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SCHUMPETER : THE MAN I KNEW

by

Richard A. Goodwin

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BADIA FIESOLANA, SAN DOMENICO DI FIESOLE

SCHUMPETER : THE MAN I KNEW

I make no apology for writing about my personal experiences and perceptions about this great economist. His published works are there for everyone to read, and they read very well indeed, so that I excuse myself from the quite unnecessary task of giving an account of his published works.

My first experience of him was in 1932 or 1933 as an undergraduate student of political science at Harvard. He addressed a society, of which I was a member, on the subject of marxism. Of what he said and how he phrased it I remember nothing, except that he was in some ways hostilely critical. As an ardent young, left-wing socialist, in the midst of the gravest crisis of American capitalism, I was outraged and very hostile.

Hence it was only natural that, when I returned to Harvard in 1937 as a graduate student of economics, I viewed with much reservation the lectures by Schumpeter, which all graduate students were expected to attend. In the course of that year my attitude gradually changed from scepticism to increasing admiration. His knowledge of current and past economic theory was prodigious, but uniquely, that weight of learning, never blunted the sharp edge of his critical, analytic power. An important and characteristic fact was that he never mentioned his own work in lectures. Indeed in his History of Economic Analysis there is no substantial mention of his own contribution, although it certainly belonged in the last section. By contrast he made great effort to read and understand the rapidly growing contemporary economic literature. During the decade 1939-49 my admiration and affection for him grew and with it a certain intimacy. In fact it is possible that Paul Sweezy and

myself were his closest American friends, along with a young student of mine, Alfred Conrad.

This leads to the problem of his relation to Marxism. On the surface it is surprising that, for someone of rather strong conservative, even reactionary, attitudes, he should have acquired two close friends who were youthful, vociferous socialists. But on closer inspection the contradiction disappears : he loved knowledge and intellectual enquiry and disputation. He had a real zest for intellectual talk with anyone, of whatever view, if he considered his opponent in possession of ideas worthy of serious consideration. By contrast he could be devastating about someone he did not respect. As an example he was asked to introduce, before a distinguished academic audience, a colloquium on a new book about the theory of games and economics, which he did with the memorable opening sentence : "This is a book written by a mathematician who knows nothing about economics and an economist who knows nothing about anything".

Alone amongst orthodox economists, he studied Marx's works thoroughly and gave them careful consideration, both critical and constructive. His love/hate relation with Marxism was life-long. One of his last essays was on "the Communist Manifesto in Sociology and Economics" (1949) and he was much concerned with the subject in his student days. In Böhm-Bawerk's seminar there was much discussion of the whole problem, fellow members of the seminar being the distinguished neo-marxists Otto Bauer and Rudolf Hilferding. Böhm-Bawerk himself wrote an influential book attacking Marx on the ground that he could not close his system.

Whether Schumpeter found Marxian concepts sympathetic because they fitted his own previous views or whether, in the central European context, prolonged exposure to Marxism led him to change his views, I never found out. However it came about, there is no doubt that in certain central methodological concepts their thinking ran on the same lines. Like Stendhal or Byron, a century earlier, Schumpeter was deeply divided : his emotional roots and sympathies were with the past, conservative and right-wing; on the other hand, intellectually and in estimation of where the future lay, he was ahead of his time in the U.S.A., very left-wing and progressive. He thought capitalism was dying and would be succeeded by socialism, though he did not like, for himself, the prospect.

There are certain central aspects of his theoretical position, what he called his 'vision', which are of special interest because of their ambiguous nature. I propose to discuss a few.


There is an apparent contradiction between his position as derived from Marx and his clearly expressed opposition to the German historical school. It was a kind of simplistic empiricism which he opposed : what he sought was an intelligible integration between historical events and economic theory. What he took from Marx and developed with great insight was the history of technology and its revolutionary impact on the economic structure and functioning of the capitalist system. Marx published Volume I of Capital in the same year as Darwin's Origin of Species. These two books, which, in their different ways, were destined to have such a profound effect on the whole intellectual and social life

of our time, had a methodological kinship, as Marx was quick to realize, though his admiration for Darwin was not noticeably reciprocated. Marx proposed to do for society what Darwin was doing for animals, to see it as an evolutionary process, generating new forms, and in this Schumpeter is to be regarded as an important follower : he elaborated the theory of technology as the central driving force of capitalism.

The distinguished French mathematician René Thom has proposed a strikingly new and influential concept in a book entitled Structural Stability and Morphogenesis. If one reflects on that title, I think one can reasonably conclude that it encapsulates what Schumpeter's elaboration of Marx was all about. Regarded as a Thomist, we can see that Schumpeter was asserting that Capitalism is endemically unstable structurally and discontinuously generating new social forms. There, however, all similarity between the two men ends : we should perhaps call this structural instability of the second kind, to distinguish it from the more familiar kind defined in terms of changes in type of motion. The consequences of such a, possibly over-ambitious, formulation are somewhat awesome, since there is an implication that the traditional methods of classical mechanics, much deployed by economists, are inapplicable. Some new forms of dynamical analysis are being developed, and these are plausibly as appropriate to economics as they have been found to be in biology and the natural sciences. It also helps to explain why the vast efforts in the accumulation and analysis of economic statistics have produced so little durable results.

This brings me to his curious relation to mathematics : already in 1906 his doctoral thesis was on the mathematical method in economics. Yet in spite of a life of intense intellectual activity, he never, for some reason or other, managed to practise what he preached : he never succeeded in using mathematics with any facility. Already in Bonn he acquired Erich Schneider as a colleague because of his mathematical background. I remember him sitting in boring faculty meetings, working at elementary math problems. Ultimately he even collaborated with a mathematician, Leonard Crum, on a book of elementary maths for economists (characterized by him as 'from creeping to crawling').

In spite of his own short-comings, he remained throughout his life a firm advocate of quantitative logic. He was a founder-member of the Econometric Society and regularly intoned the virtues of three-fold creed - Theory, Mathematics, Statistics. Amongst his papers at his death I found the manuscript of a lecture given in Japan, on his return to Bonn from the first meeting of the Society. This essay, of exceptional interest, though I judged it inappropriate for the History, was, for some reason, never published by him, but now, happily, is at last available (Journal of Economic Literature, September 1982). In it he complains of the confusions and disagreements amongst economists, maintaining that it was not the difficulties but a lack of rigour which was bringing the profession into disrepute. He confidently believed that the theoretical structure and methodology was sound, so that, allowing for diverse political premises, socialist or liberal, there could be no analytic disagreement.



Yet he himself, in the midst of the greatest depression since Napoleonic times, steadfastly rejected totally the Keynesian Revolution, not because Keynes was less committed to capitalism than he was, but because Keynes blandly denied the neo-classical, Walrasian orthodoxy, which he considered to be substantially complete and unchallengeable.

One of his favourite diversions was to discuss who was the greatest economist : he always proposed Walras, I opposed with Wicksell. This is surprising since Walrasian economics were essentially statical, whereas what Schumpeter had contributed to economic thought was the quintessentially dynamical character of capitalism. Schumpeter confessed that when he went to visit Walras he had failed to elicit any interest in dynamics.¹⁾

Nonetheless his reason for idolizing Walras was that he was the first, and, in essence, the unique, person to ask the fundamental, correct methodological question of what was the true rationale of the price mechanism as a resolution of the economic problem. It is a position with which I have more sympathy now than I had then.

Another issue is his attitude to von Neumann's contribution to economic theory (which I tend now to regard as the greatest single analytic advance of this century). In his History, he mentions the seminal paper on General Economic Equilibrium only in a footnote (p. 968). This is astonishing when one realizes that it accomplishes what Walras failed to achieve, and more importantly, supports at crucial points, the heterodoxy of both Schumpeter

and Marx. The matter is still more surprising since Schumpeter did realize the importance of the thing. I know because, when it first appeared in German (from the Menger seminar) in 1935-36, he told me that a very important paper had been published and asked me to read it and report to him (to my great shame and loss, I told him I thought it useless since it treated capital in the unrealistic way of joint product).

Schumpeter should have had a very specific reason to be pleased with the von Neumann paper. He had quarreled with his teacher Böhm-Bawerk because ~~he~~ ^{he} maintained that in the stationary state the rate of interest would be zero. By this he did not mean that he was only interested in equilibrium states, but rather as a way of showing that the nature and functioning of the economy could only be understood dynamically. This fundamental point on the rate of profit has always been ignored or denied by economists : Böhm-Bawerk ejected him from his seminar and tried to keep him out of the profession in Austria; it was only by direct intervention of the Emperor (for mysterious reasons) that he was able to obtain provincial posts, first in Cernowitz and then in Graz.

At last in 1938, he was given magisterial support by von Neumann's central proposition that only by perpetual growth at a positive rate of profit can capitalism achieve an equilibrium. True, he could have objected to the assumption of a constant technology, but, unfortunately, I do not remember what his own verdict was, nor did he leave any indication of his views in the notes he left for the unfinished final section of his History.

The third and most important aspect is his 'vision' of the historical, evolutionary nature of capitalism, a point of view which puts him in a select small band which mainly consists of him and Marx. I had returned to Harvard with a burning desire to deploy mathematical business cycle theory to unravel the mysteries of capitalism. Schumpeter listened sympathetically, and at length, to my telling him that what he needed was mathematical cycle theory to put his own theory properly. With the humility of a truly dedicated spirit, he said to me, if I would give a course of lectures on the subject he would attend, which, in the event, I did (he persuaded Gottfried Haberler also to attend). Thus I achieved the, no doubt unique, distinction of being his pupil and his teacher. And I thought I had convinced him, for he was very receptive to a number of the methods I utilized. Therefore it came as a great shock to me to find that in the very last paper he ever wrote, before dying in his sleep in his home at Taconic, he said the future of research lay in the study of the records of the great business enterprises - no mention of econometric model building and testing!

Now after many years, and in view of the poor results of model building, I sympathize much more with his point of view and see it as the logical culmination of his own unique contribution. Like Marx he was a student of the morphogenetic nature of capitalism. The economy is not a given structure like von Neumann's model, nor a collection of identical hydrogen atoms, it is an organism perpetually altering its own structure, generating new forms. Unlike most organisms it does not exhibit durable structural stability : it is perhaps best thought of as a

kind of hyper-Darwinism, perpetual evolution. We are so familiar with it, we normally do not realize how remarkable it is. It is not like morphogenesis in animals and plants, where the species is programmed to generate a particular structure, and exhibits structural stability by creating the same form for thousands of years. Rather it is analogous to the much disputed problem of the generation of new species.

The economy is unsteadily generating new productive structures. In this sense Schumpeter was profoundly right to reject the elegant new mathematical models : they are the analysis of the behaviour of a given structure. He saw that not only was the economy creatively destroying parts of its given structure, but also that one could not analyze a given structure, ignoring that this cannibalism was going on.

The most difficult aspect of this is to explain why a large number of independent technical innovations, which seem to occur fairly continually in capitalism, nonetheless get bunched, or occur in 'swarms'. My own view then was, and still is now, that his original formulation was wrong in the sense that it was too neo-classical, of the partial equilibrium type. The analytical framework is too narrow, too fragile, to carry the weight of explaining business cycles. More serious is that Schumpeter absolutely and completely refused to salvage his theory if it involved help from Keynes's General Theory.

I many, many times tried to convince him that the theory of effective demand showed how a steady flow gets converted into an oscillation.

The explanation for this blindness (somewhat endemic in the Harvard faculty, and elsewhere in the U.S.) is not easy to explain. Lord Kahn who knew both men, suggests that it was jealousy on the part of Schumpeter. I do not think so : Schumpeter did not hide his attitudes and I would have perceived a strong element of jealousy. My explanation is rather different : by the time he was writing the General Theory Keynes had forgotten some of his economic theory, and, like most English economists of his generation, he never had known the great Austrian, German, French and Italian traditions. His ignorance or innocence of tradition meant that he could the more easily liberate himself from the shackles of the full employment assumption. But for Schumpeter this represented a failure of intellect and knowledge, and so was unforgiveable.

Schumpeter was a very romantic person in the nineteenth century mode : he was in some ways not unlike Lord Byron. To begin with there is a persistent rumour, which remains, to me, unconfirmed, that he was the illegitimate son of a very highly placed Austrian noble. Like Byron he was a super snob and liked to épater the snobs by espousing unpopular and even progressive causes. Thus, in the First World War he was pro-English in Austria. And in the Second World War he was pro-Hitler, saying, to anyone who cared to listen, that Roosevelt and Churchill had destroyed more than Genghis Khan. He hated cant and enjoyed opposing the conventional and banal in society. Thus though he was a convinced theoretical anti-semitic, he, in practice, behaved differently. For many years the head of the economics faculty was a stupid non-entity called Burbank.

This man had complete power of decision over all junior appointments and in this capacity he operated a numerous clause to keep down the number of Jews on the faculty. When his fellowship came to an end, Paul Samuelson, who was acknowledged to be the brightest star in the youthful sky, applied for a job and was refused by the said Burbank. Schumpeter, who had no particular affection for Samuelson, and none whatever for Jews, nonetheless was so outraged that he resigned his professorship and accepted one at Harvard's rival, Yale. The outcry was so great amongst all his students and most of his colleagues that he was happily persuaded not to leave.

And then who but a super-snob would have openly and repeatedly said that his aim had been to be best as lover, as horseman, as economist!

He once told me an awe-inspiring, specific number for the women he had known sexually. But he was no simple sexist. When in Bonn he fell in love with a porter's daughter, who was only 12 or so years of age; he asked that he be allowed to arrange for her education and to marry her when she came of age. This he did, but alas, the poor girl, whose name was Mia, died soon after. In Cambridge he kept her picture beside his bed.

When Schumpeter was approached with the idea of translating his pre-war small volume on the history of economic analysis (Dogmengeschichte), a translator was suggested. He replied that there was no need to hire a translator; he would do it himself. And in fact his ability to write English was superior to that of

most of his colleagues, let alone a professional translator. He thought it would be a matter of six months or so to do the work. In reality more than six years later he died, unexpectedly, with the work unfinished. This vastly expanded effort was for him a tragedy but for us a happy legacy. He did not merely translate but enormously expanded the volume to include all he had learned and pondered on, over a period of some forty years.

The resulting History is a monumental work, not only for its learning and analytic sharpness but also for its effort to relate economic thought to the ambient movements in society and general cultural history. He worked continuously and nearly exclusively on the History throughout the war and early post-war years. Throughout his life whenever, in the course of a lecture, seminar or meeting, an idea or new perception occurred to him, he would write it down in a small pad of yellow paper and stuff the sheet into his pocket. At his death, amongst his papers were literally thousands of these notes, many of which he used in various portions of the book.

The History was nearly complete except for the most difficult and important last section, upon which I worked, and, in doing so, could see his methods. For many important topics he had composed not one but two or three versions, in order to get one that satisfied his high standards. I shall note three which struck me.

In the Japanese lecture he attributes Ricardo's failure to treat the level of output as resulting from his lack of mathe-

matics. He had four unknowns and needed to solve four equations : instead he reduced them to one. Wages are given (Malthus); rent is zero on no rent level; profit is output less rent and wages; and [~]output is not determined since he only deals with distributive shares. Marx was deeply influenced by Ricardianism and indeed seized upon a central point that Ricardo had chosen to ignore - that profit is simply the difference between price and cost and hence a surplus arising from technology, not a reward for any productive contribution. However, it seems to me that Marx and, to a lesser extent, Schumpeter, tended to repeat the Ricardian error. The problem consists of two parts; the size of the product and how it is shared. This comes from the Classical and Neo-Classical view that, because of market-clearing, the size of the output is uniquely determined by the stock of 'factors'. Not so, some of the time, indeed most of the time, there are unemployed capacities and labourers. In distributive shares, the various components add to one, and hence the more of one, the less of another. But with unemployed labour and capacity, higher wages can mean higher profits, and higher profits can lead to, and usually do lead to, higher wages. Shares are of great importance but apply to any level of output. Schumpeter was very aware of the level of output, but tended to think in terms of variations due only to technological innovations. This helps to explain his irrational hostility to the General Theory.

He was always interested in the various meanings of Say's Law, and at some point an idea about it occurred to me and I gave him a brief note on it. I no longer remember what it was,

nor was it amongst his papers, but he carefully recounted its contents in the book. Finally, there was the question of Edward Chamberlin and the origin of the theory of monopolistic competition.

There was a view in Harvard that since Chamberlin in his academic life was a totally barren figure, he could not himself have arrived at so original a concept : he must have borrowed it from some teacher (usually thought to have been Allyn Young).

Schumpeter hoped that by writing a carefully constructed letter to the absent (on war work) Chamberlin he might elicit a definitive clarification. The reply was disappointingly unhelpful, but he scrupulously incorporated it as an attested statement on the origin of the idea.

Schumpeter was throughout those last years frustrated by the unending labour involved in completing the History. He complained regularly of being physically and mentally exhausted by the work and he felt in general less well, which proved portentous. But he felt especially frustrated because, though he refused to lower his standards, he desperately wanted to finish the work, so that he could get on to other work closer to his own personal interests. He repeatedly told me he had five more books which he wished to write, and which he was being kept from writing by the History. He recounted to me roughly the scope of the various books, but alas, I have completely forgotten them. They were to do with cultural history, the relation of economic concepts to the wider social background, semi-sociological works,

with a strong flavour of the history ideas.

He had no premonition of his death and he died peacefully in the night, undoubtedly never knowing that those five books would never be written.

FOOTNOTE

1) This is a matter of some importance. Because Walras formulates his search for a solution in terms of a succession of steps, it is sometimes maintained that this can be a dynamical process. In fact it is not so and thanks to Schumpeter, we have Walras's own statement that the solution is logical not temporal. Schumpeter formulated it so in his teaching, using the notion of re-contract to demonstrate that Walras was not considering disequilibrium states, and went on to state that he remembered "a conversation with Walras in which he tried but completely failed to elicit the slightest symptom of interest both in dynamical approaches and in a theory of economic evolution".