An Interview with Brian J. Loasby

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by

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An Interview With Brian Loasby

Born in 1930, Brian J. Loasby is generally recognised as the leading post-Marshallian economic theorist. He has published widely on the theory of economic organisation, methodology, and the history of economic ideas, including Smith and Keynes. His work includes the now-classic

with here was a of how business men necessarily make it clearly theory which could not conceivably There was wrong about the was that they not it, meant enterprise illogical of operating if it had presented, not as a of decision means generating results, maybe would not have methodological would not have into issues. weaknesses theory interest in history

NF: **I suppose you still feel, as Schumpeter did, that all good economists ought to entertain an interest in economic history?**

BL: **Yes, when one looks around and finds people judging situations without, apparently, any conception that somewhat similar situations have occurred in the past, that gives you an idea of the value of having some sense of history. For example, David Laidler takes this view, with which I agree, about monetary policy and monetary analysis that way back to the 19th century and back to the 18th century, a number of quite sensible things were said about monetary policy which have been presented as very modern discoveries.**

NF: **But this is doctrinal history, not economic history, although of course they are somehow related.**

BL: **Yes, it is doctrinal history, but most of the work on monetary issues arose directly out of particular problems and of course it was driven by the belief that money, and certainly good monetary management, could have real effects on the economy. The idea that money does not matter was a very strange idea to most of these people.**

NF: **David Ricardo may have been an exception.**

BL: **Well, I suppose David Ricardo might be one of those who held that if money was managed properly, it did not matter, but if you did not manage it properly, you could get into trouble.**

NF: **Your reference to an early political interest reminded me that one of your early articles was about regional planning [Brian J. Loasby. 1967. "Making Location Policy Work,"** *Lloyd's Bank Review***, January: 41-42.]. I wonder about the connection of that article to your other work.**

BL: **I think that was about what I was teaching at the time, that is, policy of regional development. I did some work when I was in Birmingham on relocation which was directly a policy issue in that it had to do with what was appropriate for governments to do when they were interested in relocations of industries. So I thought, among other**

things, that what I should do in relation to that was to go back to the record and see what had been tried. One of the things that was kind of fairly obvious at the time was that changes in location could invalidate the assumptions on which particular policy measures were based.

NF: **So there is some connection to your other work here - on decision making, for example?**

BL: **Yes, the work that I did at Birmingham was very closely related to decision making. It led to the rather simple point that if you are going to talk about decisions, you have to start by saying "Why did people feel there was a need to make a decision about that particular matter?" and "Why did firms start thinking of the possibility of moving?". In fact, the reason why you start thinking about it may have quite a lot to do with the way you think about it. It is a mistake to believe that every decision has, as it were, a natural form that people can discover: people think about things in different ways which indeed is very obvious if you think about the way economists do economics. The way you pose a problem can often have significant influences on the solution you come up with, the particular analytical result you come up with. One thing that really interests me about decision making is the way in which problems are formulated, and the obvious connection here with organisation: that organisational design is, as I think of it, quite very consciously intended to lead to problems being formulated in particular ways. The whole point in the Dupont change to a functional system was to persuade people to think about problems in different ways because the old way of thinking about them turned out to be not very successful in changing circumstances.**

NF: **In addition to regional planning and economic history, you have, of course, also been interested in economic methodology, the theory of the firm, perhaps a little bit of monetary theory, in epistemology, in Keynes, etc. It is rather an impressive range of different topics, and a natural question is whether there is a unified theme to all of these?**

BL: **Well, I think maybe there is** *now*. **Looking back I find myself attracted towards Henry Mintzberg's concept of an emergent strategy. What basically I was all about, I did not know at the time; and at the end it seems to be about organising knowledge, perhaps in the form of the question, "How shall we as economist organise our knowledge about the ways in which people organise their knowledge in the economy?". Putting it that way, of course, is very economical, because maybe you can transfer ideas from one level to the other level. I think that is what I probably have now come to specialise in doing. It is all accidental.**

I only became interested in how we organise our knowledge, because there seemed to be something wrong, in some respects, in the ways in which economists were organising their knowledge. Going back to an earlier point, having been taught micro economics as if it were a theory about how a business man make decisions and finding

out that it did not match very well, the question then arose: "Why do economists do this sort of things?". I certainly, as an undergraduate, had no interest in methodology at all - it was generated by problems. What one chooses to think about rationally is determined by what one sees as a problem situation, which is at least one bit of the Herbert Simon approach. Decision making is when there is a problem.

NF: **You mentioned your undergraduate experiences. Where were you educated?**

BL: **In Cambridge.**

NF: **Were you exposed to the teaching of John Robinson?**

BL: **Yes, John Robinson was, of course, the very exponent of imperfect competition theory and there is another aspect of that, in addition to what I have alreadt said, namely the Robinson/Chamberlin dispute. Chamberlin always claimed that his theory was different from John Robinson's theory, while Robinson said that it is the same except that Chamberlin began to back off because he did not want to draw the obvious conclusions about the obvious inefficiency of the market system. So that was another kind of issue, which I think I sorted out quite early by recognising that they were two different theories based on different assumptions.**

NF: **Chamberlin's being more process oriented?**

BL: **Well, I do not know if I would had said process oriented at that time, but I would now. I think I actually have written somewhere that Chamberlin's theory can be regarded as being very similar to Israel Kirzner's theory, except that Chamberlin chose to express it in equilibrium language.**

There is this wonderful little book (in terms of ideas per page), one of the best thing I have ever read by an economist called Romney Robinson on Edward Chamberlin. Romney Robinson says that Chamberlin was writing about a situation in which there was diverse demand and an important point here is that Chamberlin started with differentiated demand, while Joan Robinson starts with the problems with economics of scale and there is no theory whatever in Joan Robinson's book about why the demand curve should fall. It has to fall because this is the only way you can get equilibrium so you do not argue about it. Well, of course, having made them fall, that is the way you get an equilibrium, and then you discover that the equilibrium is inefficient. Then you say, well, the inefficiency must be caused because consumers do not understand their preferences properly or something. The preferences have no particular validity and therefore any reorganisation of industry which violates these demand curves is perfectly justifiable. But of course, Chamberlin said that these preferences do have validity and any adjustment generates welfare losses; at least, there is no presumption that the change will be an improvement.

In Chamberlin's story, firms have problems of finding out where their customers are and it is actually what is called a process story: firms are trying out different product

combinations, testing them. Chamberlin's selling costs are part of the problem of finding out where the customers are and letting customers know what one now has to offer. Nowadays, one can see that Chamberlain's monopolistic competition theory is a link between Marshall's theory of external organisation and Coase's theory of transactions costs. However, that is of course is not the way it happened; it is a rational reconstruction.

NF: **Who have been the central influences in your thought?**

BL: **Well, I suppose one would have to say that we might have to start with Joan Robinson and quite unintendedly, not at all in the way that Joan Robinson would have wanted, but by creating the sort of problems I mentioned earlier. Also, Dennis Robertson, I think, in terms of attitudes. I keep being reminded of one of the things that Dennis Robertson wrote about highbrow opinion in economics being like a hunted hare, that if you wait long enough, it will come back to you. That seems to be more and more true, given the recycling of old ideas as brilliant new discoveries and it leads one to be less than overwhelmed by the value of whatever is being propagated at the particular time.**

Then in chronological order Cyert and March's behavioural theory of the firm was quite important in suggesting that there might be some other way of thinking about decision making than the neoclassical way; what generally is called the Carnegie-Mellon approach which was very much influenced by Herbert Simon, of course. What was important was not just that here was *a* **way about thinking about things, but the demonstration that there was indeed more than one way of thinking about things and that the standard of equilibrium of economics did not have an monopoly on rational thoughts.**

I think my introduction to Thomas Kuhn was also quite important. I was introduced to Kuhn by a research student we had in Stirling, hired specifically as an historian to look at things in ICI in a way which was not conventional inside ICI. He connected medical innovation with Kuhn's argument about paradigms, and his story followed Kuhn's argument that when you have just had a revolution and introduced a new paradigm, that is the worst possible time to try to introduce another one; new paradigms are extremely resistant to change. I think that that was important because there was no need for me to make the connection between Kuhn's story and commercial innovation because it had already been done for me by this young PhD student. I produced this piece for the *Economic Journal* **[Brian J Loasby (1971), "Hypothesis and Paradigm in the Theory of the Firm",** *Economic Journal* **81: 863-885], which was not taking the original Kuhn apply to economics but taking commercial innovation interpreted** *á la* **Kuhn and applying it back to economics.**

NF: **When did you discover Austrian economics?**

BL: **That was after nineteen-seventysix, that is, after** *Choice, Complexity, and Ignorance* **was published. I later learned from some of Austrians, among them Israel Kirzner, that the book was somewhat similar to what they were up to, and they complained a little that I had not taken notice of the Austrian tradition.**

NF: **You hadn't come across Hayek's work before 1976?**

BL: **No, not really. It was undisturbed Cambridge religion in the 1940s and early 1950s that Hayek was wrong and we did not need to bother about it. I guess it is interesting that there were one or two things about what we were taught in Cambridge which I was questioning and there was lots of other things which I was not.**

NF: **But in the years that have passed since then, you basically have questioned almost all of you Cambridge background, haven't you?**

BL: **I have never been able to question very much at a time, I think. I got a note somewhere of something G.K. Chesterton once said, that a man must be orthodox on most things, or he will never have time able to practice his own particular heresy.**

NF: **I have noticed that you have been referring more and more to Adam Smith. You really seems to have become increasingly interested in his thought.**

BL: **Yes, certainly over the last few years, I would probably put Adam Smith on the top of my list and in particular because of the coherence of Smith's ideas. I have actually been making some notes on this recently. Smith starts with the question of knowledge - the essay on "The History of Astronomy" - trying to impose patterns. Then comes the** *Lectures on Rhetorics* **which moves beyond the earlier idea of the study of various styles of poets and authors. His interest lies in looking up the general purposes of communication against the background of the limits of people's knowledge and their wish to have organising principles. He cites the Newtonian method as being the most effective. He had already said that Newton's own theory is not necessarily a statement of the truth, but that it is a pattern that Newton had himself invented so that you have this idea of rhetoric as a system of persuasion, which maybe has some connection with the truth but is not the same thing. But then you go to the** *Theory of Moral Sentiments* **which is a coordination theory.....**

NF: **There is also a link to economics here [in** *Lectures on Rhetorics***]: Smith has a sentence in which he says that exchange has a crucial element of persuasion.**

BL: **Yes, I was going via** *Theory of Moral Sentiments* **to make that point. You have if you like, the marketing concept in Smith, exchange involves putting oneself in other peoples shoes. If you think about this, this famous quotation - the butcher, the brewer,**

and the baker, and about appealing to their interests, well, you cannot appeal to their interests unless you know what their interests are. It is not purely self-interest - that is not Smith - that is not how economic activities are coordinated in Smith. All this is underneath *The Wealth of Nations*, **which is a part of Adam Smith's grand design.**

NF: **But surely all the parts of this grand design are not explicitly coordinated, as it were, by Smith?**

BL: **No, they are not, but they all belong together. You can say that Adam Smith believed that all these fitted together, that nowadays we know better, that we do not need to bother, but I am inclined to think that we still need to bother about it.**

I am sure Marshall did. A very important way to interpret Marshall is as someone in the Smith-tradition, who clearly felt himself to be there. I think I should say something about Marshall. But I find this very difficult, because on the one hand a lot of the things I have done I think of as being Marshallian. On the other hand I do not think I have actually discovered very much directly from Marshall. Go back and read the *Principles*, **and you find that Marshall have already said it, but I did not see it there until I worked it out for myself . I don't know how many times I read the** *Principles* **before I actually noticed the phrase "external organisation". My impression is that other people have a somewhat similar relationship to Marshall. The first time you read the** *Principles,* **it all seems perfectly obvious, though when you read it a second time it does not seem quite so obvious, and when you read it the third time you begin to appreciate that there are lots and lots of things there which you do not notice unless you are being sensitised to them. I think this a general proposition.**

NF: **Your interpretation of Marshall is very Loasbian, as it were. What have been the reactions from other Marshall specialists?**

BL: **The three leading Marshall scholars in the world, John Whitaker, Denis O'Brien and Peter Groenewegen have all expressed agreement on parts of my interpretation - this is probably something I should check up on - but I cannot think of any major disagreement with any of them.**

NF: **What about the point, which I think can be found in your work on Marshall, that he was indeed a genuine evolutionary economist, that he really operated with a population perspective?**

BL: **Look, I think I would have to say that, though I might have given other impressions, I ought not to argue that Marshall's** *Principles* **is some sort of basics text in evolutionary economics. It is in the true sense a neo-classical book - a new version of classical doctrines, which is not what we nowadays mean a neoclassical book - it is a neo-***classical* **book. It has a lot of Ricardo in it and it has a lot of equilibrium models in it and things that could be developed in that kind of way and it has a lot of Cournot**

in it. But if you take it seriously what Marshall says in those prefaces, that economics is a difficult subject, that you have to take it step by step, that you start with the simplest things, etc., and that the big questions are the questions about economic progress, then you obtain another picture. Marshall, the Victorian, wants to improve the conditions of the people and you can't do that by more efficient allocation, you just have to be more productive economically, and also very importantly, you have to change people's preferences. That is where he would like to get to, but you do not get there in *Principles***, Volume One. You would get further in** *Principles***, Volume Two, but of course Marshall never got around to writing it. And even then, the theory of evolution was not very well developed at that time. But you do have this Smith/Darwin combination; for example, the statement about progress depending on increasing differentiation of function. This is Marshall doing what Smith praised Newton for doing - bringing two separate things together: Marshall says biological and economic organisation have the same principles. Of course, he also has this statement about the importance of organisation.**

NF: **That organisation aids knowledge?**

BL: **Yes. Knowledge depends both on the division of labour, in the way Smith told it, and on organisation. You get this differentiation and organisation story, and this is the growth story - this is the Smith/Darwin growth story in Marshall. It can be found in** *Industry and Trade*; **there is a chapter about the American way of doing things and a chapter about the German way of doing things. It is incredibly up to date in some ways; very close to what some people are doing on national systems of innovation and to Porter's work on the competitive advantages of nations.**

So that is a very important part of Marshall; it is not the hole of Marshall because he is interested in price theoretic questions. But even in that, the statical equilibrium stuff, if you read it all carefully, there are lots and lots of warnings about that this is the mechanical model and you have to be careful when you apply it, etc. This is the kind of mess that people got into in the 1920s in the debate on increasing returns.

NF: **And in the 1930s in the discussion of socialist allocation, as Mises and Hayek pointed out. I suppose this similarity between Marshall and the Austrians also helped you become interested in Austrian economics. I also want to ask you about your opionion about what goes on in Austrian economics these days?**

BL: **Well, I think that Austrian economics is getting more interesting as it is getting less monolithic. This is perhaps partly a matter of self-confidence; there was a period when they felt that they had a particular position to defend against almost everybody else in the world; you were either inside the circle or you were outside, and if you were inside you had to stick completely to the doctrine.**

NF: **One could argue that there is a large number of fellow travellers, such as Dick Langlois, Ulrich Witt, yourself.... - and that they are actually doing the good Austrian work.**

BL: **Perhaps that is right. In this connection, I think that if Lachmann had been alive and had been a bit younger he would be much more welcome among the Austrians now. He was regarded as a dangerous figure. He was inclined to give too much away - saying nice things about Keynes, for example.**

NF: **Saying nice things about Keynes is just among many similarities between your work and Lachmann's work. Another one could be your shared admiration for Shackle's work. I suppose you felt rather close intellectually to Lachmann?**

BL: **Yes, one of the major regrets is that I did not had more to do with him. I had the opportunity to starting a correspondence with him a few years before he died, but I never actually met him. I guess that the reason why I didn't was that I didn't feel that I was quite ready; there were things that I would like to say, but I did not really have myself sorted out. I will always be in a situation where not having myself sorted out, I think. But I had a really interesting exchange with him about the way Frank Hahn's attitude had been changing, and Lachmann said something to the effect that Hahn is much too intelligent and much too keen a observer of the economic scene not to see the danger of particular sorts of developments. I actually later met Frank, who was, first, very surprised that Lachmann had though it worth while to send his best wishes, but secondary, very pleased to receive them.**

NF: **Yes, Hahn is interesting. You know that he has relatively recently ("The Next Hundred Years",** *Economic Journal* **101 (1991): 47-50) written something like that "economics in the future will return to its earlier liaisions with theoretical biology", or something like that.**

BL: **Just after that was published, a number of people that were shaking their heads and were saying "We wish Hahn had not written this; he is betraying the tradition of economics".**

NF: **But of course you agree with Hahn. This brings us to evolutionary economics.....**

BL: **Evolutionary economics is, I think, a much less well defined field than mainstream economics, which in itself have become difficult to define nowadays.**

NF: **There has latest been this tendency to think of evolutionary economics in terms of** *methods***, such as genetic algorithms, classifier systems, etc., and as being** *defined* **by those methods. Do you support this view?**

BL: **Well, my idea of evolutionary economics simply is that you need three things: one is that you need some kind of system that generates variety and two is that you need some kind of selection processes, and, three, you need some kind of persistency. Now, it seems to me that there is no reason whatever why any of these three components should be analysed in the same way that biologists analyse the equivalent in biological systems. Although I think there is a requirement if you are thinking about human processes, human processes should not be in** *conflict* **with the biological story of what humans are actually capable of doing.**

I don't think I have any exclusive views on *methods* **- I just think you should always be very careful. For example, it doesn't seem to me to be illegitimate to make equilibrium part of one's way of understanding economic evolution, particularly if you think of equilibrium as a way of explaining the way in which something persist, because you do need something to persist in an evolutionary story. But the general equilibrium model, which explain why everything persists - nothing would change - seems to be relevant only as a kind of foil, a description of what the world would have to be if no evolution would ever take place (this is a Hahnian argument).**

It does worry me when people try to use equilibrium theory as a way of avoiding explaining the processes. It worries me a little bit that a lot of evolutionary biology is actually equilibrium biology. I thought that the argument was that only by understanding the processes can we properly understand the outcome and only by understanding the outcome can we understand the processes. Walras' idea was that you start with the equilibrium. If you cannot show that there is an equilibrium solution, then there is no point in trying to find out how you get to it. So it is perfectly legitimate to say that first of all, let us see if there is an equilibrium and then go and see how the process is. It seems that people reach the conclusion now that the requirement for an existence proof is incompatible with the requirement of telling a process story. I think in fact that this is what Franklin Fisher has done [Franklin M. Fisher. 1983. *Disequilibrium Foundations of Equilibrium Economics*. **Cambridge: Cambridge University Press]. I do not think that anybody has gone beyond Fisher, who says that you need some highly implausible conditions, namely "no favorable surprises": you do not discover any new opportunities on the way - but any story about process and development is about discovering opportunities on the way.**

NF: **General equilibrium theory really seems to have been falling out of favour among mainstream economists. It probably all has to do with the rise of game theory, applied game theory, the rise of industrial organisation economics, etc.**

BL: **Yes, it seems to me that one of the attractions of game theory for the more methodologically inclined is that it appears to solve the equilibration problem. The equilibration problem is false trading and the false trading changes the equilibrium. But in game theory everybody just sits down and works it all out. You do not actually play the game from the beginning and there is no possibility of false trading.**

NF: **Perhaps we should finally talk a little bit about the firm and organisations. I t strikes me that you really began, not as an organisation theorist, but at least as someone very interested in organisation theory, and in some ways** *Choice, Complexity, and Ignorance* **is an organisation theory book. But you don't seem to be very much interested in organisation** *per se* **any longer.**

BL: **That is probably true. But I have actually very recently read the volume Oliver Williamson edited about Chester Barnard and have gone back to reading Barnard. I am intrigued by the point that one of the functions of the executive is changing peoples preferences inside the organisation. This is something that needs to be developed. Safeguards against opportunism are an important part of the story, but there is a lot more to the story than that. And, of course, the Barnard argument about changing preferences is of course a process view, and the fundamental trouble, I think, with Williamson's transaction cost economics is that it is an equilibrium story.**

NF: **And it is also a story that neglects all sorts of cognitive factors, whereas the main organisational design problem perhaps really is getting everybody on the same cognitive wavelength.**

BL: **Well, maybe, but there is the other side of that - the opportunity costs of that - if you get everybody on the same wavelength, there are lots of innovations that will never happened. The very important policy implication of that is that national champions are likely to be a very bad idea.**

NF: **Do you see transaction cost economics and work on firm capabilities as two distinct fields or as converging or related fields?**

BL: **I think this is one of the things Mark Casson is actually very good at thinking about. He begins by reinterpreting the transaction cost story of firms, and sees them as intermediaries in the economic system that reduce the transaction costs compared with the system in which the consumers have to deal directly with factor owners. One of the very important aspects of this is that in standard transaction cost economics, transaction costs have, if you like, the properties of standard production functions: for any transaction there is a determinate cost for anybody who wishes to take this transaction. I think it is a very valuable part of Casson's argument that the costs of transaction depends on who is doing the transaction and how they are doing it. Firms develop specific capabilities in transactions.**

The question that Coase was getting at in 1937 about how to distinguish the costs of making arrangements from the costs of performing the actually operation of this generating product or services is clearly something that is very important. However, I think there are a couple of problems here. One is that the convenient separation of production activities and transaction activities comes at a cost. What it misses out is the

Chandler story of increasing the transaction costs in order to be able to reduce the production costs.

NF: **One of your contributions to theory of the firm has been to argue that it should be seen as a** *reserve*. **What do you really mean by this?**

BL: **I developed the idea with reference to Menger and Hicks' work on liquidity, which built on the idea that reserves are in general pools of resources, which you only require because you do not have sufficient information about what is going to happen in the future. Well, Coase's explanation of the firm is essentially also that the firm exists because people do not know what is going to happen in the future. Therefore, the concept of reserve should be applicable to firms.**

This also connects to capabilities. We will have to bring back the Austrians again, and specifically the Austrian idea of capital as a collection of assets, which are related together. If you simply extend that to human capital, then capabilities can be formed as a collection of such capital assets, which can deliver services, to use the Penrose term. One of the essential things about the Austrians' conception is that these capital structures are *oriented*; **they are not general purpose. Capabilities are also oriented; in fact, it seems to me that this is part of what Penrose means by "the productive opportunity" - that the firm's resources are oriented in a particularly direction. By exercising capability you find out things you did not know. What is the range of a capability is never fully known. Computers and computer production are, of course, the actually classical example. Computer manufactures have, in general, very vague ideas about to which use the machines could be put. The range of possibilities are not very well defined and can also change as one goes along; people find that it can do things they did not know it could do.**

NF: **Professor Loasby, thank you for your time and patience.**

Some selected work of Brian Loasby

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