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Economic theory in an imperfect world: Frank Hahn, general equilibrium, and Keynesian economics

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Abstract

Frank Hahn was both a Keynesian economist, active in policy debates, and an economic theorist. Placing his work in the context of attempts to make theoretical sense of Keynesian economics, this paper explores the way Hahn used abstract general equilibrium theory to draw conclusions relevant to policy in an ‘imperfect’ world that does not conform to the assumptions made in the theory. Hahn’s rigorous approach to theorizing as the route to understanding is assessed in relation to the different approaches of Robert Lucas, Milton Friedman, Paul Samuelson and Axel Leijonhufvud. (90 words)

‘Keynes had a poet's intuition and a practical man's grasp but he did not know how to theorise rigorously.’ (Frank H. Hahn)

1. Introduction

The writer of Frank Hahn’s obituary in the *Daily Telegraph* began:

Professor Frank Hahn, who has died aged 87, led the revival of Cambridge University as a centre of excellence in economics and was the author, with Kenneth Arrow, of a classic book on equilibrium theory. ... Hahn was better known to the wider public, however, as the co-instigator of a famous letter to The Times, signed by 364 of Britain’s most eminent economists in 1981, warning Margaret Thatcher’s government

that its economic policies would deepen the depression, erode the industrial base and undermine Britain's social and political stability.¹

The background to this letter, drafted by Hahn together with his Cambridge colleague, Robert Neild, was an article they had written for *The Times* in the previous year.² In that article, they had challenged Milton Friedman's claim that, in the long run, the economy would revert to a natural rate of unemployment, defined as 'the level that would be ground out by the Walrasian system of general equilibrium equations, provided there is embedded in them the actual structural characteristics of the labour and commodity markets, including market imperfections' (Friedman 1968, quoted in Neild 2012). Hahn and Neild challenged this claim about economic theory.

a. In the previous decade mathematical economists had shown that, even if there were no market imperfections, the Walrasian equations could not be expected to produce a general equilibrium with full employment.

b. The idea that they would do so in the presence of market imperfections, which abound in reality, was even more far fetched (Neild 2012, summarizing Hahn and Neild 1980).

In the letter they applied this conclusion to the government's decision, in the 1981 budget, to impose tight fiscal policy in an attempt to reduce the growth rate of the money supply, even though unemployment was high and rising: 'There is no basis in economic theory or supporting evidence for the government's belief that by deflating demand they will bring inflation under control and thereby introduce an automatic recovery in output and

¹ <http://www.telegraph.co.uk/news/obituaries/9960411/Professor-Frank-Hahn.html> (accessed 17 January 2020).

² Hahn and Neild (1980).

employment; present policies will deepen the depression'. As their earlier article had made clear, 'economic theory' referred to general equilibrium theory to which Hahn was a significant contributor. According to Hahn and Nield, the claims that Friedman had made on the basis of Walrasian general equilibrium theory did not follow from the theory. Thus, although the letter did make claims about the consequences of the Conservative government's economic policies—that they would 'deepen the depression, erode the industrial base of our economy and threaten its social and political stability'—its central argument was an argument about rigorous, abstract economic theory of a type that critics have claimed to be irrelevant to the analysis of real-world problems.³

Hahn's work, therefore, raises the issue of the relationship between highly abstract, rigorous and typically mathematical economic theory and applied, policy-oriented economics. This is not because he used theory to derive direct policy conclusions—he was an economic theorist who did not engage in applied work—but because he used economic theory to challenge arguments about economic policy that other economists made. The reason why it is difficult to use theory to analyse policy is that the construction of models that can be analysed rigorously requires that the theorist make assumptions that ignore important features of the real world. In short, the theorist is confronted with what, to a Walrasian general equilibrium theorist, is an imperfect world—a world in which not only is competition imperfect but information is limited and asymmetrically distributed, economic agents are not

³ For the present argument, the accuracy of the letter's predictions about the British economy is irrelevant. However, given the widespread belief that signatories to the letter, which included me, were proved wrong, I will merely note that a strong case can be made that the letter's predictions unfortunately proved to be correct.

completely rational, not all goods have market prices and many other imperfections.⁴ Hahn sought to link the two very closely in a way that other economic theorists did not. For example, Paul Samuelson, as eminent an economic theorist as Hahn, was content with a much looser connection between his formal, mathematical theorizing and his macroeconomic policy conclusions (see Backhouse 2017).

2. Keynesian economics and General equilibrium

The General Theory of Employment, Interest and Money, the book in which John Maynard Keynes proposed the theory that, he believed, would revolutionize the way the world thought about economic problems was published in 1936. This was in the middle of the decade in which there was a revival of interest in the general equilibrium theory proposed by Léon Walras⁵. Keynes, a student of Walras's contemporary, Alfred Marshall, did not share this enthusiasm for Walras and argued in a way that was foreign to the rising generation of mathematical economists. Trained as a mathematician, he argued in terms of functions and their properties and, despite being concerned with the properties of the economic system as a whole, he did not construct what a modern economic theorist would recognize as a general equilibrium theory.⁶ Like Marshall, Keynes used mathematics to support arguments that could not be fully captured in the mathematics because reality was more complex.

⁴ This way of putting the problem should not be taken as implying that the problems that Keynes sought to analyse could be solved by removing these 'imperfections.' Hahn, like Arrow (discussed below) provided arguments about why such departures from the competitive ideal were inevitable and could never be eliminated. He was adamant that a 'perfect' world, though it might be a useful benchmark, could never describe the world in which we live.

⁵ Backhouse and Medema, 2014.

⁶ This argument made in this paragraph is developed in detail in Backhouse 2010.

Frank Hahn regarded Keynes as one of his heroes on account of his insights into how the economic system worked. However, he also argued that Keynes did not know how to theorize rigorously, hence his claim that Keynes had a poet's intuition. In saying this, Hahn was referring to the gap between what Keynes was trying to say and what his mathematics allowed him to say. For example, Keynes analysed investment in terms of a schedule in which the marginal return on investment was a declining function of the rate of interest, even though it was important for his argument that investment depended on expectations of the future that could not be quantified. The *General Theory* contained all the elements of what later became known as the IS-LM model, but he never wrote down such a model, leaving the door open for interpreters to do so (see Phelps-Brown 1937). This omission was probably deliberate because such a model would not capture what he considered the crucial parts of his argument, notably the fact that we have little information about the future.

In contrast, for the rising generation of mathematical economists, rigorous theorising meant writing down systems of equations that corresponded to the assumptions one was prepared to make, and drawing only those conclusions that could be deduced from those equations. In order to make sense of how the different markets in Keynes's theory fitted together, interpreters typically constructed systems of two or three simultaneous equations (equality of supply and demand for goods, money and possibly bonds) and an inequality (supply of labour greater than demand). Such modelling had no necessary connection with Walrasian general

equilibrium theory but, given the growing interest in Walras and general equilibrium theory, it was probably inevitable that economists would, sooner or later, use it as the framework within which to make sense of Keynesian economics. John Hicks did not focus on general equilibrium theory in his highly influential 'Mr. Keynes and the "classics"' (1937) but, two years later, in *Value and Capital* (Hicks 1939), he did.

In an obituary, Hahn paid tribute to what he had learned from Hicks, implicitly revealing much about his own attitude towards economic theorizing. He linked Hicks to a pragmatic British tradition in economics that accepted that 'economic' variables could not explain everything and using the minimum of mathematics needed to establish one's points. The main point where Hicks parted company with that tradition was in being less interested in improving the human condition than in explaining economic phenomena. 'Reading Hicks,' Hahn wrote, 'always renews one's faith in the importance of economic theory as a means of understanding (and not necessarily of prediction)' (Hahn 1990a, p. 540). A belief that the function of economic theory was understanding was a view Hahn held very strongly.

Hahn praised Hicks's attempt to approach 'the Walrasian enterprise ... from the "bottom up", that is, from a well articulated micro-theory' but immediately went on to claim that 'Perhaps the most lasting and important part [of Hicks's *Value and Capital* (1939)] is sequence analysis and the accompanying discussion of expectations' (Hahn 1990a, p. 541). The reason why this was good economics was that such an approach made it possible to what he called 'an economics of mistakes

and their correction' (ibid.) The modern literature on learning and expectations would come to be seen as developing from Hicks's sequence analysis, for 'He knew all about the need for such work and indeed posed the right questions' (Hahn 1990a, p. 542). Whilst these remarks were no doubt made with hindsight, it is plausible to see Hahn as being indebted to Hicks for prompting him to think deeply about these issues. Twenty five years earlier, Hahn had praised Hicks's work on methods of economic dynamics in which expectations and the future entered in what he called 'an essential way' (Hahn 1966, p. 84). And in his obituary of Hicks he testified to how exciting it had been to encounter Hicks's discussions of consumer theory and stability in the 1940s (Hahn 1990a, pp. 540-1). Hicks was neither the first nor the last to adopt period analysis but his work was nonetheless highly significant in that temporary equilibrium in the context of a dynamic, period analysis could provide a theoretical rationale for models such as IS-LM.

Franco Modigliani (1944), in a much-cited article, explicitly used Walrasian general equilibrium as the framework for evaluating Keynesian ideas. 'The only rigorous procedure,' he wrote, 'is to set up a complete "Walrasian" system and to determine the equilibrium prices and quantities of each good' (Modigliani 1944, p. 46). In principle this should, as in Walras's theory, or Hicks's *Value and Capital*, contain prices for every good traded, but because this was too 'cumbersome,' he worked with a 'reduced' system: with a miniature Walrasian general equilibrium system. Oskar Lange (1944) went even further in using Walrasian general equilibrium theory to frame debates over Keynesian economics. However, unlike

Hicks, neither Modigliani nor Lange tried to ground their theory in the period analysis.

This provided a seemingly rigorous way to think about Keynesian economics, but it was still not satisfactory. Money played a crucial role in Keynesian theory but money was never thoroughly integrated into general equilibrium theory. Don Patinkin, who studied with Lange in Chicago, managed to solve some technical problems with the quantity theory of money and, in *Money, Interest and Prices* (1956, 1965), formulated a general equilibrium theory in which money was treated as a special good. His work helped to cement the idea that Keynesian economics was concerned with disequilibrium: Keynesian problems arose when equilibrating forces did not remove unemployment within a socially acceptable period of time.

Although Walrasian general equilibrium theory had become central to debates over Keynesian economics, the way in which this was achieved raised fundamental problems. Patinkin assumed perfectly competitive markets. Perhaps as serious, he linked Keynesian economics to the weakest part of general equilibrium theory—the analysis of stability. *Tâtonnement* processes (in which no transactions took place until equilibrium prices were established) were understood but they ruled out precisely the disequilibrium transactions to which Patinkin was linking Keynesian economics. There were results on the stability of non-tâtonnement processes (e.g. Arrow et al 1959) but these were far less general.

There were further equally fundamental problems. One noted by Arrow (1959) was that out of equilibrium it was logically impossible for all agents to be able to buy

and sell as much as they wished, thus violating the assumptions of perfect competition, implying that it might be necessary to turn to a theory of imperfect competition. The same problem was addressed, in a different way, by asking what demand functions would look like if agents were constrained in the quantities of goods they could buy and sell. Patinkin had begun to address this in *Money, Interest and Prices* in that he argued that demand for labour would depend on the quantity of goods and services firms were able to sell, but he did not see this as the prelude to a more general theory of disequilibrium transactions. The stimulus to this came with Robert Clower's (1965) argument that unemployment implied that households could not sell all the labour that they wanted to sell, and this meant that the Walrasian demand equations needed to be modified to incorporate realised sales of labour. Though this was not appreciated for several years, this argument exactly paralleled Patinkin's argument about the labour market. A further problem was that including money balances in the utility function did not answer the deeper question of why money should be held in the world described by general equilibrium models.

Though some general equilibrium theorists realised the need for a new, non-Walrasian concept of general equilibrium, the economist who did more than anyone to foster this idea was Axel Leijonhufvud, in his very widely cited book, *On Keynesian Economics and the Economics of Keynes* (1968). Seeking to elucidate the value theoretic content that he believed was in the *General Theory*, and despite not offering any formal, mathematical model, his achievement was to outline a world in which, because not everyone could buy and sell as much as they wished at the prevailing

prices, supplies and demands depended on quantities traded as well as on prices; a world in which adjustment speeds played a crucial role in determining how the system responded to disturbances; and in which the costs of acquiring information mattered. Leijonhufvud compared his vision of markets to the one Friedrich Hayek had proposed in the 1930s but, where Hayek had argued that the complexity of resource allocation problems meant that the market could act as an information-processing device, achieving results that governments could not achieve, Leijonhufvud provided reasons why they would frequently fail to produce desired outcomes, vindicating Keynes.

3. Developing a general equilibrium theory for an imperfect world

These attempts to use Walrasian general equilibrium theory to make theoretical sense of Keynesian economics provide the background against which Hahn's work needs to be understood. In the 1950s he engaged directly with Modigliani and Patinkin, working within the framework they had established. He pointed out an error in Patinkin's treatment of bonds, working out the implications for the determinacy of the system, and he argued that Modigliani's model was incorrect, using arguments similar to those used by Patinkin, and he defended Patinkin's critique of the 'classical dichotomy,' according to which relative prices were determined independently of the money price level (Hahn 1953, 1955, 1960). The extent to which he was working within the conventional framework is shown by his having ignored, in a paper on growth, the problem of how resources were allocated out of equilibrium, when plans were, by definition, inconsistent (Hahn 1962).

In 1962, Hahn changed direction, moving towards more formal general equilibrium analysis in the tradition of Arrow and Debreu (1954). Together with Takashi Negishi, whom he had met on a visit to Stanford in 1959-60, Hahn proposed a model in which trade took place outside equilibrium, but in which equilibrium was nonetheless stable (Hahn and Negishi 1962). Though an exercise in 'pure' general equilibrium theory, it was a step towards a more rigorous theory that could provide support for Keynesian economics. The previous year, Negishi (1961) had published a paper on imperfect competition, and though they must have known of Arrow's argument about the necessity of imperfect competition out of equilibrium, published in the year they were together at Stanford, no connection was made in the paper between disequilibrium and imperfect competition.

In his textbook, *General Competitive Analysis*, written with Kenneth Arrow (Arrow and Hahn 1971), the final chapter discussed the Keynesian model. This, they argued, necessitated analysing a model that differed from the standard one in important respects. Money needed to be incorporated, agents had to be modelled as inheriting assets and obligations from the past and account had to be taken on their expectations of the future. Though they chose different terminology, they followed Clower in distinguishing effective from notional excess demands, and in arguing that there might be a 'quasi-equilibrium', a state in which prices did not change even though there was excess supply in one market. They conceded that their chapter was no more than a beginning in taking Keynes seriously, because a precise formulation of his ideas would be 'extremely complex' (Arrow and Hahn 1971, p. 367). They did,

however, fully endorse Keynes's claim that 'the theoretical evidence to be adduced from constructions in which these problems [the influence of the future and the past on the present, and the absence of key futures markets] did not arise is not relevant' (Arrow and Hahn 1971, p. 369). In other words, general equilibrium models that did not account for key features of the world could not be used to establish the truth or falsity of Keynes's claims.

In the same year, Hahn (1971b) tackled what was arguably an even deeper issue in general equilibrium theory, in that it had implications not only for out-of-equilibrium behaviour but for the equilibrium itself: he sought to model an economy in which transactions were costly—a model in which buying and selling goods used up real resources.⁷ This marked a major break with the Arrow-Debreu world and a move towards a non-Walrasian theory. It meant that ownership of goods became significant, that buying and selling prices of goods (along with borrowing and lending rates of interest) might differ, and that it became much more difficult to define an efficient allocation of resources. An equilibrium might well be inefficient. However the most important implication was, Hahn argued, that it became necessary to analyse sequences of markets: it was impossible to assume that all transactions took place simultaneously. Time became an integral part of the model, not simply something that needed to be taken into account only in analysing out-of-equilibrium behaviour. Although Hahn did not then know what an appropriate

⁷ In an earlier paper (Hahn 1965), he had considered the existence of equilibrium in an economy with money, focusing on Patinkin's model.

theory would look like, the presence of transactions costs and the need for transactions to take place in time created a potential role for money—with properties that distinguished it from other goods—something that had no place in the Arrow-Debreu world.⁸

Hahn was, however, not satisfied with the way he had tackled money and he continued to work on the problem (Hahn 1973a, p. 449; see also Hahn 1973b). No doubt building on what he had learned from Hicks, he saw period analysis and hence ‘sequence economies’ as crucial, but not all sequence economies were equal. A key distinction was between ‘inessential’ and ‘non-inessential’ sequence economies. An ‘inessential’ sequence economy was one where transactions took place at different dates but where the choices available to the agent were the same as those available to an identically-specified Arrow-Debreu economy in which all transactions took place at once. In contrast, a ‘non-inessential’ sequence economy, the dates when transactions took place could affect the opportunities available to the agent. It was only in the latter that Hahn saw any role for money. Equally important, non-inessential sequence economy—and no economy in which there was a futures market for money—could be Pareto-efficient. The significance of this argument is that Hahn was trying to model a transactions technology that made money essential, without having the need for money depend on arbitrary assumptions. He was creating a model to capture what he believed were essential features of the real

⁸. Money had the properties that (1) it was the same for all individuals; (2) it could be stored costlessly; (3) it did not enter the utility function.

world, though in order to analyse them rigorously the model had to become extremely abstract and depart from reality in ways that were believed irrelevant to the problem in hand. There was neither production nor uncertainty, and in order to ensure that money was held, he had to assume that agents began with an initial endowment of money that eventually had to be returned to the government (which knew what the initial endowments were). He could argue that it was a move away from the Arrow-Debreu world, but the resulting theory was hardly realistic.

The fact that these results about money were obtained only at a highly abstract level was no doubt why, in the same year, Hahn concluded a discussion of the foundations of monetary theory with what he called 'a defensive note'. He conceded that there was truth in the argument, associated with Friedman, that it was more fruitful to adopt a Marshallian approach and 'to put the Walrasian search for generality and rigour behind us' (Hahn 1973b, reprinted in Hahn 1984, p. 172). However, rigorous analysis could show that there was a profound difference between economies within which money was essential (in other words, economies in the date at which transactions took place mattered) and ones in which it was inessential. Given that it was clear that money was essential in the real world, such analysis, he argued, had made 'a fundamental difference to the way in which we view a decentralised economy' (Hahn 1984, p. 173). In other words, it undermined the idea that models such as the Arrow-Debreu model, in which money played no role, could provide valuable insights into how resources were allocated. This was a theme to which he repeatedly returned, arguing that to believe that an Arrow-

Debreu general equilibrium model applied to the real world was to show one did not understand it.

Hahn was not the only economist to search for a new type of general equilibrium theory.⁹ In presenting models of economies where money was essential he was able to point to a different, arguably parallel, approach being taken by Jean-Michel Grandmont and Yves Younès (1972, 1973), two of a large number of francophone economists who sought to develop new conceptions of non-Walrasian equilibrium. Jacques Drèze (1975) developed a highly influential model in which prices were inflexible, including what came to be known as a Drèze equilibrium. At Berkeley, in a PhD thesis supervised by Debreu, Jean-Pascal Bénassy (1973) developed both a ‘Keynesian’ concept of equilibrium involving sticky prices and an equilibrium involving monopolistic price setting.¹⁰ While this work, as he put it, was within ‘the general framework of Debreu’s “Theory of Value” [Debreu 1959],’ the thesis also explored macroeconomic models and price-dynamics, drawing on what became one of the most widely cited papers on the subject, by Robert Barro and Herschel Grossman (1971). Their ‘general disequilibrium’ model, had combined Patinkin’s insight that constraints on sales of goods would reduce the demand for labour, and Clower’s observation that demand for goods would depend on the actual level of employment, to show how there might be an equilibrium in which effective excess

⁹ A selective overview of the literature, not focused on Hahn, is in Backhouse and Boianovsky 2013, chapter 7.

¹⁰ Drèze’s model was not published until 1975, but an earlier discussion paper on the subject, dated 1972, was cited in Bénassy’s thesis.

demands were zero even though there was unemployment and excess supply of labour.

For much of the 1970s, though Clower and Leijonhufvud were still widely cited, it was arguably the model proposed by Barro and Grossman that came to represent the new approach to macroeconomics. The result was that 'disequilibrium macroeconomics' came to be seen not as the more general theory of market interaction when agents face informational constraints that Leijonhufvud envisaged, but simply as a theory of what happened when prices were sticky. Some economists, such as Edmond Malinvaud, whose widely cited *The Theory of Unemployment Reconsidered* (1977), which offered an elegant restatement of one of Benassy's models, started from the premise that the phenomenon of stagflation showed that markets were not working smoothly. It was, however, more common to take the view that it did not make sense to assume sticky prices at a time when inflation was in danger of getting out of control. There was therefore a move towards more explicit analysis of the factors that prevent a competitive equilibrium from emerging, such as information asymmetries and imperfect competition.

In the mid-1970s, drawing on the disequilibrium theory of Bénassy, Malinvaud and Younès as well as on work by Roy Radner and Michael Rothschild on information theory Hahn sought to develop further concepts of general equilibrium. In one paper he investigated what he called 'conjectural' equilibria (Hahn 1977). If competition is imperfect and information limited, agents have to form conjectures about the prices at which they will be able to buy or sell commodities and labour,

and about the quantities they will be able to trade at those prices. In such a world there is an equilibrium when, given a set of conjectures, the system generates signals that are consistent with those conjectures. Where he parted company with more conventional theories was that these conjectures might involve an element of arbitrariness: the requirement that there be a conjectural equilibrium did not necessarily remove that arbitrariness.

In another paper (Hahn 1978) he began with a Drèze equilibrium, in which prices were fixed and agents were rationed, and then moved on to one in which agents formed conjectures about how their perceived rationing constraints could be relaxed by charging a different price. This implies imperfect competition, but with a crucial twist: imperfect competition was not central to the model. A Walrasian, full-employment, equilibrium was possible but it was also possible to have non-Walrasian equilibria. It was, he argued, possible for an economy to get stuck in a quantity-constrained equilibrium, analogous to that described by Barro and Grossman, even when prices were not fixed and a Walrasian equilibrium was available. This was a vision of the world closer to the one described, though not modeled formally, by Leijonhufvud.

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4. The Lucasian challenge

When Keynesian economics came under attack from what came to be known as

‘monetarism’ in the late 1960s, Hahn was among the critics. His main objection was to the confidence Friedman had in his own views given that the arguments he adduced for them appeared to be weak. Friedman might appeal to statistical evidence but he made use of what Hahn considered ‘informal,’ ‘inadequate’ and ‘casual’ theorising that suggests a ‘lack of seriousness’ (Hahn 1971a, p. 62).¹¹ The obvious example was Friedman’s well known description of the ‘natural’ level of unemployment as that ‘ground out by the Walrasian system of general equilibrium equations’ when modified to cover real-world imperfections but Hahn went further claiming, ‘Friedman has a lazy man’s theory of the demand for money in that he explains it by unanalysed utility and productive services’ and that his empirical results were no more than suggestive (Hahn 1971a, p. 69). After a lengthy discussion of Friedman’s arguments about the optimum quantity of money, Hahn found it depressing that Friedman did not offer a serious study of efficiency in an economy in which money played an essential role, and that he even seemed ‘quite unaware of the tasks facing a student of money’ (Hahn 1971a, p. 72). Friedman might write as though he was willing to change his mind in response to new evidence, but Hahn found it difficult to take such remarks seriously. In contrast, Hahn was genuinely uncertain about the correct answers: ‘It is a good part of my own position that the formulation of a model of an economy which can account for money is immensely difficult and remains to be accomplished’ (Hahn 1971a, p. 63).

However, Hahn’s main target was not Friedman but Robert Lucas and the economists he chose to call ‘Lucasians’ (e.g. Hahn 1982a, *passim*; 1982b, pp. 311, 321, 324; Hahn and Petrie 2003, p. 206). Friedman ideas might have the greater appeal with policy-makers but it was Lucas’s new classical macroeconomics that had the greater effect on academic economics. Lucasian ideas also involved a much more explicit use

¹¹. Hahn 1971a was a review of Friedman 1969.

of general equilibrium theory, for he took up, from Edmund Phelps the idea that markets should be modelled as being continuously in equilibrium and used stochastic shocks to explain the business cycle (Lucas 1972). This had the advantage that random shocks ceased to be something added on for the purposes of econometric estimation but became integral to the theory. The Lucasian approach also stimulated econometric research because of the problems posed by forward-looking expectations for established estimation methods (see Lucas and Sargent 1981). The price of bringing macroeconomics closer to econometric methods was reliance on the representative agent model because, although Lucas and his followers were building general equilibrium models, they were highly simplified models in which the aggregation problems arising from heterogeneity of agents was ignored. To cut a long and familiar story short, this was the origin of the new classical macroeconomics, real business cycle theory and what came to be known as the dynamic, stochastic general equilibrium (DSGE) model.¹²

Hahn became one of the most vigorous critics of the way general equilibrium theory was used, for he did not consider it theoretically rigorous. In his Presidential Address to the Royal Economic Society, he wrote:

Much of what goes under the title of ‘classical macro-economics’ I find profoundly unsatisfactory. I find it so not because of any Keynesian pieties nor, I hope, because of political preferences. It is the crudeness and incompleteness of the theoretical

¹². See Backhouse and Boianovsky 2013 for a short account; for a more detailed history, see Hoover 1988 and De Vroey 2016.

foundation which disturbs me. (Hahn 1988, pp. 957-8).

As examples of ignored problems, he cited the possibility that markets might be incomplete and that there might be multiple equilibria. Though he was making such arguments for at least two decades, a good statement of his position is provided in a book that was the result of discussions with Robert Solow over many years: *A Critical Essay on Modern Macroeconomic Theory* (1995). In this book, Hahn and Solow started from the new classical model and modified the assumptions, one by one, showing that as soon as this was done, the theory's Panglossian policy conclusions disappeared. These modifications, all in the direction of greater realism, included replacing agents who maximised utility over an infinite horizon with a consumption-loan model of household saving, introducing wage stickiness and monopoly power derived from increasing returns to scale.

Hahn was clearly far from alone in being a strong critic of Walrasian economics—it had innumerable critics, both among economists and non-economists. However, unlike many critics, this did not lead Hahn to reject Walrasian theory: as a student of Kaldor, he had a history of defending general equilibrium theory against Keynesian critics. Unwilling to give up the commitment to what he called 'lucid and rigorous theorising', he argued that Walrasian theory was far from useless: it was just 'seriously incomplete' (Hahn 1990b, p. 1). Account had to be taken of imperfect competition and strategic behaviour, missing markets, and imperfect and asymmetric information, as a result of which the fundamental theorems of welfare economics, on which the Lucasian approach relied, did not apply. He summarized his attitude:

While none of us (I believe) has much sympathy for attempts by hook or by crook to

fit actual economic data into a (vastly simplified) Walrasian framework, it is also the case that we know that framework to have been, and to continue to be, of great importance to economic theory. A genius may arise and make it totally obsolete. But the rest of us must think our way step by step to a richer and more satisfactory construction. (Hahn 1990b, p. 4)

An important contribution to this task came through his encouragement and support for younger economists, especially at Cambridge, where, with the support of the Economic and Social Research Council, he organised a project, involving over twenty economists, ‘Information, risk and quantity signals in economics.’ Such issues were systematically explored in the project that culminated in *The Economics of Missing Markets, Information and Games* (1990), from which the previous quotation is taken. This project involved over twenty economists. What they had in common was a commitment to what Hahn described as ‘lucid and rigorous theorizing’ in the tradition of Arrow and Debreu and an acknowledgement that Walrasian general equilibrium theory was ‘seriously incomplete’ (Hahn 1990b, p. 1). Starting from the assumption that the Arrow-Debreu model postulated many markets that did not exist in reality (the model required that there exist insurance and futures markets for all goods), he observed:

‘Missing markets’ lead to reformulations and questions. The economy has to be studied as one in which there is trading at every date. This in turn requires agents to formulate expectations concerning futures market signals in different states of the world. One can postulate that these are ‘rational,’ that is, that each agent knows the future price of every good as a function of the state of nature. However one is then naturally led to ask how these expectations come to be held; that is, one will be interested in the difficult and very open questions of the learning by agents and with the interaction of such learning with what there is to be learned. ... However, one also

wants to know why particular markets are missing; can the existence or non-existence of markets be itself a matter for a theory to settle rather than to take exogenously?

Here one is at once face to face with the problem of information and information transmittal ... (Hahn 1990b, p. 1)

David Kreps constructed a theory of out-of-equilibrium beliefs and how these beliefs could affect out-of-equilibrium behaviour. David Newbery asked about the consequences of missing markets and what could be done about this. Margaret Bray investigated the way in which financial markets transmitted information, considering markets where rational expectations equilibrium existed and ones where it did not. Other contributors considered more concrete problems including the economics of research expenditure (Partha Dasgupta), bank runs (Luca Anderlini), auctions (Eric Maskin and John Riley) and labour contracts. The underlying theme was that the standard Walrasian theory to which Lucasians had turned was defective because it did not allow such questions to be asked.

5. On theoretical rigour

The debate between Keynesianism and its critics (often lumped together under the loose heading of ‘monetarism’) was both theoretical and empirical. Friedman claimed that his policies were founded on detailed and persuasive empirical evidence, and the new classical macroeconomics, and subsequently real business cycle theory and DSGE models were defended on the grounds that they were not only theoretically rigorous but also supported by statistical data. Methodological discussions, therefore, have often focused on the extent to which these debates were in fact empirically driven and whether economics can live up to its claim to be an empirical science (see, for example, Blaug 1992; Backhouse and Salanti 2000). From this perspective, the role of general equilibrium theory is questionable, for it is

generally believed to have little empirical content.

Hahn took a different view. He praised economic theory (see Hahn 1985, pp. 10-28). His reasoning was that theorising led to understanding, and whilst this might be a necessary precondition for successful prediction, it was not the same as prediction. The Arrow-Debreu theory of general equilibrium was not intended to be realistic or to generate predictions but to aid understanding. It was based on sensible axioms, notably that people have preferences and that they will not persist in holding beliefs that are clearly mistaken. However, if theory was to create understanding of money and expectations, both of which were generally considered central to macroeconomics, a theory that went beyond the Walrasian theory favoured by the new classical macroeconomists was required. Hence his developing models of sequence economies and non-Walrasian equilibria. This made him critical of economists who engaged in what he considered casual theorising, even if their theories were tested against statistical data. Economists needed to be serious in their theorising, which meant recognising the limitations of theory. Bad theorising, Hahn argued, involved 'a premature claim to understand' (Hahn 1985, p. 22). He was thus critical of Friedman's 'as if' methodology on the ground that it did not result in understanding because it did not explain the causal processes involved. He was even more critical of 'Lucasians' and others who abused theory by drawing conclusions from theories that were demonstrably false. Not surprisingly, Hahn vigorously defended the use of mathematics in economics, despite conceding that it could have two harmful effects: it might lead theorists to ignore factors that could

not be captured in the mathematics, and it might inhibit creative thinking that necessarily lacked rigour in its early stages.

Hahn, throughout his career, confined himself to rigorous economic theorising, consistently demonstrating, at a high level of rigour, that the Walrasian theory could not bear the weight being placed upon it. He and those associated with him, such as the contributors to *Economics of Missing Markets, Information and Games*, worked out rigorous accounts of phenomena that could not be accommodated within the Walrasian paradigm. In order to achieve this level of rigour, it was necessary to work with highly abstract models and to use a level of mathematical analysis that most economists found challenging, limiting the audience for such work. It may have seemed obvious to Hahn that a theory that, when defined with sufficient rigour to prove its coherence, had no role for money was inadequate. But this failed to undermine the Chicago claim that the world was sufficiently close to being competitive that the model of perfect competition was a sufficiently good approximation. Models of the type used by Lucas thrived despite the arguments of Hahn and other economists about why they should not be taken seriously.

It seems clear that an important reason for this was that, for many economists who did not probe as deeply as did Hahn and his associates into its conceptual problems, Lucasian theory, based on rational agents and perfectly competitive markets that equate supply and demand, was intuitively appealing. The Lucasian approach also prospered because of its rhetoric of empirical verification and the

challenge he offered to established empirical modelling practices.¹³ His monetary shocks model of the business cycle might eventually prove unable to withstand criticism, but it evinced an apparent seriousness about data analysis missing from the work of many of those who argued that competitive equilibrium models were treacherous guides to action. ‘Disequilibrium’ theories inspired econometric work, testing whether or not markets were in equilibrium. For example, Richard Quandt and Harvey Rosen were convinced that such theories could explain the US labour market better than equilibrium theories (see Backhouse and Boianovsky 2013, p. 84).¹⁴ Quandt has testified to the strength of economists’ prior beliefs about markets. In the US, the common reaction to his paper was ‘What you are doing [testing whether markets are in equilibrium] is silly because everyone knows that prices clear markets,’ while in Europe it was, ‘Everyone knows that prices never clear markets, hence there is nothing to test’ (quoted in Backhouse and Boianovsky 2013, pp. 75-6). One factor contributing to the belief that markets should be modelled as in competitive equilibrium was probably that, as markets were deregulated and as international trade became freer and more competitive, the assumption that the world was competitive seemed more plausible than it had when trade unions clearly had great bargaining power and there was talk of a corporate ‘technostructure’

¹³ Consider the collection titled *Rational Expectations and Econometric Practice* (Lucas and Sargent 1981). Goutsmedt et al (2019) argue that many Keynesians criticised Lucas (1976) on empirical grounds.

¹⁴ However, others concluded that only very limited conclusions could be derived from disequilibrium methods, and that the approach was of little value (see Laroque and Salanie 1995).

controlling much economic activity. A set of theoretical and empirical techniques that clearly *did* apply (as did disequilibrium theory) to centrally planned economies, with their history of shortages and rationing, was a disadvantage in claiming to provide explanations of how increasingly free market economies would behave.

Perhaps the main problem with Hahn's advice about theory is that it was a counsel of perfection. It required economists not to draw premature conclusions from theory. He likened the competitive equilibrium theory from which Lucasians drew conclusions to scaffolding:

This theory at the end of the twentieth century can at best be regarded as scaffolding and not the building. The latter at present has a few bricks, and some of the material that will be needed is lying around. Honest economists will be engaged on the building: they will not claim to have brick and mortar when they are standing on planks. Above all, while there are no objections to tidying up the scaffolding here and there, let the scaffolders be silent on public affairs while the building is nowhere in sight. (Hahn 1982a, p. 106)

However, that is not the world in which economists live, where the pressure to publish places a premium on novelty and immediate relevance. In the short run (which may last longer than the problem situations to which economists are responding), theoretically more rigorous models may fail to outperform others. Lucas (1980) recognised this when he argued that models involving what he calls 'free parameters', not grounded in individual rationality—models that violate the canons of general equilibrium theory—may perform better than ones that do not. He might also have pointed out that models based on demonstrably unrealistic assumptions, such as homogeneous agents and full information may perform better than ones based on more plausible assumptions.

It may also be that Hahn was too fastidious in his advocacy of ‘serious’ theorizing. This was the view of at least one critic, Hyman Minsky. Reviewing a series of lectures on *Money and Inflation* (1982a), in which Hahn tackled monetarist or Lucasian claims head on, Minsky (1984) acknowledged the brilliance of some of Hahn’s arguments and he sympathised with Hahn’s positive view of Keynes and his critical attitude towards the Arrow-Debreu model. However, he claimed that Hahn’s fastidiousness concerning rigorous theorizing prevented him from following up the leads that would have taken him away from ‘standard general equilibrium formulations’ (Minsky 1984, p. 450). This means that Hahn was ‘properly destructive of much academic cant, but ... not yet a serious builder of theory’, for he failed to integrate Keynesian insights into a satisfactory theory (Minsky 1984, p. 451). Minsky saw Hahn’s real economy as remaining that of Walrasian general equilibrium theory, even if he sought to move beyond that theory.

A useful comparison is with Leijonhufvud. Leijonhufvud did not even try to construct an argument to Hahn’s standards of rigour—perhaps this was why Hahn did not engage with his work—and he relied on verbal analysis, but he offered a vision of what a different type of economics might look like, in which the intuitions of Walrasian theory would often be a misleading guide to action. For around a decade, Leijonhufvud’s vision inspired economists to develop new models. However, by the late 1970s, despite these attempts to develop models that instantiated the Keynesian vision of the world as interpreted by Leijonhufvud, and despite the attempts of Hahn and his associates to develop non-Walrasian theories,

‘classical’ theories based on perfect competition had regained their hold. The reason may have been the technical difficulty of theories such as Hahn’s, or it may be that, when applied to macroeconomics, the result was generally perceived by many economists to be fixed-price models that lacked theoretical foundations and that were inappropriate for a world in which prices were changing all too rapidly. Imperfect competition, menu costs, asymmetric information and other complications were successfully used to salvage Keynesian propositions from the wreckage resulting from the new classical onslaught, but no general theory emerged that was capable of challenging the vision that underwrote the appeal of *laissez faire*.

Minsky’s critical assessment of Hahn’s approach fails to recognize the extent to which Hahn *did* try to develop a non-Walrasian general equilibrium theory even if he failed to consider directions, such as taking better account of banking, that Minsky considered important. Minsky was, however, right to see Hahn as seriously constrained by his commitment to theoretical rigour. The issue is whether such rigour is preferable to looser theorizing, when we are faced with a world that is far more complex than anything that can be modelled — a world where *no-one* has a rigorous theory capable of capturing everything that we believe to be important. Hahn’s commitment to theoretical rigour undoubtedly expanded the boundaries of what could be shown theoretically, especially when, in the 1960s and 1970s, he began to develop theories, inspired by Hicks, that provided theoretically more rigorous models than the essentially static models used by Modigliani and Patinkin. However, even to those who were sympathetic, it was more effective in

undermining naive theories than in building alternatives. And it failed to undermine the widely held belief that the new classical economics was theoretically more rigorous than its Keynesian predecessors. To a public impatient for answers and requiring simple explanations that made sense, Friedman's monetarism, however glaring its defects in the eyes of a general equilibrium theorist, was far more persuasive, at least in the short term. In the long run, of course, we do not know whether Hahn's critique of the abuses of general equilibrium theory may, despite short-run setbacks, prove more influential.¹⁵ This leaves room for debate over whether, in practice, Hahn's commitment to theoretical rigor was better than the more eclectic approach of Keynesian economists such as Samuelson, or the verbal logic of Leijonhufvud.¹⁶

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¹⁵ I leave open the question of whether this has already happened with the incorporation of imperfect competition into DSGE models.

¹⁶ When it came to methodology, Hahn focused on its consequences in practice, and did not accept the argument that methodology should be studied as rigorously as possible (see Hahn 1992a, 1992b; Backhouse 1992).

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