

Geoffrey M. Hodgson and Institutional Economics

Interview by Carlos Mallorquín with Geoffrey Hodgson on 8 September 2000 at the University of Hertfordshire, UK.

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CM: I see something very interesting in your academic background given that your undergraduates studies were in mathematics and philosophy, not in economics, how did that come about?

GH: When I was at school I was very interested in mathematics. I was quite good at it and I decided to study mathematics in university. However, when I was in the university I worried about the limitations of mathematics, and the tendency towards formalization for its own sake. I became more interested in philosophy, politics and economics.

CM: Limitations in relation to social and political issues?

GH: I was always interested in social and political issues and the concentration on pure mathematics became a problem for me while I was at university. In fact I transferred to a joint degree in mathematics and philosophy. I am glad that I have studied both subjects because I find them both extremely useful for the kind of work that I do in economics.

CM: I've noticed you taught mathematics at high school level, is that an interesting experience?

GH: I shall tell you how I became a high school teacher. I was at University as an undergraduate between 1965 and 1968. As you know, in 1967-1968 there was a huge student rebellion. I was much involved politically, and at that time a Marxist. However, this involvement affected my degree results. I did not get a very good first degree. At first it seemed that I did not have much of a chance of pursuing postgraduate education, so I became a schoolteacher. This was also an obvious choice because my parents were schoolteachers. I very much enjoyed teaching mathematics to schoolchildren because I had a good understanding of the basics of the subject. But after four years I decided to try and get back into academia, to study economics, which I really wanted to do. When I was a schoolteacher I wrote a still unpublished piece of work, which is an attempt to bring some mathematical principles into Marxist economics. This was sufficient for the University of Manchester to take the risk and admit me as a graduate student, despite my first degree being a weak class. So I returned to university and studied economics for two years. I got a Masters degree in that subject. I had a variety of teachers. Some were very influential on me. I shall mention two in particular. First, there was David Laidler,¹ who is a monetarist economist. He now lives in Canada. He is a very friendly and interesting person but his analytical standpoint is very different from my own.

CM: Interesting in that he heard your themes on Marxism...

GH: Laidler is an extremely good teacher, one of the clearest and best I know. He has published very good undergraduate textbooks. His ability to communicate is excellent. I got on with him, not because I agreed with him, but because we had a common interest in economic theory. I wanted to understand how neoclassical economics worked. I also wanted

¹ Por ejemplo: *Monetarism, microfoundations and the theory of monetary policy*, University of Western Ontario, 1988; y *Taking money seriously and other essays*, Massachusetts Institute of Technology, 1990.

to get inside it and criticize it. I think that uninformed criticism is a very bad thing; you have to understand what you are trying to criticize.

CM: And the other teacher?

GH: The second influential teacher was Ian Steedman.² Politically he was close to Marxism, but an independent thinker. He had a strong reputation as a technical theorist in the area of Sraffian economics. He had made a number of major contributions to capital theory and to the theory of international trade. I was very pleased that he agreed to supervise my work on my Masters dissertation. This dissertation was on the application of Sraffa-type analysis to the labor theory of value under what is technically known as joint production: that is, production with multiple simultaneous outputs. For me at the time it was an interesting technical exercise. It had important implications. Steedman and I both came to the view that there were problems in the formalization of the labor theory of value, in part because of this particular problem of joint production. Steedman published a paper in the *Economic Journal* on this topic in 1975. He also produced a book in 1977, *Marx after Sraffa*. This had a big impact as a sharp, critical work.

CM: We are talking of the mid seventies?

GM: That's right.

CM: You hadn't yet come about on to institutionalism; you hadn't read on those issues? I've noticed that around that period, you participated at the Annual Conference of Economic Socialists. And what I am really asking were you involved politically in an organization?

GM: I was then politically active. From about 1967 until the early 1974 I was involved in a number of Marxist groups, which flourished at the time, many of which were close to Trotskyism. Some of these groups still survive, but they are now smaller. In 1974 I rejoined the Labour Party. At first I was a sort of critical Marxist, but my position changed politically after that. I became a parliamentary candidate in 1979. I stood in a Conservative constituency and I was unsuccessful. Until that time I was active in politics and also involved in theoretical debates. For instance, in 1977 I wrote a book on *Socialism and Parliamentary Democracy*. This was an attempt to argue that socialists, at least in developed democracies, had to respect parliament. This book was a critique of the more extreme politics of the late 1960s and early 1970s. Overall, up to 1979, there were these twin tracks to my activity. On the one hand I was active in the Labour party. On the other hand I was engaged in theoretical discussions in economics, using Sraffa-type analysis to try to remove the dogmatic and incorrect elements in Marxist theory, while at the same time retaining some powerful elements at the core of Marxist analysis. When I addressed Marxists, my motivation was to rid Marxism of the dogma. I have never liked a dogmatic attitude to doctrine.

CM: When was it that you started reading on Institutionalism? How did it come about? Because you had found many deficits in Marxism, politically and theoretically.

GH: In 1979 Margaret Thatcher was elected for the first time as Prime Minister. My premonition was that, despite my wishes, Thatcher might be in power for a long time. The last thing I wanted to do was to have a career as a politician in opposition. Eventually I abandoned any idea of a career in parliament. Gradually I became less active in politics and I devoted more time to my research in economics.

² Entre otros libros: *The value controversy*, London New Left Books 1981; *Fundamental issues in trade theory*, St. Martin, New York, 1979

On the academic side of my life, I became very worried by further developments within Marxism at that time, in addition to those that I have mentioned already. Jon Elster³ and John Roemer⁴ and others were beginning to formalize Marxism in terms of the rational agent models. Some people believed that this made Marxism respectable. But I was very worried. I had already had a view about the limitations of deductive and mathematical methods in general. Furthermore, I had come to the view that the rational agent model was a central defect within mainstream economics. It was strange and worrying to see Marxists embracing it.

Then another event occurred: I had the opportunity to go to America for a year. So I spent one academic year, from 1980 to 1981, teaching in Bennington College, Vermont. Edward Nell was instrumental in me getting this visiting professorship there. Nell is a sort of Marxian-Sraffian-Keynesian.

CM: He has a very good book on a critique of neoclassical economic theory.⁵

GH: He has written many books. Bennington is a small college and it has a very interesting history. It was the college where the great institutional economist Karl Polanyi wrote much of *The Great Transformation*.⁶ Polanyi died in 1964 but his collaborator Harry Pearson⁷ was there. I came to America to open my horizons. There I became more familiar with the old institutional economics. So when I returned from America in 1981, I was in a mental state of transition, where I had rejected both dogmatic and analytical Marxism. I was also looking around for an alternative way of thinking economic problems. Academically I was then quite isolated. At that time there were no networks I could turn to. I also changed my job. I moved to Newcastle Polytechnic, now called the University of Northumbria. They gave me very good working conditions, where I could do research and read more on institutionalism.

CM: I see that you had been thinking on democratic or parliamentary planning, you did a book on this...

GM: Yes. In the 1980s I sustained my earlier interests on issues of planning, worker participation, and the relationships between markets and planning. I published the book *The Democratic Economy* in 1984. I also worked on theoretical issues. In my research, the next peak event was the publication in 1988 of *Economics and Institutions: A Manifesto for a Modern Institutional Economics*.⁸ This book is an attempt to begin systematizing a modern institutional economics. It is a critique of the assumptions of neoclassical economics and an attempt at the beginnings of the development of an alternative approach. In the early 1980s my work wasn't cited very much. I was not invited to many international economic seminars or congresses. After 1988, the atmosphere began to change. For example, the *International Joseph Schumpeter Association* was formed in 1988. I was personally involved in the formation in 1988 of the *European Association for Evolutionary Political Economy*, which provided another network for evolutionary and institutional economists. Richard Nelson and Sidney Winter's book (*An Evolutionary Theory of Economic Change*) had appeared in 1982 and its impact was growing. By the end of 1980s there was an accelerating movement. There was an opening up, not within economics departments, but in other areas of academia, such as in business schools. At first this process was not particularly strong in the UK, but it has

³ *Making sense of Marx*, Cambridge University Press, 1985; entre sus múltiples libros.

⁴ John Roemer comp. *Analytical Marxism*, Cambridge University Press, 1986, entre tanto otros libros suyos.

⁵ E. Nell y Martin Hollis, *Rational Economic Man*, Cambridge University Press, 1975.

⁶ Existe traducción por FCE.

⁷ Por ejemplo: *Dahomey and the Slave Trade*, Washington, University Press, 1966; compilador et al de *Trade and Markets in the Early Empires*, The Free Press Glencoe, 1957.

⁸ University of Pennsylvania Press, 1988.

become stronger since then. I found myself to be part of a broad, international movement. Overall, from 1988 to 2000, there has been quite considerable growth in work in both institutional and evolutionary economics. But much of this is outside departments of economics. There has been a great change but there is still some distance to go. Personally, I now get overwhelmed with invitations to attend congresses and seminars.

CM: Perhaps you can draw a rough picture of past and present economics. Let me give you an example. In South America in the last fifteen years, I think we are not producing economists, we are producing good private accountants. I don't doubt that they have great techniques which are useful, but not economists, they are good at math's, at accounting, they can resolve riddles created by their algebraic equations, and econometric models, but they don't get taught the history of economic thinking, neither are they taught philosophy. Have you perceived changes?

GH: While activity in institutional and evolutionary economics has broadened, there has been an opposite, narrowing process in departments of economics. I have seen this change in the last 28 years. When I went into the economics department in the University of Manchester in 1972, it was one of the most prestigious departments of economics in the UK. And yet it was highly diverse. It included a number of Marxists, such as Pat Devine, David Purdy and Ian Steedman. It also had Keynesians and monetarists. Now things are very different, in Manchester and elsewhere.

CM: Pluralism ...?

GH: The department in Manchester was both seriously academic and pluralist. Even the monetarists were engaged with real problems. People like David Laidler were not interested in mathematical puzzles. Far from it. He wanted to understand the problem of inflation and deal with it, by trying to understand the causal mechanisms involved. That was the kind of atmosphere at the time. Many economists wanted to address real world problems. This attitude was quite widespread. In the 1970s, Cambridge was the most prestigious economics department in the UK and it was still dominated by Joan Robinson, Nicholas Kaldor, Piero Sraffa and many other people of similar mind. Also in the early 1970s, in the top international journals in economics, the proportion of mathematical articles was still much lower than it is today.

CM: Perhaps is not the mathematics *per se* that should be in question, but rather the underlying notion that there is only one type of argument that can use mathematics...

GH: Today there is lack of intellectual pluralism within most departments of economics. Economics in recent years has come to represent one approach, with one basic way of doing things. There is little discussion about its validity. In many universities you simply have to accept it or move to a different department.

CM: How did that happened, did the curricula changed, or was it the appearance of new teachers?

GH: I think is a global phenomenon. There are number of drivers in this process. But the most important thing is that after the Second World War, the USA became the hegemon of public economic discourse and the powerhouse of ideas. Postwar American economists have a technocratic approach to the subject. Everyone else tries to emulate the American way of doing things. Up to the 1970s, Britain had a very strong tradition of economics, with names such as George Shackle, Nicholas Kaldor, Joan Robinson, John Hicks and many others. Today, there are very few prominent names in this country. In contrast, America has rapidly increased its share of key economists. By the 1980s, many leading British economists felt obliged to emulate America. So they pursued such topics as general equilibrium theory and

the macrofoundations of macroeconomic theories. Now game theory is everywhere in vogue. The pressure has been all in one direction and the subject has become less pluralistic. There was also a growing obsession with the use of mathematical techniques.

In recent years the British ‘Research Assessment Exercise’ accelerated the process even further. Universities in Britain receive much of their money from the state. A major part of this money is allocated on the basis of perceived research strength. Periodically, departments are judged on their research quality, and money is allocated according to the strength of these departments as perceived by the reviewing panel. In the 1980s and 1990s this process became increasingly important. In economics, the crucial control of the assessment process is in the hands of *Royal Economic Society* – the professional association of economists in the UK – which by the 1980s was totally dominated by neoclassical economics. All the signals to economic departments were that you could only succeed and get a higher research score by using neoclassical techniques. To be ranked highly you had to have top game theorists or econometricians. With the next round of research assessment the screw came with double force, because people perceived and acted upon these signals, producing a vicious circle. This forced an increasing narrowness of perspective. This process has been catastrophically visible in the last ten years. Today, economics in Britain is even narrower than it is in the United States. It is especially narrow when compared to Latin America and some other countries. As I have mentioned already, most of the work in institutional economics in Britain and elsewhere has been done outside economics departments, that is in business schools and so on.

CM: I think Institutionalism in America was defeated politically, many of its ideas haven’t been destroyed theoretically, many can’t be defended, but in general its flaws are not many.

GH: The American Institutionalists were very strong in the 1920s and 1930s. But by the 1930s they began to disintegrate and to fracture theoretically. The intellectual climate in America changed very rapidly. People were no longer sympathetic to the foundational presuppositions of institutional economics. Institutional economics was originally built on instinct psychology, evolutionary principles and pragmatist philosophy. But all these things became very unfashionable in America in the 1920s. Although institutionalism in the 1930s was still strong as a movement, it had lost its theoretical integrity. Its fracturing and its uncertainty can be seen in the writings of John Commons, who was then trying to systematise institutionalism. Wesley Mitchell was another major and highly influential figure, based in Columbia. But his influence was more to do with national income accounting and business cycle theory. He didn’t really address the underlying theoretical issues. When those two figures died in the 1940s, the leadership of institutionalism passed on to Clarence Ayres. He took a quite different direction. He was interested in the role technology as the primary driver of economic change. This of course is a powerful idea, but he saw institutions as negative constraints. This was a different theoretical turn. Institutionalism lost its way theoretically. It was partly defeated in theoretical grounds.

In terms of policy influence, institutionalism suffered from the Cold War. In the 1930s institutionalists were very strong in supporting Roosevelt’s New Deal. Institutionalists also had being very active in pursuing limited planning, price regulation, institutional change in the banking system, and so on. Institutionalists were very much involved in advising the government, and their contribution in this respect has been underestimated. However, with the onset of the Cold War and the McCarthy period, it was very difficult for American institutionalists to operate in such an oppressive ideological climate. These events contributed to their marginalization.

CM: I have noticed that you are writing a new book and you touch the main writers since the nineteenth century, but in one of the chapters you discuss Talcott Parsons. I think he is an interesting figure, because for some reason he makes a detour. His teacher's were Hamilton and Ayres and yet, to use his metaphor, he crossed the Atlantic (Weber, Durkheim, Pareto, Marshall) he didn't want to discuss his notions through the institutionalists. He barely cites Veblen. I believe that he was thinking of his future career, and that his ideas were not going to fall in the right place if he debated with Institutionalists. What you think on this issue?

GH: Yes. I have just completed the draft of a book called *How Economics Forgot History*. It discusses the demise of the historical school and the rise of ahistorical economics. In it there is also a chapter on the role of Talcott Parsons in establishing an ahistorical sociology. Parsons is a very interesting case. It is not well known that he was brought up as an institutionalist by Walton Hamilton and Clarence Ayres at Amherst College in Massachusetts. He then went to the London School Economics and then took a Doctorate in Heidelberg in Germany. Fluent in German, he read Sombart, Marx and Weber. He brought back the ideas of Weber to the USA and to the English speaking community. I agree with your suggestion that there is a strategic calculation in his career. He returned to Harvard in 1927, not to the sociology department – because there wasn't one there until later – but as an instructor in the department of economics. Harvard was then a staunchly neoclassical department. Harvard was the main focus of opposition to institutionalist departments such as Columbia, Wisconsin and Berkeley. Even Chicago had Frank Knight, who often described himself as an institutionalist. Many leading universities, with the exception of Harvard, had strong institutionalist elements. So Parsons had to make his way in this circumstance. Also the role of Schumpeter is interesting. After visiting Harvard, he then became full professor there. Schumpeter was also very critically of institutionalism, at least when he was in the US. But previously in Germany he had expressed some sympathy for the ideas of the historical school. So suddenly Schumpeter also seemed to take a strategic career decision, to be more hostile to institutionalism.

CM: You have mentioned the two persons that have made the two of the most unfortunate jokes in the social sciences. Knight and Schumpeter in reference to Parsons, *The Structure of Social Action*, said: “there is only one law in sociology. Bad talk drives out good talk”.⁹ While Schumpeter talked of it “understandable only if translated into German”.¹⁰ Schumpeter, who was perhaps the only person was in any condition to understand him, pushed him away, as also did Knight, but I am talking about the young Parsons, in *The Structure of Social Action... a tour de force*

GH: Parsons applied for some research support, and Schumpeter wrote a favourably testimonial for *The Structure of Social Action*. In an article in 1935 in *Quarterly Journal of Economics*, Parsons published criticisms of Veblen and institutionalism that in fact are very weak. Parsons didn't like the instinct psychology of James and others, whereas Veblen has embraced it. Parsons wanted to separate sociology from psychology. One of his major marks on modern sociology has been the separation of sociology from any psychological grounding. I think personally that this is a retrograde step. Parsons's attack on institutionalism was the means, not only to draw a line under his own history his own past and his institutionalist education, but also to accelerate the process by which sociology would separate from psychology. After that he hardly mentions institutionalism ever again.

⁹ Knight, citado por Richard Swedberg, *Economics and Sociology, Redefining their Boundaries: Conversations with Economists and Sociologists*, Princeton University Press, 1990, p. 15 Es obvia la referencia a Gresham.

¹⁰ Ibid., nota a pie de página no., 8.

CM: Lately you have been retrieving the German Historical School, and it's interesting because at the moment you are trying to conceptualise time space and space with out having a general theory...

GH: The German historical school made an enormous contribution. Gustav Schmoller, Werner Sombart and Max Weber were part of this huge movement, lasting from the 1840s to the Second World War. These people wrote extremely important analytical as well as historical works. The idea that they are purely descriptive and atheoretical is just a myth, perpetuated by ignorant people who don't know anything about them.

CM: Given the dominance of neoclassical economics, in policy and ideas, there doesn't seem to be any space to talk about the growth poverty all over the world, and that perhaps something is going wrong, surely there is something wrong...

GH: My personal view is that poverty and inequality are two of the most important problems facing humanity. It is to the enormous shame of governments that these issues are not very high in the present agenda. Furthermore, economists – with some notable exceptions – are not much bothered with these issues. Having said that, the ideological atmosphere has changed a little in the last ten years, and economists have become more open in policy terms. From the point of view of the policy debate, the 1980s were a very bad period because of the ideological domination of neo-liberal economics. Particularly since the end of the Cold War in 1989, the policy debate is much more open. Today, even among neoclassical economists, you can find quite a variety of ideological views. For example Joseph Stiglitz has been critical of the World Bank policy. Because of the end of the Cold War, the West no longer has to be so defensive and protective of free market economic ideas. So on the positive side we have a greater ideological pluralism. But on the negative side, the issues of poverty and inequality are still not yet high on the agenda.

CM: Without jumping towards utopia, I think we have to build utopias ...

GH: I discussed this recently in my book *Economics and Utopia*,¹¹ I used the end of the century as a touchstone to discuss two great utopias. One of these is socialism. Socialism was a utopia of the twentieth century. Many have declared its death, but it is still a living idea. The other great utopia is the libertarian idea of a free, unfettered free market. To a large extent, both these utopias have failed.

I am in favour of utopian thinking in the sense that we have to imagine futures. As social scientists we are obliged to consider alternatives. Any consideration of any policy issue involves utopian thinking. But I am against utopia thinking in the rigid sense of a rigid blueprint, which is the classic utopian approach. I believe in evotopia – an evolving vision. My utopianism is based on the belief that we have to think of our future without poverty, with greater equality, without environmental catastrophe.

CM: We have to construct new institutions ...

GH: Of course ...

CM: Current international institutions were for a period of Cold War, there are no excuses for their actual existence.

GH: The building of new global institutions involves great problems. Yet it is one of the great issues on the agenda for the twenty-first century. Clearly, national governments cannot deal on their own with the major global problems. We need extended collaboration and/or supranational institutions. Many such institutions, such as the World Bank and the IMF, have

¹¹ Routledge, London, New York, 1999.

failed in the past. But some global institutions are crucial, in order to create appropriate structures, to help prevent disasters and to deal with pressing problems.