufacturing, what he called "management by means."

He portrayed US Big Three automakers management under 7 rubrics:

1. the individual is responsible,
2. control results
3. follow finance-driven rules
4. manipulate output to control costs
5. increase speed of work

6. specialize and decouple processes
7. the individual is the cause – blame

compared to the Toyota management Kata (a kata is an organization specific practice routine), a system wherein:

1. relationships are reality, and management
2. nurtures relationships,
3. masters life-oriented practices,
4. provides output as needed on time,
5. changes how work is done,
6. enhances continuous flow, and
7. when troubleshooting, considers mutual interaction as the cause of a problem – not individuals (Johnson and Bröms, pp. 186–87)

Johnson complained about the shortcomings of management by result in an article published in 1992, in a paper he did with Anders Bröms in 1995 (Locke, 1996, p. 287), and he returned to the theme in the book he did with Bröms in 2000. He observed:

> "Successful [U.S] managers believed they could make decisions without knowing the company's products, technologies, or customers. They had only to understand the intricacies of financial reporting. … [B]y the 1970s managers came primarily from the ranks of accountants and controllers, rather than from the ranks of engineers, designers, and marketers. [This new managerial class} moved frequently among companies without regard to the industry or markets they served. … A synergistic relationship developed between the management accounting taught in MBA programs and the practices emanating from corporate controllers' offices, imparting to management accounting a life of its own and shaping the way managers ran businesses" (Johnson and Bröms, 2000, p. 57).

Johnson came to despise these lifeless pyramidal structures imposed on work processes and managed by computer-oriented-production-control experts:

> "At first the abstract information compiled and transmitted by these computer systems merely supplemented the perspectives of managers who were already familiar with concrete details of the operations they managed, no matter how complicated and confused those operations became. Such individuals, prevalent in top management ranks before 1970 had a clear sense of the difference between 'the map' created by abstract computer calculations and 'the territory' that people inhabited in the workplace. Increasingly after 1970, however, managers lacking in shop floor experience or in engineering training, often trained in graduate business schools, came to dominate American and European manufacturing establishments. In their hands the 'map was the territory.' In other words, they considered reality to be the abstract quantitative models, the management accounting reports, and the computer scheduling algorithms…" (p. 23).

The US system of management by result is not only different from management by means but inimical to it adoption. Mike Rother learned this when he and his team spent five years

investigating the Toyota Kata (2004-2009), a system of "unseen management routines and thinking" through which the investigator has to find his way "along unpredictable paths through a systematic process of discovery and adjustments." This became particularly challenging to this group of management consultants when they tried to teach management by means in Western firms whose managers have a command and control mindset. Rother ran into the difficulty especially when teaching Western managers about empowerments. Empowerments seek

> "To move decision-making close to where the action is… but just telling people they are empowered [a command and control approach] is insufficient for tapping the brainpower inside an organization in a purposeful way. If people in organizations are expected to make decisions and navigate rapidly at their level, rather than waiting to be told what to do, they need to be taught effective skills for how to do it" (Rother, 2014, p. 4).

To appreciate management by means investigators must be able to grasp all the capacities – thinking, feeling, and willing – that go into an organization wherein employees are empowered. This requires the historian's investigative methods not just those of the mathematically shaped scientific paradigm codified and taught in departments of economics and business schools. The upshot is that economists and MBAs lacked the wherewithal to do much to explain or even to understand the Japanese organizational challenge to US manufacturing from an economic policy standpoint.

For those smitten like Walras with the "physico-mathematical" paradigm, Boulding's suggestion that mathematics is the culprit is unacceptable. They cling to the scientific method while trying to make it relevant by multiplying model building.

But, as Lars Syll noted,

> "The insistence on using analytical formalism and mathematical methods comes at a high cost:
>
> [I]t often makes the analysis irrelevant from an empirical-realist point of view. Applying closed analytical – formalist – mathematical – deductive – axiomatic models, built on atomistic-reductionist assumptions to a world assumed to consist of atomistic-isolated entities [the view of orthodox economics], is a sure recipe for failure when the real world is known to be an open system where complex and relational structures and agents interact. Validly deducing things in models of that kind doesn't much help us understanding or explain what is taking place in the real world we happen to live in" [Syll, 2015].

If the economists' models cannot explain what is taking place in the real world, they cannot delineate useful economic policy.

There is, therefore, something wrong with people who continue to follow the same analytic formalism and mathematical methods over and over again, expecting a different outcome – it is one definition of being crazy. Rational people, if analytic formalism and mathematical methods do not uniformly succeed as a guide to economic policy formulation in the real world, would not make them the sole basis of economics.

Anchoring economic policy discussion in historical analysis

Historians deal with people in their specificities, but historians are generalists not, unless qualified accidentally additionally, experts in science, mathematics, and technology. This means, for example, in the case of Russell Ackoff, that historians understand that he, an acknowledged expert in operations research, developed serious doubts about the usefulness of OR modeling, but they do not try to examine the validity of Ackoff's claims because they are not mathematically and scientifically able to do so; they leave that to the knowledge experts, and report on the findings. The critics of scientific decision modeling cited in this article, are not historians but qualified experts in fields of natural and social science, and mathematics.

The historians' contribution to economic policy formulation stems from elsewhere, that is, their particular expertise in examining the economic experience of people, in the peculiarity as well as similarity of individual and group activities (in companies, communities, regions, and/or nations). The method is comparative history, which excites and informs economic policy discussion, not by revealing eternal laws like economists try to discover in economic models of their own device, but by showing through historical research how different human experiences in the flux of time and specificity of place stimulate discussions of policy making, that, of necessity, are constantly under revision according to circumstance.

Nothing illustrates this approach better than the work of the economic historian Friedrich List in his fight with classical economists about policy matters pertaining to economic development. List, probably the most prominent economic historian during the age of classical economics, lived on the periphery of the London market emporium, in List's case mostly in German lands and America, where in the era of the First Industrial Revolution (1750-1850) people believed that they were disadvantaged by the operation of market forces tied into the market heartland. David Ricardo might talk about comparative advantages in trade, but List thought that the London centered market emporium did not offer a level playing field for less technologically advanced areas competing with the more advanced. Classical economists focused on markets, List focused on nations and regions, whose economic and industrial welfare have incidentally been the principal preoccupation of the educated public in the real world at least since the French Revolution and the Napoleonic wars.

When trying to explain why some peoples succeed more than others, List delved into the histories of communities and national states in various stages of existence, seeking time-based cause and effect reasons for transformation or resistance to economic change. He included classical economics in his nation-based scenario because he thought that it provided an ideological superstructure that promoted the prosperity of London's market emporium at the outsiders' expense (List, 1841, p. 159. Daastøl, p. 241).

For those use to looking at America and Germany as technologically advanced nations, this view appears nonsensical, but in List's time, German-speaking Central Europe was a poor, politically divided, underdeveloped, market segmented land, *vis-à-vis* the economic leader Great Britain, and America in the years List lived there (in the 1820s and 1830s) a technologically backward, half-slave ridden, undeveloped country.

At the national and international level List favored protectionism, for which today's orthodox economists almost exclusively remember and condemn him. The proof he cited to support the policy was not grounded in the *a priori* reasoning of classical economists but in the evidence

provided by the effects of tariff policy in America, where protectionist measures, enacted to shield infant industries from unfair British competition, he believed, had produced prosperous communities. From an economic policy perspective List's views, which were broadly shared by historical economists, have usually prevailed over the free trade advocacy of classical economics. Classical and neoclassical economic models are incapable of explaining how a cartelized German economy became technologically the most advanced industrial economy in Europe between 1871 and 1914 or how the US rose to be the first industrial nation globally during the same period behind high tariff walls.

List's policy recommendations encompassed much more than tariffs. He was particularly outspoken about the benefits of railroads, affirming that transport served primarily a cultural or ideal function, much like the internet today: to convey ideas, spark new ideas, and promote innovation. When evaluating the importance of modern transportation and communications, he perceived the significance of what is now called clustering and networking as a manifestation of mental capital (Locke, 2000, 2015, Chapter I; Locke and Schöne, 2003). He asserted that only in towns do the populations enjoy safe and effective administration; he realized that better communication, because of geographical proximity among actors, boosts cooperation, improves synergy among skills and trades, and fosters efficiency through less travel and consumption of resources like time and energy (List, 1841, pp. 203-204; Daastøl, p. 247).

He thought that the population of an industrialized society had to be brought together in a few conurbations in which are concentrated a great variety of skills, productive powers, applied science, art, and literature. Here are to be found great public and private institutions and associations in which theoretical knowledge is applied to the practical affairs of industry and commerce. Only in such conurbations can a public opinion develop that assures national prosperity (List, 1837, p. 69. Daastøl, p. 246). On the other hand, he argued that scattered productive power in the countryside environment dissipated the productive force of nations (List, 1841, pp. 203-204; Daastøl, p. 247). He also spoke out about the positive effects of urbanization because communication density spread uniform systems of weights and measures and awareness of distance, and time, and urban institutions provide money and credit, regulated by law.

List argued that it was the statesman's duty to adopt policies that would promote these institutions. By making the nation the focus of economics and shifting the analysis from the market place to the competitiveness of nations and regions, he made the visible hand of the state, not the invisible hand of markets, an important source of economic policy formulation.

For those who focus on how to overcome backwardness the Listian tradition is alive. This is true particularly of people who are concerned with national systems of innovation. Their work peaked perhaps with the OECD report in 1997: *National Innovation Systems* (OECD, 1997; Daastøl, pp. 233-36). The organizational theorist Bengt-Åke Lundvall writes that among the group preoccupied with innovation theory, the economist Christopher Freeman by referring to List established a connection between their theory and the development theory that List had pushed in his work on mental capital.

Lundvall noted that List's analysis focused on the development of productive forces rather than on allocation issues, and that he had been critical of and polemical about Adam Smith's "cosmopolitical" approach, where free trade was assumed always to be to the advantage of the weak as well as the strong national economies (Daastøl, p. 236). Freeman explained that

to understand international competition "[W]e must go to the original source of the national competitivity school; Read List in the original, and notice" that the first of his "fundamental points… [is] the importance of mental capital…" … "If we are really to understand international competitivity, then it is of no use to go back to Adam Smith and still less to Ricardo and the 'school' of neo-classical comparative advantage theory…" (Quotes in Daastøl, p. 236).

The historian's focus on intertemporal comparative analysis also contributes to policy making. During the first industrial revolution in Britain most skilled craftsmen were not formally educated in science or engineering. They were self-trained, or, rather, trained in apprenticeship or in a firm on the job. Know-how was acquired primarily through *tacit learning*, intuitive and inarticulate, only through individual experience in the relevant context, where the knowing subject is involved, as opposed to *explicit knowledge*, codifiable, generated often through logical deduction, capable of being aggregated at a single location, stored in objective forms without the knowing subject being involved in the aggregation – a learning process with which we became so familiar in the 20th century.

When Britain had the technological lead in the first half of the 19th century its mental capital depended on tacit learning and when British technology moved abroad, the tacitly trained workmen carried the most advanced technology with them. Accordingly, when the French entrepreneurs, Drouillard and Benoist decided to manufacture iron rails for the first French railroads, they turned to the English engineer Charles Manby to build the forges and blast furnaces (à l'anglaise) at the factory site in Alais, Gard. Manby recruited skilled English workers, identifiable from their names in the records of the firm (Wall, Saunders, Shakespear, Crane, and so forth) to construct and run the factory. The specificity of historical investigation, e.g., correspondence and company archives reveals the education and training suited to the state of productive forces at the time (Locke, 1978, p. 45).

During the Second Industrial Revolution the mental capital needs that drove productive forces during the First Industrial Revolution changed. This is obvious for the science-induced industries of the Second Industrial Revolution, like coal tar color dyes, pharmaceuticals and in the new firms employing the electrolysis processes, like aluminum, but it was also the case in older industries as they progressed technologically, in metallurgy, for example, that moved from iron to the new alloy steels used in bearings for high speed machinery and internal combustion motors, and in other scientifically based product lines – in shipbuilding, in the conversion from steam to diesel and electrical power packs, and even in new methods of steam locomotion, in mechanics, etc. The administrative demands in firms also grew as the visible hand of management in large firms and in producer and distribution regional trade associations replaced the invisible hand guiding competition in a forest of firms, which classical and neoclassical economists affirm, guaranteed market efficiency. A new organized capitalism emerged whose mental capital requirements could no longer be entirely met through the know-who networks of men tacitly trained on the job. The older networking of practical men remained, but to it was added networks of scientifically trained chemists and engineers stemming from institutions of higher education to man the new high tech industries.

For those desiring to remain on the cusp of technological change, in Britain, in France, the new mental capital requirements set off alarm bells because they feared that the German education-high tech industrial Gestalt had provided the lead.

Investigating this transformation is the meat of historians as it was for contemporaries who were witnessing the change. In France and Britain contemporaries discussed comparative

systems of education with an eye to educational reform. Educators treated the subject directly, e. g., Charles Lauth (1900), *Rapport général sur l'historique et le fonctionnement de l'Ecole Municipale de Physique et de Chimie industrielle*; Albert Granger (1901), *Etude de quelques laboratoires industriels et des écoles techniques supérieures en Allemagne*; so did parliamentary and government publications that resulted from investigations, for example, "Report of the Select Committee on Scientific Instruction" (1868), "Report of the Royal Commission on scientific instruction and the advancement of science" (1870-75), "Royal Commission on Technical Instruction," reports and minutes of evidence (1882-84) that discussed British worries about a faltering technology due to educational deficiencies in the mental capital stock of the late 19th century.

Because historians, unlike orthodox economists who develop a purely economic framework of understanding, add the dimension of institutional heritage to their understanding of the present, they also through intertemporal comparisons investigate the non-intellectual, noneconomic factors influencing policy making. Just as cultural historians look to Japanese group consciousness rooted in their past when they explain the effectiveness of management by means in the Toyota Kata, the historian's time based analytical dimension permits them to consider how noneconomic institutions that pre-existed the Second Industrial Revolution, and were not economically created by it, inadvertently fuelled high tech industrial development that brought a new technological order into being. In Germany the scientific and research values of *Wissenschaft* institutionalized in academic culture when Germany was disunited and economically backward, proved to be much more fruitful in the development of engineering and business educational networking during the Second Industrial Revolution than did English and French traditions in higher education.

Unlike economists who imbibed analytical formalism and mathematical methods, historians are not in the predicting business. They do not claim to be able to foretell the future from a knowledge of the past because they know when they learn their craft that there are too many unknowns left out of the inherited record, too much of the future is unexpected, irrational and, hence, unpredictable, and too many anachronisms exist inside the historian's mind to engage in accurate, precise prognostication. However, historians also know that human beings are immersed in the evidence of their past and that intertemporal and international comparative histories in their specificities provide a rich source of tacit and explicit knowledge for the public debate about economic policy options that are germane to the economic situations in which people live.

This is as true today as it was in List's time. Recently a group of German business economists asked me to participate in a workshop about the *Ideengeschichte der BWL* (Intellectual History of German Business Economics). My task was to compare German and American management education. I seized on the opportunity to compare different historical experience in a paper entitled: "Reflections on the Response of BWL and US MBA business school education to three major incidents in recent economic process: 1. The Japanese challenge in manufacturing in the 1980s and 1990s, 2. The start-up habitat of Phenomenal Silicon Valley at the turn of the 21st century, and 3. The financialization of the economy, which has occurred with growing intensity since the 1990s." Dominique Turcq, who heads a very savvy management discussion group in Paris (the Boostzone Institute) and has worked in consultancy for decades on comparative American, French, Japanese, and German management, published an edited version of the paper on his blog, with the following introductory comment:

> "We generally do not present academic paper on our blog but we are happy to be able to present this because **some of the major elements of this document, although lessons from history, are major inputs, in my view, on how to think about management, innovation, education and training, for the uncertain and complex future we are facing.** In particular this paper shows how the lack of foresight, of contact with the field and of understanding of the Japanese manufacturing revolution by management education institutions in the US (and all Europe but Germany) disadvantaged the development of industry in Europe (but Germany), how most management education institutions missed completely the innovation and entrepreneurship challenges raised by the Silicon Valley, how the financial education bias in management education taken in the last two decades have led to another complete misunderstanding of what the real world is about and, quasi directly, into major and socially dangerous income discrepancies in our societies. The German model, with significant elements like a priority to technical education, to practical training, the importance of co-determination, the illegality of stock options as a mean of compensating senior executives until 1999, the closeness to field banking systems, etc. is brought to a new light and helps to understand major differences between the German economic model and most other ones in the Western world. This paper not only very seriously questions the model of MBAs but also shows us how education choices and financing choices can be determinant in the long term view of a corporate/ country competitiveness. One quote before you start reading: 'The competence acquired from a business school MBA education did not prompt start-up entrepreneurialism or produce venture capital innovative IT firms'" (Turcq, 2015).

If, as Shiller and Wang write, people are the subject of economics and policy formulation is its principal aim, and the work and methods of historical research and exposition can shed light on both, then why are historians so ignored in economics? The answer is that they were not neglected until after World War II when a combination of historical forces conspired to enable a number of wilful men and women to impose a "scientific paradigm" on their subject that eliminated historical and institutional economists from the discipline. Historians still read the works of Schmoller, Sombart, Weber, Veblen and other once famous economists but in economics their work has been dropped to be replaced by the new so-called scientific paradigm. George J Stigler in an article published in 1964, notes how quickly after the war neoclassical economists and econometricians triumphed over the old study programs:

> "Whereas in 1892-93, forty percent of the references cited in American economics journals were in foreign languages and half of these in German, total foreign language citations have fallen to less than four percent in recent times and German has almost vanished as a foreign language from American economics" (Stigler, 1965, p. 47).

Stigler went on: "If references to Schmoller are now rare, references to differentials and matrices have made some sort of compensation" (Stigler, 1975, p. 47).

It is relatively easy for historians to find critics of orthodox economics to quote, like Syll and numerous other disappointed social scientists, e.g., Ackoff, Johnson, et al cited in the text. Their work combined with comments in the public media remind people every day of the

failure of the new paradigm economists to anticipate economic crises, to devise policies that can quickly end them, to even be able to decide "scientifically" when the Federal Reserve should raise or lower interest rates and how much. It is much harder, however, to find economists who can appreciate the historian's work, since the historical method, historical exposition -- history itself has been banished from their province. In their work, mainline economists and their critics read and quote the work of economists trained in the new paradigm almost exclusively. When historians make comments about policy decision making based on historical research, economists mostly ignore them – not because the historians are wrong – but because economists have in the last three decades of the 20th century created an academic culture that lacks the historical consciousness and knowledge necessary to dialogue.

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SUGGESTED CITATION:

Robert R. Locke, "History as a source of economic policy", *real-world economics review*, issue no. 74, 07 April 2016, pp. 49-63, http://www.paecon.net/PAEReview/issue74/Locke74.pdf