# The Empty Well of Historical Thought

A Review of *Perspectives On The History Of Economic Thought Volume Nine: Themes On Economic Discourse, Method, Money And Trade,* edited by Robert F. Hébert Aldershot: Edward Elgar 1993, pp. x + 214 ISBN 1-85278-660-4. £39.95

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> A young economist who believes that Adam Smith was the Smith who founded the Mormon faith will only provide innocent amusement outside of Utah, at no cost to his professional status (Stigler, 1988, p.215).

Despite an abiding interest in the intellectual history of economics, George Stigler maintained a life long predilection for reducing this very intense interest down to the level of an idiosyncratic quirk. According to him, the young, thoroughly modern theorist has nothing of direct use to gain from such investigations. Although for Stigler, the subject, like many a first love, may never have lost its allure, he almost seems to suggest that the career minded economist should cultivate it only as something of an indulgence. Because of this, he not only had no qualms about, but strongly endorsed the fact that the subject itself was "not taught at a professional level in most great universities" (Stigler, 1988, p.207).

The potential charms he does list for this very peculiar endeavour would most likely fail to induce rising young academics to go pawing through those seldom opened volumes in the library stacks. Stigler sees these works as providing non-specific advantages: honing one's reading skills by carefully following a difficult non-mathematical argument, acquiring a more sceptical stance to received wisdom by realising the errors that famous economists had both accepted and committed in the past, plus whatever gain derives from a one way conversation with the acknowledged great minds of the past. What isn't however available for those interested enough to pursue this course is any relevant material for specific issues. One doesn't go to Smith or even to Marshall to learn about demand theory.

This attitude comes dangerously close to a belief that *'History is bunk'*. Stigler, while not precisely some latter day Henry Ford figure, did obviously believe in a 'market place of ideas' metaphor. In the continual struggle between contending camps, the soundest ideas, in this view, ultimately prevail. Starting from this assumption, it can only follow that current ideas must inevitably be superior to those of the past. Given his deeply held belief in market competition and the versatility of the price mechanism, what else could Stigler be expected to believe and advocate? Therefore, though we may be no more than midgets standing on the shoulders of giants, the working theorist need only keep his gaze focused upward.

This struggle however, with its inherent progress, owes more to the Social Darwinism of Herbert Spencer than to anything that Darwin himself cared to claim. There is no random and impersonal mechanism at work which can match environment to ideas in the way indicated by evolutionary struggle. The various sellers of those ideas are simultaneously quite active in attempting to construct an environment entirely conducive to their own particular merchandise. An assumption of objective progress in this march of ideas must be ingenuous at best. As always, the victors get to write the accepted historical accounts and define the terms of progress. It is therefore arguable that faulty economic ideas will eventually, under the extreme scrutiny of ambitious academics, be swept into deserved oblivion. In fact, it is not too outrageous to claim that any economist would be hard pressed to state a theory which has for once and for all been relegated to a deserved place in the universal dustbin of intellectual endeavours. Even those ideas which have been thoroughly thrashed, manage to return from the dead sporting a shiny new guise. Economists may be the ultimate recyclers. Since for many economists history of their discipline extends back no further than five years, the wheel is frequently rediscovered with great pride of craftsmanship.

In this regard then, I find myself being forced to disagree with George Stigler despite his well known reputation in the field. The economics of the past are not necessarily only dusty relics, lacking sufficient insight to light our future paths. By tracing the road that economics has travelled we become more aware of other alternatives, of how certain ideas gained sway. The best work in the history of economic thought gives us the ability to see current debates from a different angle by examining similar debates in the past. Issues in economics have been ever recurring. It is this valuable service that the best pieces in this recent collected volume actually accomplish.

The articles in the collection are all selected papers from the 1991 History of Economics Conference held at the University of Maryland. The bakers dozen of pieces are in order of appearance:

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| **Rhetoric and methodology** | *Roger Backhouse* |
| **Criteria of scientificity and methodology of the social sciences: Menger, Mises and Hayek** | *Daniel Dufourt and Pierre Garrouste* |
| **Kirzner vs. Becker: rationality and mechanisms in economics** | *Maurice Lagueux* |
| **Adam Smith on the limits to human reason** | *Spencer J. Pack* |
| **The renaissance of Adam Smith in modern theories ofinternational trade** | *Bruce Elmslie and Antoinette M. James* |
| **The great chain of being: a possible source of Malthus'smetaphysics** | *Scot A. Stradley* |
| **New England's depression-proof free banking system: the viewpoints of Henry Charles Carey and Charles Coquelin** | *Philippe Nataf* |
| **Thomas Attwood: crude inflationist or precursor of modern money and banking theory?** | *Sylvie Diatkine* |
| **Walras's `general' general-equilibrium model** | *Jan van Daal and Albert Jolink* |
| **Keynes, equilibrium and modern economic systems** | *Giovanni Caravale* |
| **Keynes vs. Pigou on the economics of war: isolating the properties of competing analytic perspectives** | *William J. Barber* |
| **`Discovery' of two-stage least squares: the predicament of a living primary source** | *Robert L. Basemann* |
| **Japanese modern economics, 1930-1945** | *Aiko Ikeo* |

This is a volume for selective reading done in any order that happens to strike the reader's fancy. Only a potential reviewer would feel obliged to plod through the volume step by given step. The editor does try to organise the papers by drawing them in under rather broad themes. The connections often can only be described as rather strained. This is fortunately no reason to avoid browsing through this collection. We would all, were it only possible, like to avoid spending time in the type of disappointing sessions which characterise most conferences. An edited collection of conference papers hypothetically spares us any onerous empirical testing. A skilful editor safely pilots us away from those typically Scylla and Charybdis type sessions that make present day conference going more of a necessary chore than a pleasure. By this I mean sessions in which the triviality of the topic induces boredom or in which the boring presentation creates an atmosphere of triviality.

Despite the lack of any noticeable theme, most potential readers should find enough in this volume to make its perusal worthwhile. Without such a publication, many of these article would likely have been scattered amongst a number of journals and thus possibly overlooked. All the articles are not calculated to suit every taste. Interests do, as they must, vary. Ideally, this should not matter. Any material can be made compelling through skilful writing. Unfortunately, it has become almost traditional amongst economists that writing standards need rarely rise above the serviceable in professional writing. The belief seems to be that the intrinsic value of the material itself should be sufficient for the discriminating reader. So we are usually given our helpings of insight in what can at best be described as unadorned prose. Perhaps indeed, most economists are aesthetic puritans who view any enticement on the part of an author as uncalled for strumpetry. But without such enticements, our pre-existing preferences are only reconfirmed. Far better if we could occasionally be tempted to stray into other areas as well. While the prose of this particular volume never rises above the unobjectionable we should be grateful that with few exceptions it never dips below a level of serviceable adequacy.

Nodding to the ever present wisdom of brevity, I will not attempt to summarise or comment on each one of the articles appearing in this collection. Instead I will for the most part limit my attention to those which provoked my interest. This of course is a standard which is necessarily personal. I hope though to avoid inconsistency by concentrating on those which most clearly demonstrate how and why the study of the history of economic thought can be fruitful in ways that extend far beyond the limited scope decreed for it by George Stigler.

In this vein, the piece by Giovanni Caravale is nearly a perfect representation of how not to engage in the study of the history of economic thought. Its value consists almost entirely in providing a warning to future researchers as to what not to do. It neither simply provides some basic raw material for future work, nor does it help us to understand current issues by exploring past problems. Caravale purports to be rationally reconstructing Keynes' General Theory using what he terms a classical notion of natural equilibrium. Only such an approach is capable "of supplying a satisfactory representation of Keynes' conception of unemployment as a 'dominant', non-transitory feature of contemporary economic systems" (Caravale, 1993, p.137).

What is the reality that lurks beneath this statement. Without acknowledging it, Caravale appears to be adapting a Neo-Ricardian long period equilibrium approach to economic analysis. This precludes any serious consideration of short period analysis. (Caravale distinguishes logically sequential events, short period/long period, from chronologically sequential events, short run/long run. This is to be in turn differentiated from the standard neoclassical ahistorical, reversible analysis.) Given that his task is to make sense of the Keynes of the *General Theory*, eliminating the validity of short period analysis seems to run counter to one of the few points of agreement concerning that much interpreted work.[1] With his pair of Neo-Ricardian glasses firmly in place Caravale has very few options. Either he can dismiss Keynes out of hand as confused and therefore not worthy of further consideration. Or, he can reason as follows: a) Natural equilibrium is the correct way of performing economic analysis b) Keynes is an acknowledged great economist c) Keynes must have, at least implicitly, analysed the economy in a way compatible with a classical notion of natural equilibrium.

Caravale has sufficient modesty to shy away from outright attribution of this approach to Keynes. What he does say is

> It is also important to emphasize that the procedure followed here - that of attempting a `rational reconstruction' of Keynes's theory in the sense specified above (the effort to show that the essence of Keynes's theory can be expressed in terms of a notion whose exact definition and implications may have become clearer after the *General Theory* had been written; and that Keynes's explicit position is compatible with such `reconstruction') - appears, in the light of his oscillations and his methodological ambiguities, to be preferable to the (stronger) thesis of the outright *attribution* to Keynes, however interesting this latter may be, of a specific conception of economic equilibrium (1993, p.153)

At this point a befuddled reader might ask, how much of Keynes' theory actually remains if the reconstruction runs so counter to his intentions? Perhaps we should accept an instrumentalist approach here. Caravale seems to be implicitly honouring Milton Friedman's methodology when he performs his idiosyncratic interpretation. What the actual author thought he or she was doing becomes largely irrelevant. What matters is what the author was perceived as saying or in this case, using a radically subjective approach, what Caravale prejudges Keynes as saying.

Caravale seems to favour a different meaning of equilibrium than Keynes. He also seems to confuse long period expectations with long period equilibrium. At least, his attempt to differentiate between the two is far from clear. Basically he doesn't succeed in mounting an argument for seeing Keynes through these particular lenses. While his long period equilibrium approach may adequately explain unemployment, he never sufficiently demonstrates that more standard short run models must inherently fail to do the job. One is then left wondering why he feels compelled to drag in poor old Keynes as a support for his unemployment analysis. The practice of using some well known work to push one's own agenda is at best a dubious exercise. Oddly enough, what Keynes shares with Ricardo is the ignominy of being thrust on a Procrustean bed of preconceived notions, of being trimmed so as to make some rhetorical point. Just as Marshall refused to concede to Ricardo a labour theory of value (Marshall, 1982, p.672) so Caravale refuses to grant Keynes the validity of short period analysis.

Where Caravale's work seemed to promise much of interest and relevance while delivering little of either, Barber's article is fortunately just the reverse. The thoughts of Keynes and Pigou on the economics of war seems vaguely interesting but a bit narrow if not verging on the obscure. But like the best in the history of economic thought, Barber uses this idea only as a starting point to deal with much broader issues.

It is usually supposed that the classical man of straw that Keynes so thoroughly pillors, bears the visage of A.C. Pigou rather than Marshall. Given the confrontational style of the *General Theory*, Keynes' deliberate attempt to stir the pot, it is often assumed that the real differences between the two were exaggerated for polemical purposes alone. There is some undoubtable truth to this. Yet the suspicion still remains that there might be something more fundamental and less purely rhetorical in Keynes' self professed "struggle of escape from habitual modes of thought and expression" (Keynes, 1960, p.viii).

To investigate what such differences might be, Barber cleverly looks at two books which closely embody an application of each economist's theoretical framework. In 1940 both economists brought out works dealing with the economic effects of the war and its expected aftermath. Pigou revised and reissued his 1921 book, *The Political Economy of War* while Keynes brought out a slim volume, *How to Pay for the War*. Pigou's approach was decidedly micro based while Keynes, as to be expected, depended on aggregate demand management. This in part reflected their quite differently maintained positions, the economist as policy activist versus the economist as academic theorist who at best can have only the most indirect influence on such action.

Pigou saw the necessities of war as an occasion of market failure. As always, failure of this sort, was to be righted by government intervention in the workings of the price mechanism, namely price controls and rationing. The war itself would partly be financed by saving (the indirect result of restricted consumption goods) and a post war capital levy. Keynes saw the issue not in terms of pricing but in terms of restraining aggregate demand. Enforced saving, deferred pay in his terminology, would restrain demand while at the same time financing the war effort. It would have the additional advantage of preventing a post war slump as the delayed consumption became the engine of increased demand.

Pigou saw any post-war unemployment as merely transitional in nature, the result of the expected need to restructure the economy generated by the changes the war would bring. Paring down certain industries and expanding others would take time but would pose no lasting problem. Over the longer term, however, the required reallocations should be manageable 'provided that the wage-policy of Trade Unions and the monetary policy of the State are properly married together' (Pigou, 1943, p.28). There were no overtones of Keynesian underemployment equilibrium here (Barber, 1993, p.166).

Echoes of this debate are certainly strongly with us today. In a sense, Pigou may be once again on the ascendant as much of the current debate over governmental intervention seems to be centred on structural adjustment and micro-policy, rather than traditional demand management.

Another interesting contrast on approaches to studies in the intellectual history of economics comes to the fore in two investigations of nineteenth century thought concerning the problems of money and banking. In one case, Philippe Nataf contributes potential grist for the mill. Basically he provides information, in this case the views and findings of Carey and Coquelin on free versus regulated banking. To bring these views to our attention is of course a service. Moreover an author should not be faulted for the paper he or she didn't write. Nonetheless, the work would be of even more value had it been tied in to the current debates over free banking as well as those concerning the effects of banking regulation. One can only assume that in such a short piece the author allows his readers to draw the obvious conclusions.

Sylvie Diatkine goes to the trouble of making these connections explicit when she looks at another nineteenth century writer on money and banking, Thomas Attwood. The links to later theorists such as Hawtrey and Keynes in their mutual concern with monetary policy and its relation to price stability are clearly traced. The relevance of Attwood's concerns with the interrelationships of money to the rest of the economy, especially the nature and utilisation of credit money did not of course end with Hawtrey and Keynes. Understanding how these issues developed, the on going debates among economists, can only help us gain a deeper understanding of more current issues.

It is not after all only our supposedly superior current understanding, though this proposition is clearly arguable, which helps to make the past more comprehensible. The process of studying intellectual history is at its best when it engineers a two way street. Bruce Elmslie and Antoinette James illustrate this point by re-evaluating Adam Smith in the light of recent theories of international trade. They not only restore Smith's reputation but at the same time provide insights into what these recent departures in trade theory mean, particularly those of Paul Krugman.

The new trade theories recognise the inadequacies of strictly comparative static models. The origin of such models in the work of Ricardo were a departure from the more dynamic, economies of scale and division of labour, approach favoured by Smith. Smith's previously undistinguished, but undeserved, reputation as a trade theorist rested largely on the general adoption of the comparative advantage framework as the correct standard of achievement. Thus by re-evaluating Smith in the light of these previously mentioned new theories of trade, we gain a greater understanding of the ways in which these new theories depart from the more traditional pathways taken by comparative static approaches. In effect, this is aided not only by analysing Ricardo's significant departures from Smith's theories of trade but also by taking Smith's theories seriously. This means not condemning Smith's approach out of hand simply because he doesn't adapt the familiar comparative static approach of Ricardo.

Methodological differences whether characterising trade theory or any other aspect of economics usually provide fertile grounds for historians of thought. The methodological issue which explores the importance of the assumption of rationality for the analysis of efficient markets is, like any other of such issues, still unsettled. Is it fundamental or merely an assumption of convenience, an aid to simplifying analysis? An exchange in the sixties between Becker and Kirzner sought to deal with this important issue. The significance of the problems raised, as well as the exchange itself, has to some degree been lost. Maurice Lagueux attempts to bring it back to centre stage. While it is quite easy to question some of the author's analysis, it does raise some crucial questions. Not only the more obvious one as to which operational definition of rationality will best further economic analysis, but also what characteristics are most fundamental to markets. Kirzner takes issue with Becker over the idea of intentionality. The Austrian tradition denies that a mechanical or clockwork model, such as the one that Becker plays with, can lead to any fruitful results no matter how well it be forced to mimic a functioning economy. If economics concerns choice, then choice, meaning intentional choice must be the starting point for any analysis.

George Stigler believed that sometime in the thirties, economics made a choice, perhaps conscious, to relegate its own history to the forces of benign neglect. Despite working and teaching in this field all of his very active career, Stigler never mourned this decision. In his view, intellectual resources are too scarce to squander on what are only peripheral endeavours. The very best articles in this collection demonstrate that in this particular instance George Stigler judged badly. One can only disagree with the statement he makes in his essay which poses the provocative question, "*Does Economics Have a Useful Past?*"

> Some historians of economics - Schumpeter is an eminent representative - believe that an understanding of the evolution of a science helps to understand its present structure. This claim may be conceded and restated as the plausible hypothesis that correct knowledge never has a negative marginal product. Nevertheless, one need not read in the history of economics - that is, past economics - to master present economics (Stigler, 1982, p.107).

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## NOTE

1. I do not mean to suggest that the way in which Keynes employed his short period analysis is not controversial. There are as well many passing comments and allusions to long period results. One might even argue that the vision of the work is essentially long run, chronologically speaking. However, the model itself remains steadfastly in the short period. You would be hard pressed to find commentators bold enough to claim otherwise.

   > Furthermore, he [Keynes] confined his *model* - though not always his argument - to the range of short-run phenomena ... it does not seem to be realized sufficiently how very strictly short run his model is and how important this fact is for the whole structure and all the results of the *General Theory*. The pivotal restriction is that not only production functions and not only methods of prod-

uction but also the quantity and quality of plant and equipment are not allowed to change, a restriction which Keynes never tires of impressing upon the reader at crucial turns of his way (Schumpeter, 1946, pp.511-512).

The fluctuations in real income under consideration in this book are those which result from applying different quantities of employment (*i.e.* of labour-units) to a given capital equipment, so that real income increases and decreases with the number of labour-units employed (Keynes, 1960, p.114).

The general price-level depends partly on the rate of remuneration of the factors of production which enter into marginal cost and partly on the scale of output as a whole, *i.e.* (taking equipment and technique as given) on the volume of employment (Keynes, 1960, p.294).

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