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Interview

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'It is not enough to do training in highly demanding mathematical economic modelling or econometrics to become a good economist'

Interview with Harald Hagemann

Harald Hagemann, born 1947, is a Professor emeritus of Economic Theory at the University of Hohenheim, Stuttgart, Germany. He received his diploma and doctoral degrees from the University of Kiel, and he formerly taught at the University of Kiel, the Free University of Berlin and the University of Bremen. He was also a Visiting Professor at several international universities. He is a Life Member of Clare Hall, University of Cambridge, and in 1999/2000, he was the Theodor Heuss Professor at the Graduate Faculty of Political and Social Sciences of the New School for Social Research, New York. He was the President of the European Society for the History of Economic Thought from 2010 to 2012 and the Chairman of the German Keynes Society from 2013 to 2019, and he has been on the editorial boards of several journals. His research has focused on the theory of capital, structural change and employment, and the history of economic thought, and here in particular on German-speaking émigré economists during the Nazi period. He received a festschrift in 2012 (Krämer et al. 2012).

How did you start studying economics? What was the motivation? How did you get in touch with heterodox economics, and what were the first steps in your academic career?

I think from the very beginning I had also a political motivation in studying economics. The main reason was that the Great Depression with more than six million unemployed people was one of the decisive causes why the Nazis came to power in Germany. There were some other reasons, but the causes of this high unemployment in 1931/1932 and the political consequences stimulated my interest.

I was lucky that I studied in Kiel, my hometown, where economics at that time was one of the leading departments in Germany with a high international orientation. Germany was a latecomer in Keynesian economic policy, which basically started with Karl Schiller, when the Social Democratic Party entered the government in December 1966 at the background of a recession. But in economic theory at universities, it was already in the early 1950s that Erich Schneider introduced Keynesianism in his textbooks

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(Schneider 1947-1952), which dominated for about two decades in Germany. Schneider's Keynesianism was more of the neoclassical synthesis type – like Hicks and Samuelson – but with a strong Scandinavian flavour. Schneider was a leading figure not only in teaching economics at the University of Kiel but also as long-time chairman of the Theoretical Committee of the Verein für Socialpolitik (German Economic Association) and as director of the Kiel Institute of World Economics in the 1960s. Jürgen Kromphardt, who in 2003 took the initiative to found the German Keynes Society, did his PhD with Schneider in 1957.

Schneider retired in 1968/1969, and he had two successors. As director of the Kiel Institute, Herbert Giersch came, who was the leading figure of the neoclassical orientation in German economic policy. But Giersch got a chair for economic *policy*, and not, as Schneider had it, for economic *theory*. The successor of Erich Schneider in economic theory was Albert Jeck, who came from Munich, where he was a student of Erich Preiser, who was open-minded to Keynesianism and had a strong interest in issues of income and wealth distribution.

Together with Heinz Kurz, who came with Jeck from Munich, I became an assistant and doctoral student of Albert. That was the beginning of my academic career. Among the greatest debates at the time were the Cambridge–Cambridge controversies in the theory of capital. Capital theory became the central topic of the PhD theses of Heinz and me. Heinz graduated in 1975 on Sraffa (Kurz 1977), and I dealt with the Solow–Pasinetti controversy on the rate of return on capital and graduated in 1977 (Hagemann 1977). Therefore, Pasinetti from the very beginning had a strong influence on my work.

Already in Kiel, particularly Heinz and I developed closer contacts to Cambridge, UK. In 1977, we had Luigi Pasinetti and Joan Robinson for about ten days in Kiel. Pasinetti gave a series of outstanding lectures, and around Joan Robinson's visit, we organised a research workshop that was attended by many other economists from outside, such as Bertram Schefold. Nicholas Kaldor's story is a little bit more complex, but he came to Kiel a year later (Hagemann 2012a). In October 1987, I also contributed to the famous Kaldor memorial conference in New York (Hagemann 1991).

What role did German economists play in shaping your approach to capital accumulation and technical progress?

Apart from the Kaldor–Pasinetti work on income distribution in the 1950s and 1960s, I got also interested – including an element of local patriotism – in the work by Adolph Lowe, Hans Neisser and Gerhard Colm, who were in Kiel between 1926 and 1933, as well as Jacob Marschak and Wassily Leontief, who also were there for some years. But all of them were Jews and social democrats, and therefore none of them could stay after the Nazis rose to power (Hagemann 2021). They had very advanced ideas with one main difference compared to Keynes and normal Keynesianism, which influenced my work, too, namely, the two topics of capital accumulation and technical progress that are strongly interrelated. This is a German tradition dating back to Karl Marx, but it was also important for Joseph Schumpeter's research programme, who emphasised the influence of technical progress on the long-run wavelike movement of capitalist economies (Hagemann 2015; Dal Pont Legrand and Hagemann 2017). These elements were always there in my work, in particular the employment consequences of technological change, starting with Ricardo's machinery problem (Hagemann 2008). It is also linked with my work on the emigration of German-speaking economists. This led me to two international places, to Cambridge, UK, because of the Keynes, Sraffa, Kaldor and Pasinetti tradition, and to the New School for Social Research in New York, where the *University in Exile*, the later

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Graduate Faculty, was founded in summer 1933, as a host for German and European émigré economists and other social scientists.

You have built up a specific relationship with Adolph Lowe. How would you briefly explain his contribution? What is special in Lowe's work?

Overall, Lowe's work is integrated in the social sciences so that Kenneth Boulding (1965), when he reviewed Lowe's (1965) opus magnum *On Economic Knowledge*, was calling him an 'economic philosopher'. Here I have to focus on economics. Lowe became the founding director of the new department for research on business cycles in April 1926 at the Kiel Institute. At that time all over in Europe, research institutes on business cycles were founded according to the Harvard model of 1917 and the National Bureau of Economic Research, founded in 1920 with Wesley Mitchell as director in the first decade. Lowe's habilitation thesis was a long essay, arousing interest again in the modern debates when equilibrium business cycle theories came up by Lucas and others in the 1970s, because Lowe challenged the idea that you could analyse cyclical fluctuations, an essentially dynamic phenomenon, on the basis of the dominant equilibrium approach in economics. This was the central question in his habilitation essay: 'How is business cycle theory possible at all?' (Lowe 1926 [1997]).

After your PhD in Kiel, and a year at the Free University of Berlin 1980/1981, you moved on to Bremen in January 1982 to become a professor in the economics department and to continue your collaboration with Heinz Kurz, and then also with Peter Kalmbach and a couple of other people. What were the main achievements, and why did you leave Bremen in 1988?

In Bremen, we had a research group for post-Keynesian economic theory and policy with a discussion paper series and frequent visiting lecturers. There were five members, besides me: Heinz Kurz, Peter Kalmbach, Otto Steiger and Gerhard Leithäuser. Peter had written an excellent thesis on 'Growth and distribution in a neoclassical and post-Keynesian perspective' (Kalmbach 1972). Otto was the last economist at a Swedish university (Uppsala) who wrote his PhD thesis on the Stockholm School in German in 1970. Otto had very good contacts to Bertil Ohlin and to the widow of Erik Lindahl. In 1986, we organised an international conference on 'Keynes' General Theory after 50 years'. Gerhard Leithäuser had closer contacts to the French Regulation School, from where Robert Boyer and Pascal Petit came to Bremen, and we had discussions with them.

We were working hard in the research group with the best PhD students and mature diploma students through books like Luigi Pasinetti's (1981) *Structural Change and Economic Growth* or Victoria Chick's (1983) *Macroeconomics after Keynes*. We had many foreign visitors to Bremen, like Sidney Weintraub, Hyman Minsky, Sheila Dow, Philip Arestis, Marc Lavoie, Axel Leijonhufvud, Augusto Graziani and several others, all of them leading international scholars in a Keynesian or post-Keynesian direction. The contacts to Cambridge, which Heinz Kurz and I already established in the 1970s in Kiel, were enforced in our Bremen period. Amongst several others, such as Sergio Parrinello, Ian Steedman or Neri Salvadori, we had personal contacts with Piero Garegnani, who passed forward the role of editing Sraffa's papers to Heinz, and whose 1958 Cambridge PhD thesis 'A Problem in the Theory of Distribution from Ricardo to Wicksell', together with his other main publications, we recently published for the first time in the Springer Studies in the History of Economic Thought (Garegnani 2025), of which I am a managing editor. In the Bremen period, we enormously intensified our closer cooperation with the New School for Social Research in New York, with the exchange of professors and students.

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I think the two of you (Eckhard Hein and Hagen Krämer) went to New York in the second year 1988/1989 of the student exchange program that as a Dean I had negotiated with the New School and the German Academic Exchange Service (DAAD).

Yes, that is true, and we both benefitted from this enormously in our academic careers.

Two years before the student exchange started, we had already managed an exchange of professors for one semester. In 1985, Heinz Kurz, with Dieter Kattermann as a PhD student, moved to the New School in Spring, and Ed Nell came in the summer semester with Ray Majewski to Bremen. In the following year, I was a Fulbright Visiting Professor at the New School in the spring semester 1986, and Christof Ruehl as a PhD student went with me. In the summer semester, Willi Semmler came to Bremen.

Your cooperation with the New School was meant to be institutionalised in research with the foundation of an Adolph-Lowe-Institute, as a centre for post-Keynesian economics in Germany. Why did this fail? With hindsight, did you miss a big chance, and did you not try hard enough?

No, we tried very hard. It was not our fault that it was not institutionalised, because we had a very good initial memorandum for the foundation of the Institute. It was all planned in details with four directors, two on the New York side, Ed Nell and Robert Heilbroner at that time, and two in Bremen, Heinz Kurz and me, but there were basically two main reasons why it did not materialise. One was the financial problem. It was a main problem in Bremen, which was almost bankrupt due to the recession after the second oil price shock. The New School in New York always had financial difficulties. The second reason was a political one. Ronald Reagan was President in the United States at that time, so there was no chance to get political backing. This was a greater problem on the American side than on the German side. But financially, it was a main problem on both sides.

Bremen is one of the examples of the failure of a more or less fully heterodox economics department in Germany. You left for Stuttgart in spring 1988, Heinz Kurz left for Graz slightly before you, others remained, and when they retired, nobody was replaced. What in your view was the main cause of this failure? Why is it that Bremen was lost as a place for heterodox economics more generally, not only for post-Keynesian economics?

I must say it was not my original intention to leave Bremen, but there were several events. One main problem came up after Otto Roloff, who had the chair for public finance, left for Wuppertal. We had a major controversy thereafter, and this revealed basically three problems. First, there was no backing on the political side from the Bremen Senate for the kind of economics we were doing. Second, there was the dominance of more right-wing people in management studies. There were two or three colleagues in management who were quite okay, but others were second or third class, and started intrigues to shift the professorship to Marketing. They were not successful with this intention, but the professorship was cancelled. This was a great pity because we could have appointed one of the leading colleagues in public finance at that time. Third, the economics department was too heterodox in its composition. In particular, we had some more mediocre people in economics, who did not meet higher standards. So, you would have had to invest some money for new appointments to make it an international centre. But these funds were not available in Bremen, for political and public finance reasons. Overall, it was a pity, because we had international contacts, not only to Cambridge and the New School but also to Italy, France and many other countries.

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What was your main research while you were based in Bremen?

Our main research, also in the post-Keynesian group, was on structural change and economic growth, particularly the analysis of the employment consequences of new technologies. What today is the digital revolution, at that time it were the industrial robots, which started to be introduced in the mid-1970s in the automobile industry and elsewhere. We had the funding of 600,000 German marks from the Volkswagen Foundation, which was a lot of money at the time, in particular for Bremen, for doing research work on the employment consequences of new technologies. We also had excellent contacts with the Leontief group in New York that did the main study *The Future Impact of Automation on Workers* (Leontief and Duchin 1986), based on an innovative dynamic input–output model, for the US economy. So, this was my main topic not only in the 1980s but also in the post-Keynesian group, on top of the work on Keynesian economics more generally.

Furthermore, there was another kind of traverse from Kiel to Bremen. In my last two years in Kiel, I was involved in a project on Keynesian economic policies in Sweden, financed by the German Research Foundation (DFG). Sweden was then considered a kind of prototype for Keynesian policies, but the Swedes could rightly say they had their own tradition of Wicksell, Ohlin, Lundberg and many others for this kind of policy. This research interest brought me together with Hans-Michael Trautwein, who at that time was an external PhD student. He did his work on the Employee Funds in Sweden and on Swedish economic policies, and I became the second supervisor of his dissertation. It resulted in a long-run collaboration and friendship, as expressed in the actual Festschrift *Waving the Swedish Flag in Economics* (Dal Pont Legrand and Hagemann 2025).

Then you moved on to Hohenheim in 1988 and there your research on emigration of German economists started?

Yes, this basically started just after I left Bremen, because between 1989 and 1994, together with Claus-Dieter Krohn, we had financial support from the DFG for travelling to the United States to do archive work. This project on the emigration of German-speaking economists in the Nazi period hence turned to one of my main research topics in the 1990s and in the 2000s (Hagemann and Krohn 1999; Hagemann 2007; 2011). The main topics of the émigrés I researched were on technological change and employment. Besides Lowe (1976), there was also Neisser, who published a leading article on 'permanent' technological unemployment in the *American Economic Review* (Neisser 1942). In fact, the research work on capital accumulation, technical progress and the employment consequences of new technologies was a main reason why I came to discover the work of the German-speaking émigré economists. So these two strands of my research are closely linked.

But we assume that you also continued your international networking in that period.

Yes, from the early 1990s to the mid-2000s, I was also one of the managing editors of the new journal *Structural Change and Economic Dynamics*. Furthermore, from 1995 to 1997, we were financed by the European Union for a PhD seminar on structural change and economic dynamics that took place in Cambridge, where I closely cooperated with Michael Landesmann, Albert Steenge from Enschede, and Faye Duchin who was running the Leontief Institute for Economic Analysis in New York. There was also Roberto Scazzieri from Bologna, where I spent three months in 1996 as an Erasmus visiting professor. Luigi Pasinetti and Richard Arena from Nice were regular attendants. Some of the

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participating colleagues were also key persons in the so-called 'Wealth of Nations Study Group'. There were Mauro Baranzini, Ferdinando Meacci, Pier-Luigi Porta and Roberto Scazzieri, who also had very good relations with Pasinetti. One outcome of these collaborations is the three-volume collection *The Economics of Structural Change* (Hagemann et al. 2003).

Earlier, in 1989/1990, I had spent a sabbatical in Cambridge, when I was a visiting professor at the Department of Economics and at Clare Hall. In that time, I intensified my cooperation and friendship with many of the more Keynesian or post-Keynesian economists in the UK, like Philip Arestis, Vicky Chick, Malcolm Sawyer and Geoff Harcourt, with whom and his wife Joan I already had visited the Berlin Wall in winter 1980/1981 when I was substituting the professorship for political economics and economic theory at the Free University of Berlin. As is well known, Geoff was the 'champion of the social room' in Cambridge. He was the only one who knew everybody and who introduced you to James Meade, Frank Hahn and many others. When I arrived in Cambridge in September 1989, he introduced me to Keynes's niece, Polly Hill, and Alfred Marshall's grand-niece, Philomena Guillebaud, within a week. Geoff was a kind of spider in the web, connecting people. He surely was quite important for the networking of heterodox economists.

Avi Cohen from Toronto was our direct neighbour with his family at Clare Hall. We became close friends, and together with Geoff and Omar Hamouda, we went by car to the Hicks memorial in Oxford in October 1989 (Hagemann 1994; 2018), and shortly afterwards with Geoff and Philomena Guillebaud to the ninth Keynes seminar 'Keynes as Philosopher-economist' held at the University of Kent at Canterbury, organised so well by Tony Thirlwall at Keynes College for many years. I also spent a whole day with the wonderful Hans Singer at the Institute of Development Studies in Brighton. When I organised an international conference on the emigration of economists in the Nazi period at Hohenheim University in 1991, Singer gave a highly stimulating opening keynote on the influence of Keynes and Schumpeter on his work as a development economist.

Let us come back to your research journey. You started with capital theory and then went on to structural change and unemployment, which was then also the link with your research on German-speaking émigré economists. Today, your main research area is the history of economic thought, right?

Not fully. Of course, the impression is not wrong, but I would not go as far as Frank Hahn, for example. He visited the pre-conference of the foundation of the European Society for the History of Economic Thought, organised by Carlo Zappia in Siena in September 1995. I had the chair in the first session where Frank Hahn started his talk on the history of general equilibrium theory with the sentence, 'Now that I'm getting old, I'm doing history of economic thought', which is a kind of denigration of history of economic thought. Although I must confess, when you look at my publications, the number of equations has decreased over time. My papers are less technical than in the early years when I was dealing with not only capital theory but also structural change or traverse analysis (Hagemann 1990; 1992) where you have many equations, otherwise you cannot deal with these topics. But I have always had an open-minded relationship with the use of mathematics in economics. It depends on the topics you are analysing, and there are some topics which you cannot analyse without using mathematics.

If you look at my work in the history of economic thought, then you will recognise that a great part of it is linked to problems that also have an actual relevance, such as the employment consequences of new technologies (Hagemann 2012b) or issues of growth,

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structural change and cyclical fluctuations. Unfortunately, the problem of refugee scholarship is back on the stage again in many countries. With Will Milberg, the long-time Dean of the New School, in 2017, I co-edited a special issue of *Social Research*, to which I contributed an essay on Franco Modigliani, the 1985 Nobel Laureate, who got his PhD from the New School in 1944, with Marschak and Lowe as his supervisors (Hagemann 2017).

Let us come to the final questions: What, in your view, are the current main challenges we are facing as (heterodox) economists? And what are your recommendations for the younger generation of economists?

Coming to your first question, I think there is a great geopolitical uncertainty at the moment, not only trade wars, or issues related to the Trump administration. Where this will end, I think, is too difficult to predict now. More than 15 years ago, 2007–2009, we had the Global Financial Crisis and the Great Recession. Such a financial crisis, I am relatively sure, will come back one day, hopefully later than sooner and less shocking. So to know a bit more in some details about the main reasons of the financial crisis is always a good point for students to study. And I remember that in the years 2008 to 2012–2013, there was a strong demand by students, when they had to opt for their thesis for the diploma, bachelor or master degree, to compare the Great Recession with the Great Depression. What are the differences, what are the similarities in these crises? So studying past crises can be very important. Not only the geopolitical question today indicates that it is not enough to do training in highly demanding mathematical economic modelling or econometrics to become a good economist. I always liked the quotation from Keynes in his obituary essay on Alfred Marshall, where he argues 'that the master-economist must possess a rare combination of gifts. He must reach a high standard in several different directions and must combine talents not often found together. He must be mathematician, historian, statesman, philosopher – in some degree. … He must study the present in the light of the past for the purpose of the future' (Keynes 1924 [1972]: 173–174).

Thank you very much, Harald, for this interesting interview.

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