

Part III

The twentieth century: from American
institutionalism to the end of history

13
TALCOTT PARSONS AND
THE ASCENT OF AHISTORICAL
SOCIOLOGY

'Tis a common proof, That lowliness is young ambition's ladder, Whereto the climber-upward turns his face; But when he once attains the upmost round, He then unto the ladder tucos his back, Looks in the clouds, scorning the base degrees By which he did ascend.

(William Shakespeare, Julius Caesar)

Talcott Parsons is regarded as one of the greatest sociologists of the twentieth century. He was largely responsible for bringing Max Weber to the attention of Anglophone scholars. He also elaborated a distinctive and highly influential school of functionalist sociology. However, one of its features was its neglect of the problem of historical specificity, despite the direct influence upon Parsons of Weber and Werner Sombart. In this manner, a few elements of the German historical school tradition were transferred to the American context. But they were dispossessed of much of their content and meaning. Ironically, Parsons achieved distinction by his creation of an ahistorical school of sociology, partly by rummaging selected material from a historically-oriented intellectual tradition.

Furthermore, as well as the German historical school, another major but largely unacknowledged influence upon Parsons was American institutional economics. However, despite these major injections of historicism and institutionalism, Parsons created a distinctively ahistorical system of sociology. This chapter examines this remarkable and fateful transformation of a social science. We examine Parsons's engagement with key thinkers such as Walton Hamilton, Clarence Ayres, Leonard Hobhouse, Harold Laski, Richard Tawney, Bronislaw Malinowsky, Frank Knight, Alfred Whitehead, Joseph Schumpeter, Lionel Robbins and Ralph Souter.

THE ROLES OF WALTON HAMILTON AND
CLARENCE AYRES

From 1920 to 1924, Parsons was an undergraduate student at Amherst College in Massachusetts. Hamilton, who was a member of its faculty from 1915 to 1923, taught him institutional economics. As noted in chapter 11 above, Hamilton played a decisive role in establishing American institutionalism as a movement at the end of the First World War. Hamilton had a strong grasp of the theoretical essentials of the institutionalist movement and was concerned to promote institutionalism as a viable tool for economic policy making.

Another great influence on Parsons at Amherst was Ayres. Ayres had been appointed as a lecturer at Amherst in 1920. According to Parsons's own account, it was the example of Ayres and Hamilton, alongside the influence of his father, which helped to persuade Parsons to change his studies from biology to social science (Parsons, 1970, p. 877, 1976, p. 176; Camic, 1991, p. xv). Ayres and Hamilton impressed upon the young Parsons that institutions and culture, rather than biology alone, moulded human personality.

Given Ayres's formative role on Parsons's thought, it is necessary to sketch a portrait of the teacher in its intellectual surroundings. A forthright and independent thinker, Ayres was educated in philosophy and deeply interested in economics. His PhD dissertation at the University of Chicago had been on the relationship between ethics and economics (Ayres, 1918). As well as being Hamilton's colleague, Ayres was an enduring friend and correspondent of Knight (Buchanan, 1976; DeGregori, 1977; Samuels, 1977). Ayres did not then identify himself as an institutional economist, but he strongly believed that individuals were conditioned by social, cultural and institutional circumstances. On key questions he held a distinctive position. To understand this, we must briefly examine the intellectual context at the time.

As already noted above, in the early decades of the twentieth century, American social science was going through a massive transformation (Degler, 1991; Ross, 1991; Hodgson, 1999b). Before 1914, many sociologists and economists believed that biological instincts largely or wholly explained human behaviour. Around the beginning of the twentieth century, the leading American anthropologist Franz Boas challenged this biological-reductionist view. Boas did not deny the influence of biology on both physical and mental characteristics. But he saw social culture as far more important. However, Alfred Kroeber, a prolific student of Boas, went further. In a number of articles published in the *American Anthropologist* between 1910 and 1917 he declared that it was culture rather than heredity that determined human nature and behaviour.

Parallel developments occurred in psychology. The instinct psychology of William James and William McDougall, which was so influential in the 1890-1914 period, was subsequently displaced by the behaviourist psychology of John B. Watson, Jacob Robert Kantor and others. Although some behaviourists originally retained a role for instincts, the behaviourist stress on observable behaviour meant that, by the 1920s, many leading psychologists rejected the idea of instincts in human beings. Just thirty years after the heyday of James, the concept of instinct had virtually disappeared from American psychology.

Thorstein Veblen had accepted a role for instinct but stressed also the role of institutions and culture. His position was thus much closer to Boas than Kroeber. In the early interwar period, institutionalists such as Hamilton (1919, p. 318) followed Veblen and endorsed 'the part that instinct and impulse play in impelling . . . economic activity'. At the same time they all accepted that institutions and culture played a huge part in moulding human purposes and dispositions.

Ayres, in contrast, took a position that was close to Kroeber and those behaviourist psychologists who downplayed the role of instincts. Kantor (1922, 1924) in particular emphasised the malleability of human nature and the role of culture and institutions in forming the human psyche. Taking these anthropological and psychological developments to their extreme, Ayres eschewed instincts, to emphasise institutions, culture and the pliability of human nature. Ayres (1921a, pp. 561-5) wrote: 'When instincts fall out, institutions get their due Yet . . . the social behavior of the civilized adult is a matter of institutions and traditions . . . The social scientist has no need of instincts; he has institutions.'

Like many others in the 1920s, Ayres embraced the rising behaviourist psychology. Furthermore, he became an early enthusiast of a version of behaviourism that had no place for instinct. Ayres (1921b) thus rejected instinct psychology. He saw the individual as largely a social product. Although John Dewey remained an important influence, Ayres's agenda was very different from that of the pragmatists as a whole. Key pragmatist concepts such as habit were not prominent in Ayres's approach. In contrast, leading institutionalists such as Hamilton (1919) stressed a role for both habit and instinct in the explanation of behaviour. Most importantly, throughout his life, Veblen maintained an explanatory role for 'instincts as well as habits in his theory.¹

Under intellectual pressure at the time, other leading institutionalists shifted their position. After first endorsing instinct psychology, when it was subjected to intense criticism they began to have doubts. Hence, in his early writings, John Commons (1897, 1965) saw instinct as having a place. However, in the 1930s Commons (1934a, p. 637) seemed to side tentatively with the critics of instinct psychology. Similarly, Wesley Mitchell (1910) had earlier seen instinct as central to the explanation of human behaviour. In the 1920s he began to doubt this, writing later that 'the instinct-habit psychology will yield to some other conception of human nature' (Mitchell, 1937, p. 312).

In the 1920s, Ayres was moving with the thinkers at the cutting edge of American social science. He combined his theoretical iconoclasm with his radical liberal politics. Likewise, the young Parsons held radical and social democratic political views (Brick, 1993). Such opinions were not uncommon in American academia at the time. Liberal academics emphasised the universal potential of human achievement, seeing individuals as largely formed by culture and

¹ Much later, when Ayres (1958) claimed that his own ideas were a development of those of Veblen, he dismissed Veblen's instinct theory as of little ultimate significance. Although Ayres was right to point out that Veblen failed to define instinct adequately, nevertheless, contrary to Ayres, instinct psychology remained foundational to Veblen's position. See especially Veblen (1914).

unconstrained by biological inheritance. For this reason, the idea of biological determinants of human behaviour was opposed. Notions of instinct or biological determination interfered with the dogma that human nature was highly malleable, and that all individuals had a similar potential to achieve and to prosper.

The force behind this intellectual movement in academia had more to do with ideology than with science. Carl Degler (1991, p. viii) and others have argued persuasively that these developments were largely inspired much more by a politics than scientific evidence: 'The main impetus came from the wish to establish a social order in which innate and immutable forces of biology played no role in accounting for the behavior of social groups.' In the contexts of prevalent racism and interwar fascism, such reactions among liberal academics were understandable. The idea that there were any biological foundations to human behaviour was abandoned in the more liberal and leftist intellectual circles of American academia.

Most institutional economists were in accord with these ideological developments. However, their consequence was that the original foundations of institutionalism, in Darwinism and instinct psychology, were removed. This subsidence in its philosophical and psychological foundations weakened institutionalism at a crucial stage of its theoretical development. Institutionalism was profoundly affected by the concomitant separation of biology and social science. Accordingly, the Veblenian research programme of building a 'postDarwinian' and 'evolutionary' economics was compromised. Although institutionalism continued to represent many popular themes, it lost its original theoretical mission. For instance, while the increasing emphasis on the role of culture was an asset for institutionalism, the intellectual context in which the shift to culture took place made the further development of a distinctive and systematic institutionalist theory much more difficult. Institutionalism lost its methodological cutting edge.

Ayres was moving with the times, and ahead of many of his institutionalist colleagues. Although Ayres became the leader of American institutionalism after 1945, in the 1920s and 1930s his position was relatively marginalised among institutionalists. Yet even in the early 1920s his radical account of institutional influence proved attractive to some - at least for young iconoclasts such as Parsons.

CONVERSION AT AMHERST

Ayres taught Parsons in a philosophy class at Amherst on 'The Moral Order' with strong ethical and sociological themes. As Parsons (1959, p. 4) himself recollected:

We read Sumner's *Folkways* . . . and a whole lot of things like Charles Horton Cooley and Emile Durkheim. We also read a lot of Thorstein Veblen, for Veblen was an important mutual hero of both Hamilton and Ayres. So institutional economics was really my jumping off place.

Parsons (1976, p.178) later explained that it was Ayres who had introduced him not only to the work of Veblen but also to that of Emile Durkheim. The Durkheimian influence proved to be the more lasting of the two. While both authors stressed that the individual was always conditioned by social circumstances, Veblen had attempted to link the social and the natural sciences under a Darwinian scheme, and had made use of material from psychology. In contrast, Durkheim had stressed the differences between the social and the natural sciences, and insisted that psychology should be separated from the social sciences. At the time, in debates with his fellow students, Parsons used Veblenian language to criticise the 'leisure class motives' of those in America who were opposed to social reform (Brick, 1993, p. 368). In one of his surviving student essays he also embraced Veblenian concepts such as 'cumulative change' and 'habits of thought' (Wearne, 1989, pp. 28-9).

However, in his surviving student essays, Parsons confused a Veblenian emphasis on habits with the behaviourist psychology of conditioned reflex (Wearne, 1989, pp. 28-36). Given that Ayres admired both Veblen and behaviourism, it is likely that this mistaken conflation of two quite different doctrines was also the error of his teacher. Later Parsons was to reject both behaviourism and the Veblenian emphasis on habit. Hence, without much justification, Parsons (1935, p. 441) dismissed 'instinct' psychology' as 'a twin brother of behaviorism'. It is true that early behaviourist psychologists such as John Watson had admitted notions of both habit and instinct into their theory. However, there was an important shift of meaning. While Watson (1919, p. 273) had defined habit as a 'complex system of reflexes', Veblen and pragmatists (such as James and Dewey) saw habit as something that may exist even if it is not manifest in behaviour. As well as habits of behaviour there are habits of thought. Unlike the behaviourists, the pragmatists saw consciousness as part of the process. Dewey (1922, p. 25), for example, saw habits as 'means, waiting, like tools in a box, to be used by conscious resolve'. Parsons ignored these subtle distinctions.

Parsons ratified the emphasis that American institutionalists had always given to the influence of institutions and culture on human agency and behaviour. From his reading of the works of institutionalists and others, and under the special guidance of Ayres, Parsons concluded that it was culture rather than nature that was the decisive and overwhelming factor in explaining human behaviour. He thus emphasised institutions and culture even more than the average institutionalist. Parsons followed Ayres down the more radical road, initially placing himself as a sympathetic critic of institutionalism. But eventually, for reasons we shall explore in detail below, the sympathy was to wane, and to turn into hostility. Nevertheless, Parsons still emphasised the idea that culture and institutions somehow moulded behaviour.

Ironically, Ayres turned Parsons not simply into a follower of the new doctrine - that culture and institutions alone could explain human behaviour - but also into a critic of the then majority of American institutionalists. Although institutionalism itself was eventually to transform itself and move towards Ayres's position, Parsons's critical stance towards institutionalism was to remain. In 1923 a doctrinal conflict at Amherst College led to the sacking of its liberal

and progressive President. A substantial number of faculty members, including Hamilton and Ayres, resigned *en masse* in sympathy. So ended not only a prominent enclave of institutionalist thought but also one of the most seminal relationships between a student and his teachers in the history of social science.

TALCOTT PARSONS IN EUROPE

From 1924 to 1925, Parsons was at the London School of Economics (LSE). His Amherst teachers had provided him with introductions to LSE socialist thinkers such as Harold Laski and Richard H. Tawney. At the LSE he also came under the influence of the anthropologist Bronislaw Malinowski who introduced Parsons to functionalist modes of theory. In addition, as Parsons (1959, p. 4) related, at the LSE he 'got a great deal from' Leonard T. Hobhouse. Britain's first professor of sociology, Hobhouse wrote on social evolution but was critical of Spencerism. Influenced both by the German historical school and by the new philosophical ideas on emergence, he developed an 'organic' systems view. One of Hobhouse's theoretical projects was the identification of the social institutions that were necessary to promote freedom and social order? Ever since his class under Ayres at Amherst, the problem of social order was central for Parsons too. At the LSE he found both confirmation and development of some of the ideas to which he had been earlier exposed. Parsons (1976, p. 177) later reflected that: 'Of the intellectual influences to which I had been exposed since leaving Amherst, those at the London School of Economics were the closest to the Hamilton-Ayres point of view.'

Ironically, Parsons was to play a role in the 1930s that would help to change the nature of sociological thought in all Anglophone universities. This would parallel a similar and complementary change in economics, led in particular by Lionel Robbins from the LSE. The combined result of these developments would be to obliterate any traces of institutionalism or historicism at the LSE and elsewhere.

In 1925 Parsons moved to Heidelberg University, from where he obtained a doctorate in economics in 1927. His visit to Germany put him in contact with the historical school: one of his teachers was Edgar Salin. Another was Heinrich Rickert. His doctoral dissertation was on the theories of capitalism in the writings of Werner Sombart and Max Weber, with additional references to Karl Marx. Material from his dissertation was reworked and published in two articles in the *Journal of Political Economy* (Parsons, 1928, 1929). These two essays give clear evidence of the influence of the historical school. In them, Parsons (1928, pp. 652-3) criticised 'Anglo-American economic thought' for failing to recognise the historical specificity of the capitalist system.

² Hobhouse was also a major influence on Hobson, who wrote a biography on his mentor with Ginsberg, Hobhouse's successor at the LSE (Hobson and Ginsberg, 1932). Incidentally, Ginsberg (1932) went against the sociological trend of the time by attempting to retain some links between psychology, biology and sociology. He also maintained a notion of instinct in his analysis.

THE MOVE TO HARVARD

In 1927 Parsons was appointed an instructor in economics at Harvard University. Joseph Schumpeter was a visiting professor in 1927-8. Parsons took Schumpeter's economics classes and discussed a number of issues with him (Brick, 1993). In particular, Schumpeter encouraged Parsons to study the work of Vilfredo Pareto. Importantly for Parsons's line of research, Pareto had attempted a general theory in both economics and sociology, and tried to establish a boundary between the two disciplines. Pareto's work was to have a permanent influence on both authors.

The economics department at Harvard was predominantly neoclassical. There was a growing enthusiasm for mathematical formalisation and general equilibrium theory. With the prominent exception of Sumner Slichter, the department was opposed to institutionalism. Regarded more as a populariser than a theorist, this institutionalist was safely sidelined into the subdiscipline of labour economics. The leading theorists in the department, including the influential Frank Taussig - editor of the *Quarterly Journal of Economics* until 1936 - were then hostile to institutionalism.³

Initially placed within this department, the young Parsons would have been in difficulty, had he made any expression of sympathy for institutional economics. Already a critic of institutionalism, Parsons went further. The circumstances prompted him to break entirely his dwindling links with the school. The Harvard environment encouraged Parsons to become more critical and dismissive of both institutionalism and the historical school in the years from 1927 to 1935. The change in attitude is remarkable. In the late 1920s, Parsons (1928, 1929) was still sympathetic to German historicism, but he criticised it a few years later (Parsons, 1935). Eventually, Parsons was to write of institutionalism in critical and dismissive tones. References to his own institutionalist past were omitted or downplayed, despite them having been highly significant in his own intellectual development (Camic, 1992).

However, Parsons was not inclined to follow a career as a neoclassical economist. He was not a mathematician and he remained uneasy with neoclassical ways of thinking. Having specialised in the works of both Sombart and Weber, Parsons took a number of strategic career decisions. The first concerned the possible use of the intellectual assets he had acquired in Europe, particularly his doctoral study of both Sombart and Weber.

Would he promote one of these names, or both? Sombart's name was known among intellectuals in the West and his works were in need of translation. Parsons (1929, p. 34) himself had admitted that: 'Unlike Sombart, Weber never developed a unified theory of capitalism.' For this reason, Parsons could have chosen the role of translator and interpreter of Sombart, rather than of Weber.

But Parsons chose Weber, and did not acknowledge Sombart a great deal in his future writings. Reflecting on this choice, much later in his life, Parsons (1976,

³ For a history of the department before the Second World War, see Mason and Lamont (1982).

p.178) said that he chose Weber because he was allegedly 'by far the most important', compared with Sombart and Marx. Leaving this questionable judgement of intellectual calibre on one side, Parsons's earlier works provide clues to further reasons behind his choice of Weber. In a typically brief and foggy discussion of this issue, Parsons (1937a, p. 499) remarked that Sombart's theory, centred as it is on the concept of *Geist*, 'altogether eliminates the utilitarian factors. Hence Sombart's perfectly logical and definite repudiation of orthodox economic theory.' There's the rub: Sombart repudiated orthodox economics but Weber - in Parsons's interpretation - apparently did not. We are left with the distinct impression, even in Parsons's own account, that he chose Weber over Sombart because Weber could be rendered less abrasive for orthodox economists, thus making Parsons's academic life easier. Weber's social theory, with its individualistic elements, did not threaten orthodox economics as much as that of Sombart.⁴

Alongside Pareto and Durkheim, the prospect existed of using Weber's name as a posthumous 'founder' of Western sociology. In the years 1928-30, Parsons translated Weber's *Protestant Ethic and the Spirit of Capitalism* into English. He also helped to produce an edited version of Weber's *Economy and Society* in 1947. But Parsons downgraded Weber's membership of the German historical school and his engagement with the problem of historical specificity. He exaggerated some aspects of Weber's work, to downgrade Weber's strictures on the limits of general theorising. As David Zaret (1980, p. 1193) rightly argues: 'Parsons's interpretation of Weber is idiosyncratic and unduly stresses normative aspects of meaning.' Parsons attempted a general sociological theory in Paretian spirit, while observing Durkheim's conceptual divide between society and nature.

Although he often toyed with the word 'evolution', Parsons wanted biology to play no explanatory role in the social sphere. He also distanced himself from psychology in all its existing forms. In this respect he followed and exaggerated the hints of Durkheim, Weber and Sombart. All three had been major figures in his intellectual development. Parsons paid lip service to the viability of psychology as a science, but its particular claims to enter the terrain of explanation of human action were opposed. His efforts in these directions were so successful that most modern sociologists take such impervious disciplinary separations for granted.

Parsons had two major intellectual assets. The first was the emphasis he had acquired from Ayres and Hamilton on the over-arching role of culture and institutions. The second was his recognition and possession of the highly marketable intellectual legacy of Weber. Unwilling or unable to follow either the institutional or neoclassical roads within economics, Parsons had to carve

⁴ The choice between Weber, Sombart and Marx must also be placed in the dramatic and global political context of the 1930s. Clearly, of the three, Weber was the closest to an American-style liberal. However, to make him one, his German nationalism and pessimistic views on progress would have to be overlooked (Hennis, 1988; Mommsen, 1984). Nevertheless, Zaret (1980, p.1193) argued convincingly that Parsons's ideological reaction against Marxism was significant and that 'Parsons saw in Weber's writings a non-Marxian foundation for general theory.'

out a new territory for sociology. His break from economics was his second strategic decision. As Hans Joas (1995, p. 275) put it: 'by dint of the approach he was taking, Parsons realized that he was being forced out of the prestigious discipline he had started his career in.' In 1931 Parsons transferred to a new department at Harvard, in which sociology found its home. 'Sociology offered Parsons a way out of this personal and theoretical crisis as well as a solution to the problem of the definition of the proper field of economics' (ibid.).

In 1932, Paul T. Honran of Cornell University wrote to Parsons, expressing his view that institutional economics was at a dead end (Honran, 1932). Honran urged Parsons to make the work of both Weber and Durkheim known to American social scientists (Wearne, 1989, p. 60). Both Weber and Durkheim were to be major figures in Parsons's *Structure of Social Action*.

Parsons became deeply engaged with the problem of demarcation between economics and sociology. To accommodate culture and institutions while rejecting the role of biology or instinct, sociology itself had to be transformed. Furthermore, it had to reach a new *modus vivendi* with the rising new wave of neoclassical economics and preserve its own intellectual territory. As Parsons (1970, p. 827) himself remarked: 'It gradually became clear to me that economic theory should be conceived as standing within some sort of theoretical matrix in which sociological theory also was included.'

At Harvard, Lawrence Henderson set up the famous 'Pareto Circle' in 1932, of which Parsons and George Homans were members. Henderson (1935) admired Pareto and regarded him as the modern Galileo of the social sciences. This Circle read and discussed Pareto's works in sociology and economics. Historians of ideas such as Barbara Heyl (1968) and Geoffrey Hawthorn (1976) regarded the Pareto Circle as crucially significant in the development of Parsons's sociology. Notably, Henderson was critical of institutionalism, especially if it was based on an empiricist methodology or associated with political radicalism. In his critique of empiricism, Henderson rightly insisted that any discerned fact depended in part on the theory that framed it, and agreed with Weber and others on the need for theoretical abstraction in scientific reasoning. Parsons (1937a, p. xxiii) later thanked Henderson for his extensive critical remarks on an early draft of *The Structure of Social Action*, especially on 'the interpretation of Pareto's work'. Henderson also influenced Parsons in his development of the concept of 'social system' (Parsons, 1951, p. vii).⁵

Like others in that Circle, Parsons was strongly influenced by Pareto's distinction between 'logical' and 'non-logical' actions. Pareto saw 'logical' actions as being those where means were consistent with, and appropriate for, the given ends. For Pareto (1971), the study of such 'logical' actions was the domain of economics. On the other hand, like Weber, Pareto (1935) upheld that the residual class of 'non-logical' actions governed much of human behaviour. Such actions were seen as the subject matter of sociology. Accordingly, economics was a

⁵ In turn, and on this score, Henderson had been influenced by Whitehead, who was also at Harvard, and is widely regarded as a key inspiration in the development of systems theory (J. Miller, 1978).

limiting case of the broader theory of social action that it was the task of sociology to build.

In broad terms, Parsons enthusiastically adopted this approach. He saw 'nonlogical' actions, involving ideology and values, as establishing the domain and independence of sociology as a science. This development in Parsons's thinking was outlined in letters to Knight in 1932-3 (Camic,1992, pp. lii-liii). Like Ayres, Parsons continued to benefit from discussions with Knight, who was widely read in the German and English social sciences, and highly useful as a critic and sounding board for their ideas.

THE ROLE AND MANOEUVRES OF JOSEPH SCHUMPETER

At this stage, another digression is in order, in part to understand the context of Parsons's own intellectual evolution, and in part as a relevant example in its own right. It concerns the role and intellectual development of Schumpeter. Schumpeter attained a permanent post in Harvard in 1932. He had really wanted to get Sombart's former chair in Berlin when it became vacant in 1931 but he was unsuccessful. As in the case of Parsons, Schumpeter's move to Harvard coincided with an increasing hostility, on his part, to institutionalism and historicism. Consider the following evidence. In 1926, Schumpeter published a careful and sympathetic account of the work of Schmoller and other historical school theorists. In this article Schumpeter (1926, pp. 3, 18, 22, 24 n. 46) wrote of Schmoller's 'great achievements', of his 'greatness', of his work being 'the programme for the future', of 'his overall achievements' and of his 'success'. In the same article, Schumpeter saw much merit in the work of the leading American institutionalist Wesley Mitchell. Although he also raised thoughtful criticisms, the disposition was largely positive. Within four years, however, Schumpeter was to shift the balance of his assessment of historicism and institutionalism, towards criticism alone. After his first visit to Harvard in 1927, Schumpeter became more openly critical of the historical school and highly dismissive of the institutionalist tradition. In the Harvard based *Quarterly journal of Economics* Schumpeter (1930, p. 158) referred scathingly to the intellectual capacities of both Schmoller and Veblen, and to 'the serious and even glaring defects in their equipment, both natural and acquired'. This atrocious personal abuse was supplemented with sweeping dismissals of much of historicism and institutionalism. Schumpeter (1930, p.159) pronounced on the 'unsatisfactory state of economic science in Germany' and how Veblen's erroneous teaching had fortunately been corrected in America by 'a phalanx of competent theorists'. But Schumpeter listed neither Veblen's errors, the corrections nor the 'competent theorists'. In a talk in Japan in 1931, Schumpeter (1991, p. 292) referred to the 'methodological errors of German historians'. He also described institutionalism as 'the one dark spot in the American atmosphere'. Overall, there was a remarkable transformation from Schumpeter's sympathetic 1926 article on Schmoller, to the

hostile statements of 1930-1, in which Schumpeter was keen to dismiss, and to detach himself from, the entire German historical school and American institutionalism. Fortunately for Schumpeter's career, these negative statements would have aided his application for a permanent post in Harvard in 1932.

Yet the irony is that Schumpeter continued throughout his life to draw on the work of the German historical school. Many of Schumpeter's ideas are traceable to the German historicists. For instance, Nicholas Balabkins (2000) has shown that several of Schumpeter's views concerning entrepreneurship have their origin in the work of Albert Schäffle. Schumpeter's notion of 'creative destruction' also has its precedents. Probably inspired by Friedrich Nietzsche, Sombart (1913b) gave multiple examples of how destruction and shortage can spur economic creativity, and concluded: 'again out of destruction a new spirit of creativity arises' (Sombart, 1913b, p. 207). Even more severely, Michael Appel (1992, pp. 260-2) wrote on Schumpeter's famous book *Capitalism, Socialism and Democracy* (1942) and its relationship with the work of Sombart and others:

In this context, it is remarkable that Schumpeter, who had so clearly distanced himself from the basic economic theory of Sombart, took over his ideas and perspectives on the development of capitalism almost completely. Without hinting of Sombart and the general literature of the 1920s and 1930s, Schumpeter basically offered only what had already been said and written decades ago in the German discussions of the 'future of capitalism' In his argument, Schumpeter kept completely to the thought that had been developed in Sombart's time. Schumpeter's analysis is in no way original.

The implication of this allegation is that Schumpeter at Harvard continued to draw from the German historicist legacy but gave an impression that this was largely his own work. This impression would have been aided by increasing Anglo-American ignorance of the German historicism after the 1930s. If this allegation is valid, Schumpeter had something to gain from fashionable dismissals of historicism. Yet he remained a great and original thinker in his own right. In order to establish his reputation, he did not need to connive with any vogueish or ignorant reaction against the historical school. The tragedy was that he did. It remains for future scholars to determine in detail how many of Schumpeter's ideas were in fact taken from the German historical school.

By the end of his career, however, Schumpeter (1954, pp. 12-13, 809-14) was able again to refer to historicism in a more positive manner. Furthermore, in one of his last articles, Schumpeter (1951) was to make an appeal for 'historical or institutional study' and 'detailed historical enquiry' to overcome the then growing and one-sided obsession with econometric techniques.

We are left with a conundrum. On the one hand, Schumpeter (1926, 1954) sometimes gave quite careful assessments of the historical school. On the other hand, in 1930-1 he dismissed both the historical school and institutionalism in acerbic and sweeping prose. He largely and generally ignored the historicist inspirations for his own work. These manoeuvres represent a major problem in

understanding Schumpeter's character. Clearly, Schumpeter did not always accommodate to prevailing opinion. He generally showed an independence of thought. His criticisms of the historical school would not have helped him secure the Berlin chair. He continued to praise Marx's theories, thus risking rightist hostility in the USA. He criticised Keynesianism when many other economists were on its bandwagon. Nevertheless, and in particular, the volte-face of 1926-30 and his unscholarly assessments of historicism and institutionalism in 1930-1 have to be explained. The most obvious explanation is that Schumpeter changed his stance and tone in part to please the Harvard faculty.

Crucially, it seems that contact with Harvard caused both Parsons and Schumpeter to change their published views on historicism and institutionalism. We are obliged to consider the unpalatable explanation that that their views were altered in part by career opportunism, although this is difficult to prove.

Both Parsons and Schumpeter emphasised the importance of general theory and neglected the problem of historical specificity. In the early 1930s, Parsons set to work on his general sociological theory. He obtained a grant from Harvard to work on what was to become his *Structure of Social Action*. When it was complete, Schumpeter gave this work a very positive evaluation for the university authorities (Swedberg, 1991, pp. 220-1).

Schumpeter - like Carl Menger, Max Weber and Lionel Robbins - accepted that the role of 'economic theory' was to take the individual as given. Schumpeter (1954, pp. 14-16) argued that economic theory was a 'box of tools' that applied to common features of all economic phenomena. Overall, the combined efforts of Parsons, Schumpeter and their allies at Harvard - including the graduate student and future Nobel Laureate Paul Samuelson - helped eventually to transform this academic institution into a crucial American bridgehead for the Mengerian conception of the nature and scope of economic theory, which hitherto had failed to gain an outright victory in the Germanic *Methodenstreit*.

Throughout his academic life, Schumpeter (1954, p. 827) regarded Léon Walras as the 'greatest of all economists'. There are neglected aspects of dynamism and entrepreneurship in Walras's thought (Currie and Steedman, 1990; Morishima and Catephores, 1988). Schumpeter wished to develop such insights. His move to the USA helped to promote further interest in Walras's work. However, as we shall see in chapter 15 below, Harvard also became the bridgehead of the bowdlerised version of Keynesian economics that established itself against Schumpeter's 'advice' - in the USA after 1936. Especially through the efforts of Samuelson, a strange and distorted synthesis of Walras and Keynes was born.

PARSONS'S ATTACK ON INSTITUTIONALISM

In the Harvard intellectual environment, Parsons eventually developed a direct attack on the institutional economics upon which he had been reared. Parsons (1935) criticised Veblen, largely for his dependence on the concepts of habit and instinct. Parsons was clearing the theoretical ground for his major work on *The Structure of Social Action*. In sweeping and unjustified phraseology, Parsons (1935,

p. 439) described Veblen's emphasis on 'habit' and 'institutions' as 'one element of psychological anti-intellectualism'. But Parsons failed to explain why an emphasis on habits is 'anti-intellectual'.

Parsons (1935, p. 440) also criticised Veblen's thought for its 'behavioristic stress on "objectivity" and its abhorrence of contact with the "subjective"'. Again this was off the mark. Here Parsons repeated the confusion in his Amherst undergraduate essay between Veblen's psychology and behaviourism. Unlike the behaviourists, Veblen emphasised the importance of intelligence and deliberation, as well as of instinct. For instance, Veblen (1919, p. 238) acknowledged that it is the 'element of discriminating forethought that distinguishes human conduct from brute behavior'. However, Veblen (1914, p. 6) also believed that 'it is only by the prompting of instinct that reflection and deliberation come to be so employed.' The notion that Veblen was a behaviourist who neglected subjective motives or perceptions is completely false. Many years later, Parsons (1976, p.178) gave a different account of his earlier objections to institutionalism:

I think, in retrospect, that I had two major theoretical objections to the institutional point of view. The first was that, in the name of generalized radical empiricism, it denied the legitimacy of analytical abstraction The second main objection was the neglect of cultural-normative factors in the larger picture which transcended the economic perspective.

Let us consider each of these two criticisms in turn. In the case of the first, there is a grain of truth here. It is true that there was an empiricist tendency in both institutionalism and the historical school. Wesley Mitchell, for instance, sometimes suggested that the way to build a theory was simply to gather facts (Ross, 1991, p. 321). However, no such 'name' as 'generalized radical empiricism' has ever been promoted in institutionalism. Furthermore, leading institutionalists such as Veblen and Commons clearly supported a prominent role for analytical abstraction. For instance, Veblen (1919, p. 176) had promoted the idea of economics as an 'evolutionary science with the preconception constantly underlying the inquiry' of 'a cumulative causal sequence'. Commons (1934a, p. 720) noted approvingly that 'Menger and Schmoller agreed not only that abstraction was necessary, but also that a great many abstractions were necessary in order to ascertain the whole truth.' Parsons's first criticism does not apply to the institutionalism of Veblen, Commons or many others.

Turning to Parsons's second criticism, it is blatantly untrue that institutional economists have neglected cultural and normative factors in the analysis of

⁶ To some considerable degree, however, the allegation was true of Ayres, who embraced behaviourist psychology and twice wrote - albeit with some qualification - that 'there is no such thing as an individual' (Ayres, 1918, p. 57; 1961, p. 175). Again we may point to the likelihood that Parsons's understanding of Veblen's thought was very much the questionable version that had been taught by Ayres. On some of the defects of Ayres's interpretation of Veblen see Hodgson (1998e).

TALCOTT PARSONS AND AHISTORICAL SOCIOLOGY

human agency and social structure. Indeed, Commons (1934a, p. 720) complained that Carl Menger had eliminated all 'ethical feelings' from economics. Veblen (1934, p. 30) argued that 'it is through . . . everyday approval or disapproval that any feature of the institutional structure is upheld or altered . . . these categories, with all the moral force with which they are charged, designate the motive force of cultural development.' Clearly, both Veblen and Commons recognised the normative and cultural aspects of all human action.

In short, most of Parsons's criticisms of institutionalism were untenable.' Not only were these criticisms off-target, but also Parsons fails to give credit to the influence of institutionalists in his own intellectual development. As Charles Camic (1991, p. xxiv) put it:

what appears, whenever his early writings speak of 'institutional economics,' is not an approving mention of the ideas of his Amherst mentors, but a critical attack upon the psychologism and biologism of the best-known forerunners of institutionalism, Thorstein Veblen and the quantitative economist Wesley Mitchell.

In attacking institutionalism, Parsons was not simply currying favour with his Harvard colleagues. He was also creating a space for his own brand of sociology (Camic, 1987). Parsons had to counter the institutionalist standpoint and its opposition to the compartmentalisation of the social sciences. Developments were to occur within neoclassical economics, which also would require Parsons's critical engagement.

Veblen and the pragmatists had seen habit as foundational for belief. In contrast, Parsons required a picture of the human agent that was relatively free of psychological and biological substrata (Camic, 1989). Parsons regarded habit not as a rooted propensity, but in phenomenal and behaviourist terms of stimulus and response. Hence, along with behaviourism, he rejected habit as well. Having removed habit from the picture, Parsons saw social norms and values as driving human agents and constituting the relations between them. Into the vacuum created by the removal of the pragmatist conception of action - based on habitual propensities and situated reason - Parsons was obliged to introduce an agent driven largely by values and norms. It became a 'top down' explanation of human agency in which normatively infused social structures did the explanatory work. In ridding sociology of habit, Parsons had denuded the concept of agency and conflated it into the concept of social structure. Contrary to Parsons, Veblen and the pragmatists had recognised the role of norms and values too, but they also tried to explain their grounding in shared habits of thought and behaviour. Parsons, in contrast, stressed the normative content of institutions over their role in shaping cognitive frameworks and habits of thought.

The banishment of the concept of habit from sociology was the key symptom of this disunion between the social and the natural (Camic, 1986; Murphy, 1994).

⁷ See Tilman (1992, pp. 170-4) for a discussion of some more of Parsons's criticisms of Veblen, mainly from the postwar period.

Although the concepts of habit and custom were once central to social science, largely because of Parsons and his followers, they have since been jettisoned. Symptomatically, the 1930 edition of the *International Encyclopedia of the Social Sciences* contains entries on habit and custom. In the next, 1968 edition they had been dropped. Where and when the concept of habit was retained, it was typically interpreted as mere behaviour, rather than a propensity or disposition in the sense of James, Dewey or Veblen.

After his direct attack on institutionalism in the 1930s, Parsons referred to this doctrine less often. An exception was in a footnote to his introduction to a translation of one of Weber's works. Here Parsons made the ludicrous claim that: 'Quite adequate comprehension of all Veblen's real contributions can be found in Weber's work' (Weber, 1947, p. 40 n.). Not only were the contributions of Veblen and Weber different in key respects, but also neither had any significant influence on the other. Having attacked institutionalism in the 1930s, by the 1940s Parsons was making the false claim that everything of relevance was in Weber and the institutionalists could be safely ignored.

A LITTLE PHILOSOPHY IS A DANGEROUS THING

Crucially, although Parsons was acquainted with ethical philosophy, he had little training in the philosophy of science. As a result, his use of philosophical terminology was often awkward and idiosyncratic. He read some of Dewey's works as an undergraduate, but his works show little evidence of the influence of pragmatism. He also read some of Immanuel Kant (Münch, 1981) but there is no more than limited evidence of a Kantian influence (Camic, 1987, p. 433). Despite his absorption of Weber, the traces of Kantianism in Parsons's own work are much less by comparison. At Heidelberg, Parsons read some political philosophy, philosophy of law, and philosophy of religion (Wearne, 1989, p. 44). At Harvard, Parsons came into contact with the British philosopher Alfred Whitehead, who had been there since 1924. This was his first detailed and sustained encounter with the philosophy of science. 'Prior to this point the philosophy of science occupied relatively little of Parsons's attention' (Camic, 1991, p. xxxiii). Partly under Whitehead's influence, terms such as 'organic' and 'emergence' made a confident-if sometimes confused-appearance in Parsons's work. Nevertheless, despite Whitehead, Parsons's general understanding of core philosophical issues remained patchy and incomplete.

As an example of Parsons's obscure and idiosyncratic use of philosophical terminology, consider his unusual definition of 'positivism'. Positivism was a term devised by Auguste Comte (1853) to express his view that all knowledge is based on the observation and comparison of data. The term 'logical positivism' was promoted in the 1920s by Rudolf Carnap, Moritz Schlick and others of the Vienna Circle. Like all versions of empiricism, positivism sees experience as the foundation of knowledge. Success in making prediction is regarded as a key attribute of a science. Positivism contends that observation and experiment are

the means of obtaining and testing knowledge. Unobservable entities - such as human consciousness or will - are typically dismissed as 'metaphysical'.

This established meaning of positivism had very little to do with the idiosyncratic definition of the word adopted by Parsons. Parsons (1935, p. 452) wrote: 'The "positivistic" theories tend on the whole to reduce economic activities to terms of biological heredity or the external environment or some combination of both.' A similar formulation appeared later when Parsons (1937a, 490) wrote of the 'positivistic sense of reducing social phenomena causally to terms of the nonhuman environment, as natural resources, or of biological heredity or some combination of both'.

Parsons thus identified positivism as a methodological, rather than an epistemological, doctrine. In particular, Parsons saw 'positivists' as those who would promote a kind of natural science of society. However, contrary to Parsons, positivism (as Comte defined it) does not lead to the idea that economic activities can be explained (partly or wholly) in terms of biological heredity. Such explanations play no significant role in the philosophical literature on either Comtean or logical positivism. Nor does biological reductionism necessarily involve positivism. In cavalier fashion, Parsons attached the term to an opinion that at the time he regarded as pernicious - the idea that biology or psychology can have a major part in the explanation of human behaviour. Parsons was concerned that the purposeful nature of human action should be recognised. But the proper name for a doctrine that denies the importance of purpose or intention is behaviourism, rather than positivism.

Elsewhere, however, Parsons (1937a, p. 63) seemed to have a different definition in mind. He saw the 'positivistic element' as consisting of 'the implication that ends must be taken as given'. This is very different from his other definition, and does not logically flow from it. Again, this is hopelessly remote from the standard use of the term by philosophers. Regrettably, Parsons's confused and philosophically illiterate use of the word 'positivism' has helped to render it virtually useless among social theorists - it now connotes an immense and confused variety of meanings.

Ironically, despite his hostility to 'positivism', Parsons's own work is suffused with positivism in the Comtean sense of the word. He wrote of his efforts 'to verify empirically' his ideas, he believed in the possibility of the 'empirical verification of the propositions' of a theory, he claimed 'empirical demonstration' of his own conclusions and he wrote more generally of the 'empirical proof' of propositions (Parsons, 1937a, pp. 11, 24, 698, 721). An attack on 'positivism', by someone who was unknowingly positivist to the core, can only succeed by redefining the term beyond recognition.

Another example of Parsons's twisted use of philosophical terms is his notion of 'empiricism'. Parsons (1937a, pp. 69-70) defined this as the claim that 'the categories of the given theoretical system are by themselves adequate to explain all the scientifically important facts about the body of concrete phenomena to which it is applied'. This is again hopelessly remote from the standard philosophical meaning of the empiricism, as the doctrine that knowledge is based primarily on experience rather on any body of theory. In attacking what he

described as 'empiricism', Parsons was opposing the idea that any one theory could explain everything. He argued that theoretical systems were limited because they were 'logically closed'. In making this point, Parsons was still able to allow truly empiricist notions - as noted in the preceding paragraph - to permeate his work. In the standard senses of these terms, Parsons's work was in fact both positivist and empiricist (Camic, 1987; Zaret, 1980) 8

THE QUEST FOR AN AHISTORICAL SOCIOLOGY

It was with such ill-defined terminology that Parsons attempted to dispense with the historical school position. But his critique of their central project was remarkably weak. His dismissal largely consisted of the assertion that such 'historical relativism' must inevitably lead to the vaguely defined sins of 'empiricism' or 'positivism' (Parsons, 1935). However, even with his own definitions of these terms, Parsons failed to show why this was necessarily the case.

In his promulgation of Weber's ideas among an English-speaking audience, Parsons downplayed Weber's engagement with the problem of historical specificity. Instead he emphasised Weber's valid recognition of the indispensability of universal concepts in any theoretical system. Parsons (1936, pp. 677-8) noted that Weber had shown

the logical indispensability, for empirical demonstration, of *general* concepts - both the general 'elements,' into which the 'historical individual' may be resolved, and the general laws relative to the behavior of these elements. The corollary of this is that the methodology of the social sciences cannot be confined to the 'genetic' tracing of temporal sequences.

So far so good. On this point, both Weber and Parsons had a strong case. But Parsons took his argument much further than Weber. Parsons continued: 'Different historical individuals must be capable of analysis into different combinations of the *same* elements.'

There is a key ambiguity here. If Parsons had meant that different analyses of different historical entities must involve *some* similar elements then he would have been on strong ground. However, if he had meant that all such different historical entities must be capable of analysis *entirely* in terms of 'different

8 As another example of Parsons's idiosyncratic use of philosophical terminology, consider the term 'emergence'. Although he used it, Parsons never clearly defined the concept. In a key passage in the *Structure of Social Action* where he came closest to a definition, he saw emergent properties as a measure of the organicism of the system (Parsons, 1937a, p. 749). However, Parsons's notion of emergence as a 'measure' has nothing to do with the concept as it is developed in the works of Whitehead, Lloyd Morgan, William McDougall and others. Their concept of emergence concerns the existence of properties of an entity at one ontological level, which are not reducible to or predictable from the lower-level properties of its components.

combinations of the *same* elements' then it would have been a non sequitur. It simply does not follow from his preceding argument. Weber's valid argument that some general concepts are unavoidable does not mean that all concepts must be general in their scope. Such crucial ambiguities helped Parsons to act as Weber's literary executor in the English-speaking world, while abandoning the problem of historical specificity at the same time.

Parsons's intentions were clear. The social sciences must address 'unique historical' cases but only as a 'means' towards the erection of a general theory (Parsons, 1936, p. 678). His ultimate search was for 'a substantial common basis of theory' that would serve a wide variety of circumstances (Parsons, 1937a, p. 774). As Camic (1991, p. li) puts it, in his 1936 discussion of Weber, Parsons ended up

entirely abandoning his earlier institutionalist emphasis on the *differences* between historical epochs, the value of the genetic method, and the limitations of abstract theories with universal applicability, Parsons now - in a fateful step that will inhibit his treatment of historical diversity hereafter - champions the search for uniform 'general laws'.

This 'fateful step' meant that Parsons was to break without acknowledgement from the letter and spirit of Weber's work. As Weber had recognised, there is nothing wrong with a judicious search for general principles, as long as we do not lose sight of the additional need to conceptualise historically sensitive levels of analysis. But much of Parsonian and post-Parsonian sociology has overlooked the latter point. Parsons overlooked Weber's strictures on the limits of general theorising. Parsons degraded the historically sensitive approaches of Weber and others into a scheme where 'the historical dimension of social life becomes a fund of empirical data to be used for testing general theoretical propositions' (Zaret, 1980, p.1198). A similarly relegated role for history can be found in much of post-1945 social science. However, in striking out for a general sociological theory in the 1930s, Parsons was taking a huge risk. At that time, general theories were not that popular in the social sciences. In this ambitious project, Parsons gained confidence from his reading of Pareto and the crucial support of Schumpeter and others at Harvard. Fortunately for Parsons, however, developments were to take place in economics that made general theorising much more fashionable. Among others, Lionel Robbins played a crucial part.

PARSONS AND LIONEL ROBBINS

In 1932 Robbins - a young Professor at the London School of Economics - published his seminal *Essay on the Nature and Significance of Economic Science*. Greater attention is given to Robbins's argument in the next chapter. We may note briefly here that it involved a major attempt to dismiss the problem of historical specificity. Robbins redefined economics as the universal 'science

of choice'. Economics was about the rational choice of means to serve given ends. The 'economic problem' was then to determine the best means available to meet those given ends. It applied to all economic systems, as long as there were choices to be made amid a scarcity of resources.

Crucially, Parsons did not reject this redefinition of economics. In fact, it served his purposes. By defining economics narrowly, as the science of rational choice, Robbins conceded a substantial territory to the sociologist. For Parsons, sociology was about the social and normative origin of the ends that Robbins had taken as given.

Parsons raised two criticisms of Robbins. First, for Parsons, ends and means could not entirely be separated. In addition, ends could not always be taken as 'given because they were likely to be affected by the processes involved in their attainment. Parsons (1934, p. 515) wrote: Since they contain a future reference, in the sense of being data of the physical world, they can, unlike the other elements of action, only become "given" after its completion.' This idea of the mutual interaction of ends and means was redolent of Dewey (1922, p. 34) and others. Parsons's point was sound, but not original.

Second, Parsons stressed that social action was always framed and driven by social and institutional norms. Allegedly, these normative patterns 'define what are felt to be, in the given society, proper, legitimate, or expected modes of action or of social relationship' (Parsons, 1940, p. 190). As noted above, Parsons was impelled to put such stress on the determination of meaning and behaviour by social norms because he had removed from sociology any instinctive or psychological grounding. For Parsons (1934, p. 533) the 'system of normative rules' embodied in institutions were a 'specifically non-economic factor'. The Parsonian emphasis on norms was the keystone of his autonomous sociology.⁹

In reaction against Robbins, Parsons feared that the emphasis of the English economist on facts and logical relationships, to the exclusion of ends, might lead to a neglect of the role of norms in all social activity. Parsons (1934, pp. 520-1) insisted that the idea of a 'logical gap' between ends and means should not be used to sustain 'the idea that a science dealing with "facts" cannot also deal with "norms"'. Later, Parsons (1940) extended this argument by insisting that self-interest was itself socially formed. Historical school and institutional economists had asserted the same proposition, time and time again. But Parsons gave them no acknowledgement: he took for sociology the wisdom that he had learned from institutionalism.

Parsons's tactic was to show that Robbinsian economics had to be grounded upon a general sociological theory. Economics would focus merely on the examination of the logical relationships between means and given ends.

⁹ Note that Schumpeter (1954, pp. 15, 20-1, 819) endorsed a similar but not identical demarcation between the two disciplines. His definition of 'economic theory' or 'economic analysis' was narrow and similar to that of Robbins, consigning the 'analysis of economic institutions' to economic sociology'. For instance, Schumpeter (1954, p. 21) wrote: 'economic analysis deals with the questions how people behave at any time and what the economic effects are they produce by so behaving; economic sociology deals with the question how they came to behave as they do.'

Sociology would then assume its place as the study of the social origin of the ends. Hence, Parsons (1937a, p. 768) defined sociology as 'the science which attempts to develop an analytical theory of social action systems in so far as these systems can be understood in terms of the property of common-value integrations'. Notably, this definition of the subject was not in terms of the analysis of 'social action systems' as a whole, but in terms of the impact and integration of common values. Sociology was thus defined as the study of an aspect of the social system. It had a delineated domain of enquiry. The study of other features was conceded to economists and others.

An implicit contract emerged between them. Economics was henceforth to concern itself with the rational choice of means to serve given ends; sociology was to be concerned with the explanation of those values and ends. This was the substance of their 'gentleman's agreement' mapped out in the 1930s (Ingham, 1996, p. 244). This agreement tacitly involved the severance of the social sciences from biology and to some degree also from psychology. Accordingly, philosophical questions such as the relationship between causality in the natural sciences and causality in the social sciences were ignored. With Robbins (1932), economics became the 'science of choice' without much consideration of what 'choice' actually meant in philosophical terms. And under Parsons (1937a, p. 768) sociology was reconstructed as 'the science . . . of social actions without much discussion of the materialist causes behind intention or action itself.'

Crucially, both Parsons and Robbins avoided any direct and integrated analysis of socio-economic structures and institutions as a whole. Each of them focused on a selected analytical aspect. Neither addressed the structured reality in its totality. Their example became tragically influential for the two subjects involved. Both economics and sociology became redefined in terms of the study of types of analytical problem rather than in terms of the explanation of a distinct reality. They became compartmentalised, self-reflective discourses. After Parsons and Robbins, no social science addressed the study of socio-economic systems as a whole.

PARSONS'S LIBERATION STRUGGLE AGAINST 'ECONOMIC IMPERIALISM'

However, the Robbins-Parsons attempt to redraw the boundaries of economics and sociology received a major challenge. This was from a brilliant young heterodox economist, who has since remained largely unknown and unrecognised.

Ralph W. Souter was born in New Zealand and fought in the First World War. A Rockefeller Foundation travelling fellowship brought him to Columbia University in 1928. At Columbia he obtained a PhD, and was a lecturer in economics from 1930 to 1935. In 1936 he obtained a chair in economics at the University of Otago, New Zealand, a position he held until his early death in 1946, at the age of 49.

¹⁰ Although in the 1950s Parsons became interested in psycho-analysis, he never repaired the breach between psychology and sociology.

Souter was heavily influenced by Whitehead's organicism and by Gestalt psychology. He rejected atomistic assumptions in economics in favour of an organicist ontology. He believed in a synthesis of a type of Marshallian 'evolutionary economics' (Souter, 1930, p. 59) and the best of institutionalism. His Phi dissertation is an innovative and outstanding volume, acutely critical, philosophically well informed, and far ahead of its time (Souter, 1933b). For instance, he acknowledged the origin and significance of the concept of emergent properties and brought it into his own work. Also, decades before Nicholas Georgescu-Roegen (1971), Souter (1933b, pp. 116-18) brought the entropy law into economic theory.¹¹ As discussed in more detail in the next chapter, Souter attacked the idea that the social sciences should be compartmentalised. He argued for the integration, rather than the compartmentalisation, of the social sciences. The 'organic' nature of social reality required this. Such a standpoint put him into conflict with both Parsons and Robbins, each wishing to create space and autonomy for their own separate academic discipline. Souter's (1933a) review of Robbins's *Nature and Significance of Economic Science* is discussed in the next chapter. In his PhD dissertation, Souter (1933b, p. 94 n.) argued: The salvation of Economic Science in the twentieth century lies in an enlightened and democratic 'economic imperialism', which invades the territories of its neighbours, not to enslave them or to swallow them up, but to aid and enrich them and promote their autonomous growth in the very process of aiding and enriching itself. This is the first known use of the term 'economic imperialism' to describe an attempt to link up economics with the other social sciences. Both historicists and institutionalists had previously promoted notions of a unified social science. Souter argued for such a unification, in which a form of 'evolutionary economics' would be at its apex. At the same time, Souter insisted that the imperial science should enrich rather than subjugate, and even depend upon other disciplines. For example, Souter (1933a, p. 399) wrote: 'Economics is necessarily and inevitably dependent upon sociology, upon psychology, upon technology.' Notably, this early use of the term 'economic imperialism' contrasts greatly with the works of 'Chicago' economists Gary Becker, Jack Hirshleifer and others, with which it is associated today (Becker, 1976a; Hirshleifer, 1977, 1985; Lazear, 2000). Modern, Chicago-type 'economic imperialism' involves the swallowing up of other disciplines. Professedly, all social sciences face the common 'economic' problem of competition over scarce resources. Accordingly, they must

¹¹ It is clear from both the title and content of Souter's 1933 book that he made extensive use of ideas and metaphors from modern physics. Indeed, it was one of the very few cases of a discussion of Einstein's theory of relativity in the context of economics. The fact that Souter's work was subsequently forgotten seems to counter an extreme version of Mirowski's (1989) argument—that economists have achieved recognition by aping physics at its every turn. This does not seem to be the case. The connection was less frequent and more remote.

all be enslaved by the precepts of a Robbins-type economic science. The following statement by Hirshleifer (1977, pp. 3-4) is representative of this approach:

the domain of economics is coextensive with the total sphere of all the social sciences together . . . As economics 'imperialistically' employs its tools of analysis over a wide range of social issues, it will *become* sociology and anthropology and political science. But correspondingly, as these other disciplines grow increasingly rigorous, they will not merely resemble but will *be* economics.

This is very different from the 'economic imperialism' proposed by Souter in 1933. Hirshleifer proposed the widespread use of economic tools. In contrast, Souter proposed an economics that was conjoined with insights from other social sciences. However, despite this huge difference, both forms of 'economic imperialism' challenged the Parsonian project for a general and autonomous sociological theory.

In his discussion of Robbins's (1932) *Essay*, Parsons engaged with Souter's (1933a) review. Significantly, for Parsons, the greater threat came not from Robbins but from Souter. Parsons's (1934, p. 522) rejected Souter's 'economic imperialism' vehemently, in the same way that he had opposed the idea of unified social science that had been frequently proposed by institutionalists and historicists. For Parsons (1934, p. 535), Souter's 'economic imperialism' was a tendency 'against which the sociologist, as well as other scientists, must stand up and fight for his scientific life'. But Parsons gave little detailed justification for this argument. His response to Souter was opaque and unconvincing.

Souter's post-Marshallian and evolutionary attempt to build a unified social science faced the twin opposition of a leading sociologist and leading economist. However, Souter failed to make an impact. His work is almost completely forgotten.

PARSONS AND ADOLPH LOWE

In the 1930s and 1940s, thousands of refugees from Nazi persecution fled to the United States (Scherer, 2000). Some of these immigrants brought the ideas of the German historical school to America. One of them was Adolph Löwe. He was influenced by the historical school and wrote a major review of one of Sombart's works (Löwe, 1932). Löwe emigrated to England in 1933 and remained at the University of Manchester until 1940, when he moved on to the United States. In his 1935 book *Economics and Sociology: A Plea for Cooperation in the Social Sciences*, Löwe argued, like many before, that capitalism was only one among several types of economic system, which differed in terms of 'historical structure and historical laws'. Nevertheless, reflecting the work of Weber, Sombart and others, Löwe also saw a place for 'pure theory' addressing universal aspects of all economic systems. It was the role of sociology, according to Löwe, to furnish 'middle principles' to connect 'pure theory' with 'the structure and the regularities of motion of particular "economic systems"' (Löwe, 1935, p. 136).

HOW ECONOMICS FORGOT HISTORY

Regrettably, the substance of Lówe's argument did not get all the attention it deserved. Parsons (1937b) reviewed it in the *American Journal of Sociology*. Two features are notable in this assessment. The first is its concentration on the 'logical lack of symmetry between the two sciences of economics and sociology' in Lówe's scheme. This greatly offended Parsons's attempts to demarcate separate realms for the two sciences, in which each science dealt with 'aspects' of the same phenomenon. Second, is the remarkable neglect in Parsons's review of the theoretical problem of historical specificity, which Lówe had made central to his argument. This too interfered with Parsons's project to establish universal principles for sociology to parallel those already proclaimed by economists.

Lówe, a refugee from Germany, brought with him ideas from a great tradition of economic thought. In Germany this tradition was being hampered by Nazi

persecution and crushed by Allied bombs. Any presentation of these ideas to the English-speaking world was potentially of great significance for the future. Unfortunately for Lówe, his ideas did not conform to the rising orthodoxies in economics and sociology. Hence, the legacy was almost completely lost to the post-1945 world. The injurious role of Parsons in this episode was significant.

CONCLUDING REMARKS

In 1937 Parsons published his monumental *Structure of Social Action*. Although it was largely a history of ideas - focusing principally on Marshall, Pareto, Durkheim and Weber - it established Parsons's international reputation. The boundaries between economics and sociology, so carefully drawn by Robbins and Parsons, were widely accepted from the 1940s to the 1970s. The postwar consensus in the social sciences was thus established.

Neither the historical school nor the institutionalists were part of this consensus. Eventually, institutionalists such as Veblen were denied a place in the halls of fame of sociology as well as of economics, thanks to the combined efforts of Parsons, Robbins, Schumpeter and others. Yet Parsons owed much of his own intellectual development to institutionalists, notably to Hamilton and Ayres. As Camic (1987, 1992) has argued at length, Parsons's exclusion of the institutionalists when naming his intellectual predecessors in his books was strongly influenced by their generally faltering reputation and their particularly low standing at Harvard.

Parsons never denied the variation and qualitative historical development of societies. Indeed, much of his postwar work was concerned with these issues (Parsons, 1951, 1966, 1977). But he always treated social change and variation within one, over-arching framework of analysis. The overall thrust of his work was a search for general concepts and principles that pervade all history.¹²

¹² In this project he developed the opaque and cumbersome style of writing that regrettably has been imitated by succeeding generations of sociologists. Mills (1959, p. 40) made the plausible claim that 'one could translate the 555 pages of *The Social System* into about 150 pages of

In his 1949 presidential address to the American Sociological Society, Parsons (1950, p. 5) argued for '[g]eneral theory, which I interpret primarily as the theory of the social system, in its sociologically relevant aspects'. For Parsons, there were no longer different social systems, but simply *the* social system. This is a remarkable statement for someone so familiar with the historical school and institutional economics. Although Parsons admitted '[s]pecial theories around particular empirical problem areas' these were seen as a minor and highly subordinate areas, driven by empirical data rather than by theoretical problems. Parsons explained:

The basic reason why general theory is so important is that the cumulative development of knowledge in a scientific field is a function of the degree of *generality of implications* by which it is possible to relate findings, interpretations, and hypotheses on different levels and in different specific empirical fields to each other.
(ibid.)

The logic here is both opaque and questionable. Although science often advances by unification and generalisation, knowledge also cumulates by particular knowledge of specific phenomena. Furthermore, in arguing for a general theory, Parsons repeatedly fudged an important issue. He made a strong case that some general frameworks and principles were both necessary and unavoidable for any social enquiry. But he then excluded the possibility that particular theories are also required to explain particular types of socio-economic system. This historical school insight was simply bulldozed under a general argument for general theory, ignoring the fact that special theories are required as well.

As a further example, consider his concept of 'evolutionary universals'. Parsons (1964) proposed that in evolutionary processes, specific organisational developments have 'generalized adaptive capacity'. Just as the eye evolved independently in different forms in different species, effective and durable organisational forms will likewise appear in different circumstances. Parsons (1977) believed that political democracy based on universal adult suffrage was such a 'evolutionary universal'. Another was 'institutionalized individualism'. All this began to sound like a celebration of the values of postwar American society. His ahistorical sociology looked at history but it then returned to the American present, as if to declare 'the end of history'. The 'social system of Parsonian sociology appeared more and more like an Americanised 'ideal type'.

By the 1950s, Parsons had become the most highly regarded living sociologist in the world. In an ironic crossing of paths, he visited Cambridge, England in 1953 and delivered the prestigious annual Marshall lectures in Faculty of Economics. However, there was none of Marshall's sensitivity to the problem of historical specificity. The first three chapters of *Economy and Society: A Study in the Integration of Economic and Social Theory* (Parsons and Smelser, 1956) were based on these lectures. The book attempted to show that theories attributed to economists as diverse as James Duesenberry, John Hicks, Michal Kalecki, John Maynard Keynes and Paul Samuelson could all be placed within a single, general

theoretical framework. Hence his sociology victoriously encompassed all of economic. The Parsonian analysis thus claimed to be more general than the *General Theory*. Parsons outbid both Keynes and the Keynesians in the auction of generalising claims. However, Parsons and Smelser failed to persuade many economists of the benefits of their proposed sociological imperialism.¹³ Among many sociologists, Parsons had much greater success. He established a universal and ahistorical mode of sociological theory that was widely followed. In this tradition, the historical specificity of particular social relations was overlooked. For example, the influential 'exchange theory' of George Homans (1961) and Peter Blau (1964) proposed that a wide range of activities - including gift-giving and interpersonal communications - are 'exchanges'. This universal concept of exchange obscured its specific, contractual form in a market society. Mere verbal conversation does not necessarily involve a contract. As Commons (1924) and other institutionalists had insisted, exchange in a market economy involves the contractual exchange of property rights within a legal system of private property relations.¹⁴

Today, sociological theory is in a state of crisis. A consensus over core concepts and approaches is lacking. Parsons fell out of favour in the 1980s, only to be replaced by a discordant cacophony of post-modernists, empiricists and others. Indeed, one of the principal dangers is to conclude, from the failure of the attempt of Parsons to establish a general sociological theory, that all general theorising is doomed. John Holmwood (1996, p. viii) expressed this mood: 'My basic contention is that Parsons's general theory is fundamentally flawed and, indeed, that the very programme of a general theory in sociology is mistaken.' However, the solution to this malaise is not to abandon entirely any general theory. Such a venture would be both vulnerable and internally inconsistent: the notion that a general theory has to be abandoned is itself a general theory. Any argument for such an abandonment would itself require a general meta-theory. There is no way to escape from this contradiction, without accepting that some kind of general theory must be retained, alongside a more specific, historically-oriented discourse.

Instead of concluding that all general theorising must be abandoned, it would be better to learn the vital and more specific lessons from Parsons's recasting of sociology in the 1930s. Such lessons have major implications for all subsequent sociological theory. Among them is the need to reconcile the more general with the historically confined types of analysis. Parsons himself discarded this project.

¹³ See the highly critical reviews of Parsons and Smelser (1956) by Ayres (1957), Boulding (1958) and Worswick (1957), and the (negative) remarks in Johnson and Johnson (1978, p. 155).

In his review, Ayres made no mention of the fact that Parsons was formerly his student. ¹⁴ Blau (1964, p. 93) made a distinction between 'social' and 'economic' exchange, where the latter is based on a 'formal contract that stipulates the exact quantities to be exchanged'. However, many business transactions in the real world do not involve such an exact specification. This is especially the case with the employment contract, which in general is imperfectly and incompletely specified. Blau passed over this problem, seemingly content to place such business and employment issues outside 'the economy'. This is just one case of the recurring failure within the social sciences to

demarcate adequately the boundary between 'economics' and 'sociology'. This should lead us to question whether such a boundary is possible or necessary.

With the help of others he consigned the efforts of historicists and institutionalists who were working on the problem, to the dustbins of postwar social science. As we have seen, one of his abettors was Robbins. He is one of the two main subjects of the next chapter. More than anyone else, Robbins and Parsons are responsible for the wrong turnings taken by economics and sociology in the 1930s.

14

DEATH AND COUNTER-REVOLUTION AT THE LONDON SCHOOL OF ECONOMICS

Truth is the most valuable thing we have. Let us economize it.
(Mark Twain, *Following the Equator* (1897))

Again we cross the Atlantic, to observe in the next two chapters some key events in the development of economics in Britain. The outcome, as in America in the 1930s, was that the heritage of the historical school, and the project to develop more historically sensitive theories, would be lost for the remainder of the twentieth century.

These two chapters are centred respectively on the London School of Economics (LSE) and the University of Cambridge. In the case of the LSE, there was an open critical engagement with the historical and institutional economists. The victorious critics captured the LSE positions of power. In the case of Cambridge, its culture of intellectual isolationism did not encourage the recognition or appropriation of a historicist and institutional legacy from elsewhere. Consequently, after Alfred Marshall's retirement, the problem of historical specificity was largely forgotten. As a result, the general theorists were eventually able to extricate victory from the jaws of their Keynesian defeat. This tale is told in the next chapter. Notably, despite the heterodox reputation gained by Cambridge from the 1930s to the 1980s, conditions at the LSE were originally more fertile for a British institutionalism.

EDWIN CANNAN AND ALLYN YOUNG

The towering influence over British economics, for at least the first half of the twentieth century, was Alfred Marshall. As argued in chapter 8, he praised the German historical school and recognised the problem of historical specificity. Although he was to be remembered for his neoclassical approach to economic theory, his conception of economic analysis could also readily accommodate many historical and institutional ideas.

In 1895 - five years after the appearance of Marshall's *Principles* - the London School of Economics was founded.¹ As founders of the School, Beatrice and

¹ For the early history of the LSE see Beveridge (1960), Dahrendorf (1995), Hayek (1946) and Winch (1969).

Sidney Webb had tried to ensure that economics at the LSE would be empirically grounded and practically oriented. They strongly supported inductivist methods in science and sided with the historicists. Their academic appointments reflected this policy. William A. S. Hewins was appointed as first Director of the LSE. Educated at Oxford, Hewins was also influenced by the German historical school, although he later distanced himself from it (Koot, 1987, ch. 8).

The correspondence in 1899 and 1900 between Marshall and Hewins has been noted in chapter 8 above (Whitaker, 1996, vol. 2, pp. 256, 280). Marshall advised Hewins that the economic curriculum at the School should not be overly formal or mathematical. Above all, Marshall insisted, 'economic science' was to be 'the application of powerful analytical methods to unravelling the actions of economic and social causes'. Under these injunctions, a more methodologically aware and theoretically astute institutional economics might have grown and prospered.

Another early and influential appointment was Edwin Cannan. At the foundation of the LSE, he was appointed as a lecturer. In 1907 he became a professor of economic science. Cannan emphasised the institutional foundation of economic systems and recommended that economists should also study law. In his monograph on *Wealth*, Cannan (1914) devoted a chapter to what he regarded as the fundamental institutions of the capitalist system, particularly stressing three elements: the family, private property and the state.

Another prominent figure was Harold Laski, who moved to the LSE from Harvard University in 1920. 'Laski knew Veblen personally and greatly admired much of his work' (Tilman, 1996, p. 133 n.). This created a further link between the LSE and the institutionalist and historicist traditions.

When Cannan retired in 1926, the search for his replacement reached across the Atlantic to Allyn Young. The Director of the LSE, William (later Lord) Beveridge visited Harvard and Young agreed to take the job (Beveridge, 1960, p. 91). Young became the first American professor in Britain.

When Veblen was at Stanford University (1906-9), Young was the chair of the economics department. He had been one of the very few to defend Veblen when he was fired as a result of allegations of marital infidelity from his estranged wife Ellen. Indeed, Young had testified that Veblen was 'the most gifted man whom I [Young] have ever known' (Dorfman, 1934, p. 299).

In his first major published article, Young (1911) argued that economics should turn away from equilibrium-oriented theorising. He warned against oversimplistic, all-embracing theories. He accepted the institutionalist argument that human motivations were moulded by historically contingent institutions. In particular, he endorsed Wesley Mitchell's thesis that 'the money concept itself has been an active factor in giving purpose, system and rationality to economic activity' (Young, 1911, p. 415). We have already noted that Young was one of the supervisors of Frank Knight's seminal PhD dissertation. Young's reputation quickly grew to the point that he became president of the American Economic Association in 1925.

While he remained sympathetic to institutionalism, Young (1927, 1929) began to be critical of its failure to build up a systematic theoretical approach. Like some other leading institutionalists - notably John Commons, Wesley Mitchell, Frank

Knight, John Maurice Clark, Paul Douglas and Arthur F. Burns - Young saw institutionalism as compatible with a Marshallian type of price theory. However, his interpretation of Marshallian economics was even more dynamic than that of Marshall himself, emphasising the phenomenon of increasing returns that had been consigned an appendix of the *Principles*.

Young was sufficiently immersed in the tradition of historicism and institutionalism to acknowledge the problem of historical specificity in economic theory. Soon after his arrival at the LSE, Young (1927, pp. 6-7) wrote that:

in the social sciences we must make room for two different general classes or types of investigation. In the first type we concern ourselves with certain aspects of the nature and the operations of a complicated social mechanism. We search for uniform and dependable relations that will help to explain the degree of order that is apparent in our social environment. In the second type of inquiries we seek to get an understanding, not of those general and dependable relations among things which we call 'laws,' but of specific events, particular institutions, and unique situations. We look for explanations of *differences*, of the new forms which our institutions and our activities assume from time to time.

Accordingly, Young recognised a place for both general 'laws' and particular explanations of different 'institutions and activities' in economic theory. A year later Young published in the *Economic Journal* his famous article on 'Increasing Returns and Economic Progress'. This work was a continuation of Young's attack on the equilibrium tradition in economic theory. He believed that increasing returns were pervasive in modern, manufacturing economies. Consequently, Young (1928, p. 533) explained in distinctly Veblenian language, 'change becomes progressive and propagates itself in a cumulative way.' The main function of markets, argued Young in a distant echo of Adam Smith, is not merely to allocate but to create more resources by enlarging the scope for specialisation and the division of labour.²

Nicholas Kaldor was a student of Young at the LSE. Kaldor saw the obsession with equilibrium and the failure to recognise increasing returns as a crucial weakness of economic theory. Kaldor moved to Cambridge in the 1940s, where he was to marry the theoretical systems of Young and Keynes (Toner, 1999). Young was thus indirectly to make a vital mark on the Cambridge economic of the future.

Tragically, Young (like Max Weber in 1920) became a victim of a severe influenza epidemic, and died in London of pneumonia in March 1929. He was just 52 years of age. This American's death was to be decisive for the fate of British economics.

² For a useful exposition and discussion of Young's contribution to economic theory see Blitch (1995).

ENTER LIONEL ROBBINS

In 1929, Lionel Robbins was elected to the LSE chair that had been vacated by Young. This youthful appointee was to steer LSE economics in a very different direction. Immediately, Robbins set about the task of ridding it of its institutionalist and historicist ballast. His famous *Essay* was published in 1932. In a masterly stroke, he simply redefined economics in terms that would exclude institutionalism and the historicism from within its disciplinary boundaries. Economics was to be the general 'science of choice', but it would exclude any investigation into the psychological origins or institutional moulding of individual preferences or goals. Economics was no longer to have an institutionally or historically specific domain of analysis.

To achieve this transformation of the subject, institutionalism and historicism had to be thrown overboard. But Robbins wanted to retain a place on board for the Austrian school, largely because of Menger's similar redefinition of the scope of economics. Hence Menger's stand in the *Methodenstreit* was vital ammunition against the German historicists and support for Robbins's claims. However, what Robbins retained within economics alongside neoclassicism was just one strain of 'Austrian' theory. As Anthony Endres (1997) has shown, 'Austrians' such as Friedrich von Wieser and Eugen von Böhm-Bawerk saw it as appropriate for economists to explore the formation of preferences, using insights from psychology and elsewhere. In fact, the Austrian school was not as narrow as Robbins contrived.

Robbins redrew the boundaries of the subject in a way that violated both of the broader Austrian and neoclassical traditions. Included within both these streams of thought were ideas and problematics that Robbins wished to place outside economics. Above all, leading neoclassical economists such as Marshall in Britain and John Bates Clark in America would not have subscribed to such a narrow definition of their discipline.

To succeed in defining institutionalism out of the discipline of economics, Robbins had to establish a new lineage for his ideas. To a significant degree, this had to draw upon the existing intellectual powerhouses of Britain, Germany, Austria and America. In Britain, Marshall could be cited, but only if his admiration for the German historical school could be overlooked. As noted in chapter 8 above, Robbins conveniently ignored Marshall's attention to the problem of historical specificity. Robbins managed to rewrite history, and to make Marshall his ally.

Robbins found a suitable emblem in the pioneering neoclassical economist Philip Wicksteed, and he edited his *Commonsense of Political Economy* (1933). Wicksteed defined an 'economic transaction' as one in which one party did not consider the welfare or desires of the other, merely the object of the transaction itself. This attempted narrowing of the domain of the 'economic' served Robbins's purposes.

The choice in Continental Europe was clear: Robbins chose the Austrian side in the German-speaking *Methodenstreit*. America, with its strong institutionalist tradition, was more of a problem. Robbins had to identify a leading American

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HOW ECONOMICS FORGOT HISTORY

economist who seemed sufficiently close to him. But institutionalists dominated in that country. Among the well-known American economists, one of the best options was Frank Knight, but only if he could be repackaged as a neoclassical economist and his institutionalist sympathies could be obscured or forgotten.

Robbins thus contrived an Austro-neoclassical tradition, from Carl Menger through Philip Wicksteed to Frank Knight. This created a splendid Germanic-Anglo-American triad for Robbins's project. However, it ignored Knight's sympathies for institutionalism and the historical school, and his repeated view that 'utility is misleading as an explanation of economic behavior' (Knight, 1921b, p. 145). Furthermore, Knight was personally very uneasy about the role that Robbins foisted upon him. For instance, he rejected Robbins's insinuation that he had built upon the work of Wicksteed. He insisted in 1934: 'I never read the "Common Sense" until recently' (Knight, 1956, p. 104). When Knight reviewed Robbins's *Theory of Economic Policy* (1952) he objected to the 'pervasive *ad hominem* type of argument' (Knight, 1953, p. 290) in the book. In a reaction from Robbins's (1952, p. 40) depiction of the 'degrading mystique of historicism', Knight (1953, p. 280) jumped to the defence of the historical school: 'I must say that there is a vast amount of truth in historicism, and also that it affords a sorely needed corrective to the naive utilitarian individualism of the English Classical economists.'

Despite Knight's remarks, Robbins eventually won the battle of ideas, and economics today follows more closely his methodological guidelines than those of Knight. The irony is that much of Knight's work would be excluded from the narrow definition of economics that Robbins established in the 1930s.

THE NATURE AND SIGNIFICANCE OF ECONOMIC SCIENCE

The basic thrust of Robbins's redefinition of economics is familiar to any student of the subject. What the student is unlikely to be told, however, is that this redefinition required the banishment of the two dominant and prestigious intellectual traditions, in Germany and America, to beyond the boundaries of the discipline.

In his 1932 *Essay*, Robbins criticised institutionalism and the German historical school - for failing to discover general economic laws. Robbins (1932, p. 114) wrote: 'not one single "law" deserving of the name, not one quantitative generalisation of permanent validity has emerged from their efforts.' Yet many institutionalists would be reluctant to admit that many 'generalisations of permanent validity' exist, due to the manifestly varied and changing nature of socio-economic reality. In doubting their existence, they should not be condemned outright for failing to discover them.

For Robbins, the economic problem was one of the allocation of scarce means in the pursuit of given ends. Individuals are assumed to have given utility functions and they exchange resources with each other to maximise their own utility. Such a framework universalises the concepts of 'exchange' and 'price'. It

is purported that a wide range of social and economic phenomena, in all types of present, past and future economy, can be analysed in these terms, as long as they are afflicted with the seemingly ubiquitous problem of 'scarcity'. As Robbins (1932, p. 20) himself put it: 'The generalisations of the theory of value are as applicable to the behaviour of isolated man or the executive authority of a communist society, as to the behaviour of man in an exchange economy'. All differences between these systems are 'subsidiary to the main fact of scarcity'.

Robbins saw economics as the a *priori* exploration of deductions from the axioms of rational choice. In the old debate between induction and deduction, he came down on the side of the latter. He made grand claims for the scope of deductive economics: it would apply directly to the pressing problems of economic depression and unemployment. As the world was entering the Great Depression, Robbins (1932, pp. 104-5) conjectured optimistically in the first edition of his Essay that 'the despised apparatus of deductive theory' will probably provide 'a complete solution of the riddle of depressions within the next few years'. He was clearly hopeful that a deductive general theory could solve the most pressing economic problems of his time. However, a short while later, it became clear to Robbins that this promise could not be delivered. This passage was removed from the second edition of 1935.

Although Robbins's book was not immediately an outstanding success, it eventually had a huge impact. When Paul Samuelson (1947, 1948) re-laid the foundations of postwar neoclassical economics and published his best-selling textbook, Robbins's definition of economics was adopted. The battle against historicism and institutionalism had been won - but more by act of definition than by force of theoretical argument or achievement.

RALPH SOUTER'S CRITIQUE

Ralph Souter's confrontations with both Talcott Parsons and Lionel Robbins have been mentioned in the preceding chapter. His critique of Robbins is discussed in more detail here. Contrary to Parsons and Robbins, Souter insisted that sharp boundaries between economics and the other social sciences could not be drawn. Referring to Robbins, Souter (1933b, p. 38-9) wrote:

The idea seems to be that, after an initial and final taking over of an elementary modicum of alleged psychological or other facts from those neighbouring sciences, the economist can therefore proceed with his task in magnificent isolation. And this, it would seem, is possible because he has thereby equipped himself with at least the *general forms* of the demand and supply functions which he thereafter devotes himself to manipulating [But] it is quite impossible to seek to escape the indefinite and progressive inter-penetration of the 'boundaries' of economics into the 'territories' of all the neighbouring social sciences by alleging that the general forms of the demand and supply functions are ascertainable without such exhaustive investigations.

HOW ECONOMICS FORGOT HISTORY

This was very similar to a point made by Gustav von Schmoller (1900). Similarly, Souter argued for the integration, rather than the compartmentalisation, of the social sciences. Souter reviewed Robbins's *Nature and Significance of Economic Science* in the *Quarterly Journal of Economics* in 1933. This critical and perceptive review attacked the very principle of a separate domain of economic science. Souter (1933a, pp. 378-9) wrote of Robbins's work:

it is somehow assumed that the 'analytical' method of 'defining' special disciplines in terms of their respective distinctive *attitudes* towards a common 'subject-matter' somehow or other justifies a particular science in erecting round itself barbed-wire entanglements which would be thoroughly pernicious if erected in the name of a classificatory 'definition' in terms of *different* 'subject matters.' . . . It is in this way that 'economic science' miraculously juggles 'psychology' . . . over the wall and so obtain its 'independence.' . . . in the case of economic science this segregation by means of 'analytical definition' is effected by regarding 'economics' as a *purely formal science of implications*.

Hence Souter was resolutely hostile to Robbins's attempt to define economics in terms of the autonomous, deductive investigation of the relation between scarce means and given ends. In 1933, he perceived correctly that this would eventually lead to an economics most arid and formal. Souter (1933a, p. 387) argued that 'the really appalling danger lies in the inability of the economic formalist to understand that their abstractions cannot be made intelligible except through organic subordination to a concrete social and political philosophy.' The result, Souter (p. 385) argued perceptively, was that 'the abstract formalist, in pursuit of perfect formal precision, fails alike in precision and in formality.'

Souter examined some of the reasons for this failure. Prominent among them was the treatment of time and dynamic change. On the one hand: 'From the standpoint of instantaneous or timeless "statics" . . . rationality has no meaning. It is only when we explicitly introduce the category of Time . . . that the concept of formal rationality becomes intelligible' (Souter, 1933a, pp. 387-8.) On the other hand, however, time and change challenged the 'given' ends upon which Robbins wished to erect the whole of economic science: 'For in a "dynamic" world in which we live, the working out of the "implications" of "given" data itself changes the data' (Souter, 1933a, pp. 394-5).

Souter (1933b, p. 94 n.) thus argued for an 'enlightened and democratic "economic imperialism" which would enrich both sociology and economics by a mutual fusion and interpenetration of ideas. As noted already, this version of 'economic imperialism' is very different from the modern doctrine of Gary Becker, Jack Hirshleifer and others, where it is held that all other disciplines should be subjugated by neoclassical economic theory.

UNIVERSALISM IN FRIEDRICH HAYEK'S ECONOMICS

On an invitation from Robbins, Friedrich Hayek came to the LSE in 1931, where he was elected to a chair. Although Hayek's theory was not so rationalist and deductivist as that of Robbins, he followed Robbins and his Austrian mentors by insisting that the starting point of economic theory was the supposedly universal features of the economic situation, rather than the essential features of any specific type of socio-economic system. Hayek criticised the historical school in the following terms:

To start here at the wrong end, to seek for regularities of complex phenomena which could never be observed twice under identical conditions, could not but lead to the conclusion that there were no general laws, no inherent necessities determined by the permanent nature of the constituting elements, and that the only task of economic science in particular was a description of historical change. It was only with this abandonment of the appropriate methods of procedure, well established in the classical period, that it began to be thought that there were no other laws of social life than those made by men, that all observed phenomena were only the product of social or legal institutions, merely 'historical categories' and not in any way arising out of the basic economic problems which humanity has to face.

(Hayek, 1935, p. 12)

Consider some relatively minor points first. Contrary to Hayek, there is no good reason why regularities should be absent in principle from complex systems (Cohen and Stewart, 1994). As a result, empirical observation of complex phenomena would not always fail to reveal regularities, nor necessarily lead to the false methodological claim that the sole task of economic science is description. Furthermore, modern students of complexity are aware that such regularities do not necessarily have to emanate from any presumed 'permanent nature of the constituting elements'.

More importantly, like Robbins, Hayek presumed that 'the basic economic problems which humanity has to face' were the universal dilemmas of choice and scarcity. Be that as it may, the focus on these universal phenomena gets us 'not very far' as Knight (1924, p. 229) had put it. Such a universal focus can tell us very little about specific institutions, such as private property and markets. Yet Hayek assumed that the 'basic economic problems' of choice and scarcity could be realised through the operation of markets and private property only. Attempting to reconcile his universal assumptions and his devotion to the specific institutions of property and markets, Hayek postulated that these institutions have existed, to some degree, since the dawn of humanity. This stance on the question of universal and historically specific phenomena persisted through Hayek's writings, despite some shifts in his methodological position.³

³ For discussions of Hayek's changing position see Caldwell (1988), Fleetwood (1995) and T. Lawson (1997).

For Hayek, the focus on universal assumptions was combined with a 'compositive' method of analysis, working from the individual to the whole. Much later, Hayek (1967, p. 72) wrote: 'the whole of economic theory may be interpreted as nothing else but an endeavour to reconstruct from regularities of the individual actions the character of the resulting order.' However, if historically specific institutions or circumstances affect the 'regularities of the individual actions' then there is a case for examining these specifics, in addition to the deduction of 'the character of the resulting order'. In addition, human interaction depends on *prior* social structures, such as language, the family and the institutions of socialisation. Any attempt to explain these in terms of individual actions alone would involve an infinite regress. Because institutions such as language are required for all economic and social interaction, we can never, neither historically nor logically, arrive at an institution-free 'state of nature' (Field, 1979, 1981, 1984; Hodgson, 1998a). The same unresolved problem that arose in Carl Menger's attempt to build economics on exclusively individualist foundations recurred again in the work of Hayek.

In most of Hayek's writings the market appears as some vague, universal forum in which individual property owners colude. What Hayek failed to appreciate adequately was that the market itself is not a natural datum but a social institution, governed by sets of rules defining restrictions on some, and legitimating other, behaviours (Vanberg, 1986). Furthermore, the market 'is necessarily embedded in other social institutions such as the state, and is promoted, or even in some cases created, by conscious design. Given that markets are themselves institutions, then they may grow or decline like other institutions: they are not universal entities. For much of his time at the LSE, Hayek's writings were preoccupied with his critique of planning and socialism. I have discussed these issues at length elsewhere (Hodgson, 1999a). Even here he could have benefited from the legacies of historicism and institutionalism. His incisive understanding of the market mechanism would have been further enhanced by the consideration of the institutional nature of markets. He could also have benefited from the discussion of the economic role of knowledge and the evolution of institutions in the writings of Thorstein Veblen. He could have also recognised that some elements of his criticism of socialist central planning were anticipated in the writings of some members of the German historical school, notably Albert Schäffle, Lujo Brentano and Erwin Nasse (Hutchison, 1953, pp. 293-8). But any positive mention of historicism or institutionalism would not have pleased Robbins. Although the theoretical approaches of Hayek and Robbins were quite different, what they shared was a common and deep hostility to institutionalism and the German historical school. For this reason, Hayek made little use of, or reference to, their works.

During the Second World War, the LSE was evacuated from its buildings in the centre of London and its offices were temporarily located in Cambridge. Hayek knew John Maynard Keynes well. But in theoretical terms they were permanent antagonists. When the *General Theory* appeared in 1936, the Robbins-Hayek grouping at the LSE was the chief British academic opposition to the rise

DEATH AND COUNTER-REVOLUTION AT THE LSE

of Keynesianism. Keynes is the subject of the next chapter. Hayek left the LSE for the University of Chicago in 1949. Later he was awarded the Nobel Prize in economics.⁴

CLEOPATRA'S NOSE

History is full of ironies. What if Cleopatra had not been so beautiful? The tale of love and war, involving Julius Caesar and Mark Anthony, might have been different. As another example, what if it not rained on that fateful night before Waterloo? The ground would have been drier and harder. Napoleon might have been able to move forward his artillery in the early morning, and destroy Wellington before the arrival of Blücher. What if Lenin had not arrived in St. Petersburg in 1917? The Bolshevik Revolution might not have occurred. These 'virtual histories' reveal that tremendous panoply of the potential, even greater than the rich and complex world of the actual .s

Given this, we may ask: what if Allyn Young had not died in 1929? Apparently, Young had decided before his death to quit his chair at the LSE and to return to the United States. Robbins might have got his chair anyway. But Young might have changed his mind, or he might have had a decisive influence in the appointment of his successor. If so, the story of the LSE and of British economics as a whole might have been very different. Robbins might not have climbed to power, Hayek may have never come to Britain, and a school of British institutionalists might have developed at the LSE. But it did not happen.

After retiring as LSE Director in 1937, Beveridge (1960, p. 92) wrote that Young 'was just the man to make economics as the Founders and I wished it to be'. However, he argued that after Young died, economics at the LSE took a wrong turning: 'The London School of Economics and Political Science, even after fortytwo years, had not achieved the purposes for which Sidney and Beatrice Webb had brought it to birth' (Beveridge,1960, p. 95). Beveridge desired an economics that was less deductive in character and more based on facts. Clearly this was an implied criticism of the direction in which Robbins had led economics at the LSE.

Of course, Young's death does not completely explain the demise of institutional economics at the LSE. Eventually, Robbins's 1932 Essay became popular, in both Britain and the United States. Almost certainly, the book would have been published even if Robbins had not achieved early promotion. It was endorsed by a rising generation of neoclassical economists, who became eager to redefine economics in Robbinsian terms. In any case, it was likely that the

⁴ As Hayek's theoretical position developed, and he emphasised rules and institutions more and more, his work acquired some analytical affinities with institutionalism (Boettke,1989; Leathers, 1989,1990; Rutherford,1989; Samuels,1989; Wynarczyk,1992). However, Hayek never adequately acknowledged the problem of historical specificity, even in his later writings. Regrettably, he always dismissed 'historismus' with disdain.

⁵ See Ferguson (1998) for historical scholarship in this area.

LSE would have succumbed to the growing forces of the postwar neoclassical revival. Nevertheless, Young's death accelerated a process that took much longer in other institutions. With the precocious rise of Robbins, the development of historicism and institutionalism was blocked at an early stage at the LSE.

While the *Methodenstreit* in Germany was still unresolved as late as the 1930s, Robbins and Hayek played a crucial role in rewarding Menger with an apparent victory. Two factors were critical. The first was the adoption of the Mengerian conception of the nature and role of economics by a number of scholars in the West. The departments of economics in two universities played a major role here. One was the LSE. Allied to this, as we have seen in the preceding chapter, was the migration of Talcott Parsons and Joseph Schumpeter to Harvard. Parsons brought with him Weber's endorsement of Menger's individualistic conception of theoretical economics. Schumpeter brought his own, very similar, views on the nature of economics and its relation to other disciplines. The second factor was the almost complete destruction of the German historical school in the 1933-45 period, as a result of fascism and war. Hence the victory of individualistic and ahistorical economics was as much a result of political and institutional events as of persuasive argument.

By comparison, things were very different at nearby Cambridge. From the beginning, Marshall's conception of the role and scope of economics was significantly different from that of Menger. The role of John Maynard Keynes was also crucial. Just as the neoclassical reign of Robbins succeeded the critical institutionalism of Young, Keynes in Cambridge was establishing himself as the leading theorist of a distinctive approach. Compared with the LSE, from the 1930s to the 1970s, Cambridge had a reputation of dissent from the mainstream tradition of economic thought. Part of the Cambridge story is related in the next chapter.

JOHN MAYNARD KEYNES AND HIS DECLARATION OF A *GENERAL THEORY*

Time is a device to prevent everything happening at once, space is a device to prevent it all happening in Cambridge. (A paraphrase - attributed to Dharma Kumar of Joan Robinson)

Keynes was a product of his Cambridge surroundings. His father wrote a major treatise on the methodology of economics. Educated initially in mathematics and philosophy, his move towards economics placed him under the influence of the Cambridge Marshallian tradition.

As we have seen in chapter 8, Alfred Marshall had explicitly recognised the importance and significance of the German historical school. Marshall had also urged John Neville Keynes to immerse himself in the Austro-German literature on the *Methodenstreit*. However, after Marshall's retirement in 1908, economists at the University of Cambridge paid dwindling attention to the German historical school. Strikingly, John Maynard - Neville's son - paid them little regard. Maynard Keynes followed Marshall in attempting a general theory but with none of Marshall's acknowledgement of the problem of historical specificity. Apart from occasional positive citations of Georg Knapp's (1924) theory of money, there are very few other references to either the German historical school or to American institutionalism in Maynard Keynes's works. Overall, and unlike his predecessors, Keynes made little reference to the German historical school and he seemed largely unaware of its contribution. In Keynes's *Collected Works* as a whole, there are only two minor footnote references to works of Gustav von Schmoller and no reference whatsoever to Werner Sombart.¹

Keynes candidly admitted (1930, vol. 1, p. 199 n.) that his knowledge of the German language was 'poor'. But this fact alone cannot explain his minimal engagement with other schools of thought overseas. It would have been possible

¹ Keynes had encouraged the translation of Knapp's work into English and he referred to it approvingly in his own *Treatise on Money* (Keynes, 1930). Of course, Keynes was more familiar with some of the members of the British historical school, particularly those who were friends or acquaintances of Marshall. Among these, most notably, was Foxwell. Keynes (1972) wrote a lengthy obituary on Foxwell in the *Economic Journal* in 1936.

to obtain linguistic help and to make a determined effort to obtain knowledge of a substantial, thriving and long-lasting community of social scientists situated across the North Sea, just a few hundred miles to the east of Cambridge. Indeed, at least for any economist writing before the Second World War, some knowledge of the German contribution to economics was a scholarly requirement, and not a mere option.

Furthermore, to the west across the Atlantic, institutional economics was then a substantial force in America. In this case there was no language barrier. Nevertheless, there is no evidence to suggest that Maynard Keynes was generally familiar with the works of leading American institutionalists. Keynes never referred in his writings to Thorstein Veblen. He was most acquainted with the ideas of Wesley Mitchell. Keynes (1930) cited Mitchell's work on business cycles several times in his *Treatise on Money*. In May and June 1934 he met Mitchell in New York City and received an honorary doctorate at Columbia University (Keynes, 1973a, p. 456; 1982, p. 320). Despite this, Keynes failed to mention Mitchell in his *General Theory*.

Keynes briefly referred to John Commons. In a talk and in an essay both written in 1925, Keynes (1931, pp. 303-4; 1981, p. 438) acknowledged Commons as an 'eminent American economist' and expressed agreement with Commons's idea that history had passed through 'the era of scarcity' followed by 'the period of abundance' in the nineteenth century and the 'period of stabilisation' in the twentieth. In 1927, Commons sent Keynes one of his short articles on 'Price Stabilization and the Federal Reserve System' (Commons, 1927). As a result, Keynes wrote to John Commons on 26 April 1927: 'Judging from limited evidence and at great distance, there seems to be no other economist with whose general way of thinking I find myself in such genuine accord.' However, the influence of Commons on Keynes was slight at best, for there is no evidence that Keynes turned a single page of such key works as *The Legal Foundations of Capitalism* (1924) or *Institutional Economics* (1934). Robert Skidelsky's (1992, p. 229) description of Commons as 'an important, if unacknowledged influence on Keynes' is an unwarranted exaggeration. The evidence of influence simply concerns Commons's highly questionable scheme of historical periodisation, which for some strange reason Keynes briefly found attractive.²

Many other economists who came into contact with Keynes were more aware of the historicist and institutionalist legacy. While Keynes was editor of the *Economic Journal* - from 1912 to 1945 - he saw the publication of a substantial number of articles, notes and reviews that cited the works of leading historicists and institutionalists of his day. Yet Keynes paid relatively little attention to these cited works.³

² The idea that history had passed through 'the era of scarcity' followed by 'the period of abundance' and the 'period of stabilisation' is found in Commons (1934a, pp. 773-88). It is not clear how Keynes got sight of an earlier draft of these ideas. Keynes's letter to Commons is found in the Commons Papers, State Historical Society of Wisconsin, 1982. It is quoted in Skidelsky (1992, p. 229) and Whalen (1993, pp. 1175-6).

³ In the complete years from 1912 to 1944 inclusive - while the journal was under Keynes's editorship - the following names were cited by other authors in articles, notes or reviews in the *Economic Journal*: Commons (30), Mitchell (133), Schmoller (69), Sombart (89), Veblen (39), Weber (104). (The number in brackets is the number of items in which each name appears.) However, none of these names is cited in the *General Theory*. In contrast to Keynes, many contributors to the *Economic Journal* believed that the ideas of these leading institutionalists and historicists were of sufficient interest to be worthy of citation.

Joan Robinson (1973, p. ix) famously complained that Keynes 'never managed to read Marx'. This was a serious defect for a theorist of the contemporary economic system. However, the omissions go further. Remarkably, in the *General Theory* - his most important and by far his most widely read book - there are no references in the index to leading institutionalists or historicists such as Commons, Knapp, List, Mitchell, Schmoller, Sombart, Veblen or Weber. Schumpeter also receives no mention. Apparently, their ideas played little part in the development of Keynes's theory.⁴

From a policy point of view, Keynes found many kindred voices in other countries. Yet he made inadequate reference to economists, in Germany as well as the United States, who argued long before 1936 that the stimulation of effective demand was the key to economic recovery from the Depression. In the United States, for example, the idea of using public works to revive the economy and reduce unemployment was widespread among economists by 1935 (J. R. Davis, 1971). Yet these American ideas had little impact on Keynes's writing. Language barriers alone cannot explain Keynes's neglect of closely related intellectual developments elsewhere.⁵

Remarkably, as George Garvy (1975) and Jürgen Backhaus (1985) have shown, in Weimar Germany there was a substantial group of economists - including Sombart - who had developed a sophisticated argument for increased public spending to combat recession. As Backhaus (1985) demonstrates, Sombart's views on this matter were grounded in his theoretical analysis of modern capitalism. Again, this had little impact on Keynes. Overall, as Garvy (1975, p. 393) argued, Keynes was a product of a relatively insular academic environment:

⁴ The omission of Mitchell from the *General Theory* is particularly striking, as he had helped to lay the foundations of national income accounting - a practical requirement for the operationalisation of Keynes's macroeconomic principles (Hodgson, 1999b). However, in the *General Theory* there are singular references to two American institutionalists - Knight and Kuznets - and a substantial acknowledgement of the work of the English historicist and institutionalist Hobson. Nevertheless, by comparison, the following economists received multiple references in the *General Theory*: Cassell, Edgeworth George, Hawtrey, Hayek, Jevons Kahn, Malthus, Mandeville, Marshall Mill von Mises, Petty, Pigou, Ricardo, Robbins, D. H. Robertson, Say and Smith. This number of citations to other authors would refute the defence that Keynes rarely cited institutionalists and historicists because he rarely cited anyone. Mere citation is a blunt bibliographic measure, but there is clear evidence here of a relative neglect of historicism and institutionalism.

⁵ From the 1920s on, Keynes declared that a policy of free trade was unrealisable and undesirable. Against him Robbins was one of the leading academic advocates of free trade. In responding to Keynes in the *New Statesman and Nation* on 4 April 1931, Robbins described: 'The shades of a million dead parrots - the much bewhiskered *historismus* of the past - rise up and hail him [Keynes] as a brother, "recognized at last"' (quoted in Koot, 1987, p. 210). Ironically and unfortunately, Keynes was unable to transform this into a useful compliment and to use the plentiful arguments of the historicists against Robbins. Furthermore - unlike the inert parrot of *Monty Python* fame - the flock of 'dead parrots' described by Robbins was far from deceased at the time.

A simple answer to the question why *General Theory* was written in an intellectual vacuum can be given by referring to the well-known isolation of Cambridge economists within the proverbial insularity of Great Britain . . . Keynes read little of what contemporary economists had to say . . . [n]or did he dig deeply into the wealth of ideas and analyses of the preceding generations of economists. He largely ignored the contributions of his contemporaries (and the generations preceding them) who published in languages other than English.

To what extent can the insularity of Keynes's intellectual environment explain these defects of omission? Significantly, Marshall was also at Cambridge but he referred repeatedly to leading German economists. However, the Cambridge intellectual atmosphere seemed to change after Marshall's retirement in 1908. His successor Arthur Pigou kept the Marshallian intellectual legacy alive but failed to sustain the momentum of Marshall's curricular innovations and reforms. Pigou was a notoriously shy recluse and had 'a total lack of administrative capacity' (Groenewegen, 1995, p. 755). Crucially, he had no apparent knowledge of, or interest in, the German historicist legacy. With the exception of a positive review of Wesley Mitchell's 1913 work on *Business Cycles* (Pigou, 1914), Pigou made no significant mention of historicism or institutionalism. Overall, despite his own achievements, Pigou was less careful than Marshall in reminding others of the qualified and provisional nature of all economic theory. Although Pigou himself made a major contribution, much of this was completed before 1920. In the 1920s, Cambridge economics seemed to rest on its laurels. Keynes himself was largely responsible for the revival of economics at the university. As Robert Shidelsky (1992, p. 286) put it, following the publication of Keynes's *Treatise on Money* (1930), 'Cambridge economics was coming to life after its long war-induced slumber.'

Keynes had the luck and the brilliance to write the most prominent and important economics text that provided a theoretical justification for government interventions to raise the level of employment in the 1930s. He also played a major role in the planning of the new international economic institutions that were to emerge after the Second World War. Accordingly, the term 'Keynesian' became associated with a number of theoretical and policy ideas, some of which in fact pre-date the *General Theory*. Keynes's outstanding volume codified the macroeconomic ideas of the postwar consensus. It became the bible of macroeconomic theory. However, as a result, the silences and omissions of the *General Theory* became the amnesia of postwar generations of economists and policy makers. Hence, despite the enormous importance and value of his contribution, Keynes was to play an unwitting role in the Great Forgetting that was to afflict economics

6 For a discussion of the historical and institutional context of the rise of Keynesianism see Winch (1969). It is not the intention of this chapter to undermine the importance and genuine value of Keynes's contribution. Elsewhere, however, I have criticised Keynes's overly rationalist conception of action and his neglect of the influence of the historically specific past on the decision making processes of the agent (Hodgson, 1988, ch. 10).

in the years to come. Keynes's work was both a symptom and an additional cause of the abandonment of the heritage of historicism and institutionalism in economics after the Second World War. Of course, Keynes's negative role in this respect must be put alongside his major and revolutionary achievements in economics. Nevertheless, his abandonment of historicism and institutionalism had effects that hitherto have been unappreciated, and it is necessary to discuss them here.

Keynes is rightly credited with furthering a global revolution in economic thought, which radicalised the whole approach to macroeconomic theory and policy. However, the crucial historicist lacuna in his knowledge, combined with his own admiration for general theorising, was eventually to help to obliterate both institutionalism and the historical school from the memories of both mainstream and dissident economists.

HOW GENERAL IS THE *GENERAL THEORY*?

Analytically, we focus on just one aspect of Keynes's contribution. Despite the gigantic secondary literature on Keynes and Keynesianism, it has rarely been discussed. Yet Keynes himself attributed to it much importance. Very near to the beginning of his *General Theory* he wrote:

I have called this book the *General Theory of Employment, Interest and Money*, placing the emphasis on the prefix *general* I shall argue that the postulates of the classical theory are applicable to a special case only and not to the general case, the situation which it assumes being a limiting point of the possible positions of equilibrium. Moreover, the characteristics of the special case assumed by the classical theory happen not to be those of the economic society in which we actually live, with the result that its teaching is misleading and disastrous if we attempt to apply it to the facts of experience.

(Keynes, 1936, p. 3)

The first chapter of the *General Theory* consists solely of the single paragraph from which the above quotation is taken. Here Keynes was playing a double rhetorical game. First, the term 'general theory' was used to create a contrast with the special theory of the 'classical' economists. Keynes made the convincing argument that the 'classical' theory was a 'special case': it pertained to a special and limited set of possible outcomes. Keynes enlarged on this point throughout the *General Theory*, arguing that the 'classical' theory encompassed neither uncertainty nor disequilibria. He wanted a 'general theory' that could explain such phenomena.

There is a crucial ambiguity in the above quotation. According to one possible interpretation, Keynes was seeking a theory that could explain more adequately the possible outcomes in *one given type* of 'economic society'. Such a theory would be more general and satisfactory than one that explained less. However, such a theory might not be general in the sense that it could lead to adequate

HOW ECONOMICS FORGOT HISTORY

explanations of phenomena pertaining to *other types* of 'economic society'. In the terms introduced in chapter 2, Keynes in the above passage might have sought explanations that were more general in an intensive rather than in an extensive sense. The claim made there by Keynes was that his 'general' theory could embrace and explain more phenomena within the single 'economic society in which we actually live'.

However, as shown below, Keynes also made extensive claims about the generality of his theory. He also claimed in the same work that his theory had sufficient generality to apply to several different types of 'economic society', by virtue of its supposed foundation on universal 'psychological laws'.

If the 'economic society in which we live' is different in one or more important respects from other socio-economic systems, then an attempt at an extensive general theory, embracing all or several such systems, might have difficulty including every possibility. The complexity of reality can place limits on the detailed explanatory power of any general theory. The pursuit of a general theory always involves simplification and loss of specific details. The danger is that we may fail to identify the particular mechanisms that were relatively more important at a specific historical juncture. Keynes did not seem to recognise this dilemma. He did not acknowledge that by trying to make a theory more general we may make it less able to focus on the important aspects of the 'economic society in which we live' and less able to design effective policies to deal with economic problems.

In economics, a theory with sufficient explanatory power would have to focus on the key economic relations and processes that were of importance in understanding the nature and behaviour of the system in question. Keynes did not consider that a theory with substantial explanatory power, that applied to the 'economic society in which we live', might have to be a special theory. Indeed, a 'general theory' might under-emphasise some of the historically specific features of the economic system and the causes of the prevailing unemployment of the 1930s. Keynes overlooked the possibility that a special theory could use some assumptions concerning economic institutions that were limited to a historically specific domain of analysis, and end up with much greater explanatory power.

Keynes was concerned to criticise those 'classical' theories that claimed to show that markets would clear and the economy would automatically reach a full-employment equilibrium. But the fact that Keynes clearly considered disequilibria, and other equilibria below full employment, was not enough to make his theory truly *general*. There were other types of system - such as economies without money-- that in fact had no place in Keynes's theory. The classical theory is not general, in part because it assumes price flexibility, excludes radical uncertainty and under-estimates the role of money as a store of value and means of dealing with an uncertain future. Nor, for different reasons, is ~ the *General Theory*. While Keynes dropped several of the classical assumptions, he imposed other restrictive conditions. For instance he assumed a monetary economy, without extensive barter, where money plays a special role. While ~ Keynes made his theory more general with one move, he made it less general

with another. Overall, it is difficult to say whether the classical or the Keynesian theory is more general. And if one theory is more general that would not necessarily mean that it is a better theory.

In addition, Keynes did little in explicit terms to ground his theory upon historically specific economic institutions. Although institutions, such as the joint stock company and the stock exchange, inevitably protrude into his narrative, he did not start from the specific institutions of capitalist society and then develop a theory that illuminated their principal causal processes and relations. Instead, Keynes (1936, pp. 246-7) appealed repeatedly to 'fundamental psychological factors' as the foundation for his theory. His invocation of supposed psychological factors in his discussion of economic processes is more prominent than any discussion of historically specific institutions. Specific institutions appear casually in the *General Theory* as the mechanisms through which seemingly ahistorical psychological forces express their power. Keynes attempted to develop a 'general theory' that would apply to a number of different types of socio-economic system. He conceived of this general theory as having a universal and psychological foundation.

A striking piece of further evidence confirms this verdict. As Bertram Schefold (1980) has shown, in his 1936 Preface to the German edition of the *General Theory*, Keynes made the following extraordinary but symptomatic argument:

This is one of the reasons which justify my calling my theory a *General* theory. Since it is based on less narrow assumptions than the orthodox theory, it is also more easily applied to a large area of different circumstances.'

According to Keynes, his *General Theory* applied not only to the 'Anglo-Saxon countries . . . where laissez-faire still prevails' but also to countries with strong 'national leadership' such as Nazi Germany. He made this statement on the basis that his analysis was not based on specific institutions but allegedly on 'the theory of psychological laws relating consumption and saving'. Hence Keynes clearly claimed that his theory was not based on historically specific institutions but on general 'psychological laws'. But Keynes gave little guidance on the psychological literature from which these supposed laws were derived.⁸

⁷ Davidson's (1996) translation of this passage is slightly different. In particular, he translates *weniger enge Voraussetzungen* as 'fewer restrictive assumptions' instead of 'less narrow assumptions'. Both translations are possible, but in personal correspondence Schefold has given reasons why his version is to be preferred. We are unlikely to determine Keynes's intended meaning in more precise terms, because the preface was translated into German from a draft by Keynes, which the editor condensed and has since been lost.

⁸ Schefold (1980) pointed out that, with the exception of the statement concerning 'the theory of psychological laws', the above words were excluded from the English translation of Keynes's preface to the German edition, in his *Collected Works*, vol. 7. Notably, Davidson (1996) quoted and endorsed the key words from the 'missing' passage quoted here. Hutchison (1981, pp. 261-2) has indicated, however, that in the preface to the French edition of the *General Theory*, Keynes made yet another claim, that his theory was also general in the sense that it applied to the general behaviour 'of the economic system as a whole'.

Furthermore, Keynes did not in fact deliver what he had promised: a general universal statements. In particular, he stressed aspects of human psychology. But he propensities worked out in practice except by introducing an explicit or implicit psychology had to play out its part on some specific institutional stage. It had to be institutional structures, such as to financial markets, state money and legal contracts. psychology of speculation in chapter 12 of the *General Theory* requires a specific principally the stock market. Other parts of the book, such as Keynes's theory of degree of generality, although these are not universal to all types of human society. specific phenomena. Consider the specific economic phenomena to which Keynes Even here he did not fulfil the promise of a general theory. The work did not nature and level of employment in all past, present or possible human societies. quite specific relationships in modern capitalism between employment, expectations providing a truly general theory of interest or money, Keynes explored the quite which 'money is the drink which stimulates the system to activity' (Keynes, 1936, p. thousands of years but it did not become such an elixir of production until the rise of favoured the 'general theory' rhetoric but always ended up exploring the particular capitalist system. Absent in the *General Theory* is a truly general theory of

In sum, Keynes claimed generality but relied upon the historically specific institutions of modern capitalism. Overall, one wonders why Keynes was inclined to use and emphasise the 'general theory' phrase. He could have easily and concisely called his book *A Theory of Employment, Interest and Money*. The intellectual mood of the times may have been a factor. It may help to explain why Keynes aimed to develop a 'general theory' and neglected the problem of historical specificity. Albert Einstein had developed 'the general theory of relativity' 9 Physics and biology were both basking in the illuminations of Einstein, Darwin and others, and seemingly making great strides towards the conceptual unification of each discipline. Similarly, in these 'years of high theory' (Shackle, 1967) economists were also striving towards a grand, synthetic explanation of the underlying forces of economic recovery and growth.

Notably, Keynes did show some awareness of the philosophical basis of the problem of historical specificity. In a letter to Roy Harrod dated 4 July 1938, Keynes (1973b, p. 296) wrote:

Economics is the science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to do this, because, unlike the typical natural science, the material to which it is applied is, in too many respects, not homogeneous through time.

However, there is an inconsistency in Keynes's work. In the above letter he implied that economic theory must be related to historically specific material. Yet the *General Theory* was attempted on the basis of universal 'psychological laws'. If Keynes had been aware of the vast historical school literature, which had tried to develop economics in full awareness that economies are 'not homogeneous through time', then he would have been less likely to attempt an entirely *general* theory.

Any analysis in economics that engages with reality is bound to make some assumptions about the institutional make-up of society. The *General Theory* was no exception. But the reckless striving for generality relegates specific institutions to the background, whereas they ought to occupy the centre of the stage. There is not much discussion in the *General Theory* of specific economic institutions that are, in fact, indispensable to his argument. For example, Keynes was concerned to examine the nature of the wage bargain, and the relation between real and money wages. But the institutions of the labour market and employment are not discussed in any depth. In this respect, Keynes attempted the impossible: to draw quite specific conclusions from a theory that purported to be general.

This pretence of generality has widely afflicted economics for much of the twentieth century. Because of Keynes, many of his followers have attempted general theories as well. On the other hand, some post-Keynesians have stressed the importance of history and specific economic institutions, so that the rhetoric of general theorising has been implicitly undermined.

For example, Victoria Chick (1986) has shown that standard assumptions of monetary theory are specific to the financial institutions involved. As these institutions evolve through time, different theoretical principles can pertain. In particular, the nature of money itself changes, from precious metal, to bank deposits, to data in computer memories. Chick argued that because of the institutional realities of pre-industrial capitalism, saving necessarily preceded investment. Subsequently, as soon as banks were able to create credit, saving no longer had to precede investment. As the banking system evolved it enhanced the capacity for the banks to create credit. Hence, by the 1920s and the time of Keynes, banking institutions and the credit system had evolved to the point that investment could and would precede saving. This was the quite specific historic period to which the allegedly *General Theory* applied. Subsequently, as Chick pointed out in her paper, financial institutions have developed further, with

HOW ECONOMICS FORGOT HISTORY

massive global speculation in a variety of financial assets. This may mean that Keynesian analyses and remedies can to some extent become obsolete.

Chick's argument underlines the fact that the *General Theory* was not, in truth, a general theory but it applied to a historically specific set of capitalist institutions. Like Chick, other Post Keynesians have explicitly centred their analysis on historically specific institutions. What has been largely unnoticed, however, is the implication that the professedly general theoretical status of the *General Theory* is likely to be undermined as a result.

JOHN MAYNARD KEYNES, JOSEPH SCHUMPETER AND ECONOMIC POLICY

In his extended and generous obituary of Keynes, Schumpeter (1946, p. 514) wrote of the *General Theory*:

But there is one word in the book that cannot be defended on these lines - the word 'general.' Those emphasizing devices - even if quite unexceptionable in other respects - cannot do more than individuate very special cases. Keynesians might hold that these special cases are the actual ones of our age. They cannot hold more than that.

Schumpeter thus criticised Keynes for propounding a theory that claimed to be general but in fact was not. Similarly, in his earlier review of the *General Theory*, Schumpeter noted the contrast between Keynes's claim to provide a general theory and his keenness to promote specific economic policies. Schumpeter (1936, p. 792) claimed that Keynes had adopted the 'Ricardian' practice of claiming highly specific policies from an allegedly general theory:

Mr. Keynes underlines the significance of the words 'General Theory' in his title But . . . everywhere he really pleads for a definite policy It is, however, vital to renounce communion with any attempt to revive the Ricardian practice of offering, in the garb of general scientific truth, advice which - whether good or bad - carries meaning only with reference to the practical exigencies of the unique historical situation of a given time and country.

This is a valid criticism. Schumpeter (1954, p. 1171) dubbed this defect 'the Ricardian vice'. He thus alleged a parallel between Ricardo and Keynes: 'Keynes . . . was Ricardo's peer also in that his work is a striking example of . . . the Ricardian Vice, namely, the habit of piling a heavy load of practical conclusions upon a tenuous groundwork.'

Schumpeter (1946, p. 514 n.) also noted that Oskar Lange in 1938 had 'paid due respect to the only truly general theory ever written - the theory of Léon Walras'. Lange (1938, p. 20) had argued that 'both the Keynesian and the traditional theory of interest are but two limiting cases of what may be regarded to be the general

theory of interest . . . the essentials of this general theory are contained already in the work of Walras.' Later Schumpeter (1954, p. 1082) mistakenly declared Keynes's *General Theory* to be 'a special case of the genuinely general theory of Walras'. Clearly, Schumpeter too was beguiled by the lure of a general theory. That is one reason why he praised Walras throughout his life. Schumpeter rightly pointed out that the *General Theory* was not truly general. Schumpeter's persuasive criticism of Keynes was that instead of attempting to derive specific policies solely from a theory that claimed to be general, Keynes should have analysed a historically specific situation. His unpersuasive criticism of Keynes was that the *General Theory* was not general enough.

Schumpeter's invocation of Walras as a general theorist was also questionable. Contrary to Schumpeter, Walrasian theory is not general. Walras made restrictive assumptions in his model, by excluding out-of-equilibrium trading, for instance (Bertrand, 1883; De Vroey, 1998). It has been admitted by leading practitioners of Walrasian theory - such as Kenneth Arrow (1986) and Frank Hahn (1980) - that it fails to incorporate key phenomena, such as time and money.

Although some of Schumpeter's criticisms of Keynes were on target, others were not. In particular, Schumpeter's attempt to put Keynes in the same camp as Ricardo is misleading. Although Keynes tried to draw out specific policies from something masquerading as a 'general theory', he did this in a manner very different from that of Ricardo. Ricardo was a deductivist theorist *par excellence*. Keynes preferred the more tentative and empirically informed theorising of Malthus and others.

The genuine defect that Schumpeter recognised was that Keynes simultaneously revered a 'general theory' and attempted to derive quite specific policy conclusions from such an edifice. For instance, the scope for governmental management of the level of effective demand would depend crucially on the economic institutions in a particular country and the nature and extent of its engagement with world markets. An entirely 'general theory' can tell us little of these vital but specific details. In this respect, Schumpeter's criticism hit home. It might be possible to regard Keynes's work as a framework for viable analyses that addressed such specific circumstances, but Keynes himself did not lay down guidelines for the development of historically sensitive theories.

THE POSTWAR TRIUMPH OF 'GENERAL THEORY'

Keynes's theory triumphed, at first by capturing, by the end of the 1930s, the two great Anglo-American academic bastions of Harvard and Cambridge. This was an impressive achievement and did a great deal to establish for Keynes a global and deserved reputation. Yet Harvard had been the centre of resistance against interwar institutionalism in the United States. And Cambridge had forgotten the historicist background to its earlier, Marshallian revolution. The price of victory was indeed very dear. Cambridge insularity and Harvard neoclassicism were legitimised. Consequently, the manner and locations of

the Keynesian triumph helped to obliterate the memories of historicism and institutionalism on a global scale.

The subsequent story of how the economic of Keynes was transformed and vulgarised into postwar 'Keynesianism' is well known. Like Marshall before him, Keynes was highly critical of the abuse of mathematical and formal methods. Despite this, the rising general of mathematical economists hijacked some of his ideas. Influential academic contributions - including those from Alvin Hansen and Paul Samuelson in America, and Roy Harrod and John Hicks in England - helped to create a mathematical 'Keynesian' system.

Alongside this so-called 'Keynesian general theory', Walrasian microeconomics became widely accepted in the 1940s, particularly as the result of the work of John Hicks (1939) in the UK and Paul Samuelson (1947, 1948) in the USA. As a result, 'general equilibrium theory' became the core of microeconomic analysis.

The attraction of the new 'Keynesian' macroeconomics was partly its claim to generality, partly its technocratic lure, and partly because of its apparent policy solutions to the pressing economic problems of the day. For a rising generation of technocratic economists, the synthesis of Walrasian general equilibrium theory with the Keynesian macroeconomic 'general theory' was all very appealing.

Although Keynes attacked the equilibrium theorising found in the classical and neoclassical traditions, his own announcement of a 'general theory' struck a chord among the rising generation of mathematical, Walrasian economists. If he had addressed the problem of historical specificity and eschewed the 'general theory' label, then the notorious postwar synthesis in economics would have been more difficult to achieve. Hence Keynes must take a small part of the blame for the incorporation of 'Keynesianism' into the postwar, neoclassical, textbook synthesis of microeconomics and macroeconomics.

There were justified protests from some Keynesians that Walrasian microeconomics was incompatible with Keynesian macroeconomics, but the phrases 'general theory' and 'general equilibrium theory' have two out of three words in common. While the word 'general' in 'general equilibrium theory' applies to the word 'equilibrium' rather than 'theory', general equilibrium theorists used the rhetoric and appeal of general theorising. A mathematicised 'general theory' in macroeconomics was placed alongside a mathematical 'general equilibrium theory' as its microeconomic complement. The rhetorical battle was won. As Terence Hutchison (1981, p. 249) put it: 'Keynes's work was treated as, and indeed largely re-established, *general* macroeconomic theory, as complementary with general microeconomic theory.'

The effect of this 'general theory' rhetoric in the literature in economics was dramatic. Prior to 1936, among the leading Anglo-American journals in

10 In his letter to Roy Harrod of 16 July 1938, Keynes (1973b, p. 299) wrote: 'In economics . . . to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought.' For more on Keynes's critical views of econometrics and mathematical modelling see Moggridge (1992, pp. 621-3).

economics, the phrase 'general theory' appeared in the title of two articles only.¹¹ A few book titles or subtitles carried the phrase, as in Knut Wicksell's *Lectures* (1934). After the appearance of the *General Theory*, these two words appeared in a stream of articles themselves discussing Keynes's book. Subsequently, there appeared a steady flow of mainstream books and articles, each claiming to construct a 'general theory' of some kind or another.¹²

General theorising became the vogue, in economics and elsewhere. Under the leadership of Talcott Parsons, sociology had already taken this road. At an even more abstract level, 'general systems theory' attempted a general theory of all human and non-human systems (Bertalanffy, 1950). In the United States, the Society for General Systems Research was founded in 1954.

Of course, Keynes was not responsible for all these outcomes. Nor were they all of negative worth. It is argued in this book that there is a role for general theorising in the social sciences and much can be learned from systems theory and other developments. The problem was that Keynes's use of the 'general theory' term to analyse what were highly specific historical circumstances helped to obliterate all consideration of the problem of historical specificity from economics. Furthermore, it helped to create the postwar synthesis between neoclassical general equilibrium theory and postwar macroeconomics.

Because of his influence and brilliance, Keynes was a bridge between the interwar and the post-1945 eras. He was associated with postwar policies that were designed to avoid a repetition of the Depression of the 1930s. This span between the interwar and postwar epochs was adorned with his name and shaped by his rhetoric. However, the historicists of Germany and the institutionalists of America were barred entry to the new era. They also became lost from the rewritten history of ideas.

As well as giving unintended succour to the rising neoclassical generation, Keynes's use of the 'general theory' phrase also hindered the critics of mainstream economics. Those heterodox economists, attempting to keep the radical theoretical message of Keynes alive, were also diverted from the problem of historical specificity. This was no less true at Cambridge itself. From the 1940s to the 1980s, Keynes's followers dominated the Faculty of Economics at Cambridge. Nicholas Kaldor, Joan Robinson and Piero Sraffa contributed to the reputation of Cambridge as an international centre for heterodox economics. Despite some significant and enduring recognition of the importance of history and institutions, the particular theoretical and methodological problem of historical specificity

¹¹ This search of article titles was done on the JSTOR internet database of leading economics journals. The two pre-1936 articles are Zinn (1927) and Knight (1928a). Among journals of economics prior to 1936, the JSTOR database covers the *Quarterly Journal of Economics* (1886), the *Economic Journal* (1891), the *Journal of Political Economy* (1892), the *American Economic Review* (1911), the *Review of Economics and Statistics* (1919), *Econometrica* (1933) and the *Review of Economic Studies* (1934). (The year of foundation and first inclusion is given in brackets.)

¹² For example, Wald (1947), Isard (1949), Hansson (1952), Mishan (1952), Pen (1952), Lipsey and Lancaster (1956), Chamberlin (1957), Debreu (1959), Lange (1965), Harsanyi (1966), Vanek (1966, 1970), Arrow and Hahn (1971), Olson (1986), Day (1987), Ghiselin (1987), Harsanyi and Selten (1988), Lindenberg (1990), Rosser (1991), Woo (1992).

was largely forgotten. Recognition of the importance of history, or even of historically specific institutions, does not amount to recognition of the analytical problem of historical specificity. What the post-Marshall Cambridge theorists failed to address adequately was the methodological problem of building theories that related explicitly to specific historical and institutional circumstances.

Crucially, the legacy of historicism and institutionalism remained largely untouched at Cambridge. Kaldor continued to emphasise the influence of his teacher, Allyn Young, but made little of the fact that Young was a product of a substantial American institutionalist tradition. Long after its heyday, Joan Robinson stumbled belatedly across some remnants of American institutionalism. In her book *Economic Philosophy* (1964, pp.103-7) there is a positive appraisal of Clarence Ayres's *Theory of Economic Progress* (1944). In about 1970 she came across Veblen's (1919, pp. 185-200) critique of neoclassical capital theory and concluded that Veblen was 'the most original economist born and bred in the USA' (Robinson, 1979, p. 95). It is a pity that earlier generations of students of economics at Cambridge were not pointed in this direction. It is ironic that a university that makes so much of tradition, made so little use of the long and established intellectual traditions that were at hand .3

'POST KEYNESIANISM'

When Joan Robinson, Paul Davidson, Sidney Weintraub and Alfred Eichner worked together in the early 1970s to establish an anti-neoclassical stream of economic thought, they chose the label of 'Post Keynesian economics' (Lee, 2001). This label, like Keynes himself, also fostered a neglect of earlier and allied traditions of economics.

Keynes (1936, p. viii) himself wrote of his 'long struggle of escape . . . from habitual modes of thought and expression'. Ironically, Post Keynesianism itself has faced a 'long struggle of escape' from the *General Theory* title, its claimed ahistorical foundation in universal psychological laws, and its problematic first chapter.

Despite the 'general theory' phraseology, several leading Post Keynesians have been instinctively aware of institutional and historical specificities. Robinson (1974) repeatedly emphasised the importance of 'historical time'. Davidson (1980) likewise understood that 'the economy is a process in historical time' and economic and political institutions 'play an extremely important rôle

13 In his memoirs of the university in the 1940s and 1950s, H. Johnson painted a very critical picture of Cambridge academic life, affected by the arcane and parcellised culture of the system of autonomous colleges, and with consequent and disproportionate institutional bias towards undergraduate teaching rather than postgraduate research. Lively intellectual debates there certainly were, but according to Johnson these were often 'only a tool for furthering left-wing politics at the level of intellectual debate' (Johnson and Johnson, 1978, p. 150). If we place his account alongside the Cambridge neglect after Marshall of entire traditions of economic thought -including historicism and institutionalism- then there is a case to answer of academic deficiency. On the Cambridge environment see also Winch (1969) and Tribe (2000).

in determining real-world economic outcomes. Eichner (1979, p. 172) clearly argued that Post Keynesian economics must concern itself with 'the behavior of the system as a whole, constituted as a set of historically specific institutions'. Indeed, Eichner had a much better acquaintance than most with institutional economics. However, none of this amounted to an explicit recognition of the limits of general theorising or the problem of historical specificity. As elsewhere, there is little discussion of this methodological problem in the Post Keynesian tradition. Praiseworthy historical and institutional instincts would have been enhanced by an awareness of past debates on the problem.

This methodological failure helped to undermine any focus on 'historically specific institutions'. This was particularly the case when leading Post Keynesians endorsed Keynes's generalist methodology. For example, Davidson (1994, p. 15) defended the idea that the work of Keynes and his followers provided 'a more general theory of the economy since it requires fewer initial axioms'. But, as argued in chapter 1 above, such a general theory would necessarily exclude many assumptions that were grounded on historically specific institutions. Davidson (1996, pp. 52-4) argued for 'the minimum axioms needed for the general theory . . . applicable to *all* economic regimes of money-using systems' and for the exclusion of any additional assumptions. Accordingly, the *General Theory* would be so general as it would encompass all monetary economies, spanning the two thousand years or more when money has been in use, and would have no special focus on the key institutions specific to modern industrial capitalism.¹⁴

In defence of Keynes, Anna Carabelli (1991, p. 116) wrote: 'For Keynes, a . . . theory which, at the beginning of its analysis, avoided introducing limiting assumptions of independence, was truly general.' This argument suggests that everything must be conceived as depending on everything else. However, as we have seen in chapter 1, the assumption of a monetary economy rules out a whole set of pairwise and barter interactions. A key point about institutions is that some interactions or interdependencies are ruled out. For example, a language acquires meaning because it has restrictive rules of utterance and syntax. It is in the nature of specific institutions, laws and rules that some things are restricted, prohibited or unyielding. Accordingly, just as the presumption of a monetary economy would involve more restrictive assumptions compared with a theory of barter, the adequate representation of other institutional specificities may require *more* rather than 'fewer restrictive assumptions'.

Lange and Schumpeter were in error to describe Walras's theory as entirely general, because it included several restrictive assumptions. However, Keynes, Carabelli and Davidson were doubly wrong - in claiming that the *General Theory*

¹⁴ Davidson endorsed Keynes's attempts at general theorising while he repeatedly emphasised the non-ergodic character of economic processes. In interpretations of this concept of non-ergodicity, reality itself is both changing and mutable (Davidson, 1993; J. B. Davis, 1998). Yet if economic structures can take radically different forms then these could place ontological limits on general theorising; it would also point to a more confined and historically specific domain of analysis, acknowledged insufficiently by Keynes and Davidson alike.

was truly general and in claiming that any form of generality is necessarily a positive attribute. Notably, all five of these economists were misled by the lure of a general theory. They all failed to observe that a theory designed to apply to a more particular real domain may be more adequate in its analysis of the distinguishing characteristics of the type of economy in question. What Keynesians required was not a general theory but a historically sensitive theory of a modern, monetary, corporate capitalist economy.

To some extent, the Post Keynesian label itself encouraged attempts to build a new or extended 'general theory', against the warnings, before and after Keynes, of members of the historical and institutionalist schools. It even became acceptable for those non-mainstream economists attempting to build a rival paradigm to sport the 'general theory' phrase in the titles of their own works. Hence another group of Post Keynesians avoided the institutional specifics and developed ostensibly general theories, professedly to enhance the 'general theory' of Keynes.¹⁵

Amnesia took hold. The 'Post Keynesianism' label helped to seal off the valuable pre-Keynesian heritage from view. The German historical school was largely forgotten. The history of economic thought was reconstructed largely in Anglophone terms. Clearly, other economists are not exempt from these criticisms. In the latter part of the twentieth century, not only neoclassical and Austrian, but also institutionalist and Marxist economists alike, have generally neglected the problem of historical specificity. What I am concerned to identify here is the crucial failure of Keynes and his Cambridge followers to take the problem on board. In Cambridge after Marshall, the problem was ignored. Given the role that Cambridge played as an international centre for non-mainstream economics, this neglect was disastrous, not only for Cambridge but also for economics as a whole.

Crucially, at least in the early years, Post Keynesianism lacked any developed methodological foundations. In their calls for 'realism of assumptions', leading Post Keynesians were evidently unaware of the twists and turns of the methodological battles on this theme, which had lasted for well over a hundred years. Eichner (1983, p. 211) took the untenable position that all the assumptions of a theory had to be 'empirically validated'. Similarly, Robinson (1964) believed that all 'metaphysical' assumptions had to go, simply because they were 'metaphysical'. This was an empiricist rejection of everything metaphysical - a position that had been increasingly criticised by philosophers of science since the decline of logical positivism in the 1950s. It has been argued already in this volume, when evaluating the weaknesses of the older historical school, that all science

¹⁵ Notably, Robinson followed Parsons and entered the out-generalising the *General Theory* race, with the publication of a volume including an essay entitled: 'The Generalisation of the General Theory' (Robinson, 1952). However, this essay tells us little of what 'generalisation' might mean. Also in Cambridge, Sraffa (1960) offered a general theoretical foundation for the analysis of profit oriented, market economies. Post Keynesians who have attempted to develop a general theoretical framework on Sraffian lines include Pasinetti (1981), Eatwell and Milgate (1983) and Nell (1998). In addition, the phrase 'general theory' is symptomatically included in the titles of the following non-mainstream works: Roemer (1982), Nell (1998), Ormerod (1998) and Hunt (2000).

unavoidably depends on some assumptions that are both 'metaphysical' and cannot be 'empirically validated'. Unlike Robinson and Eichner, Marshall understood this very well. Eichner and Robinson fostered a version of empirical realism that could have benefited from an awareness of its trials and severe limitations in past debates, that had already lasted for well over a hundred years.

It would perhaps be an exaggeration to quote Hegel that the only thing that we learn from history is that people do not learn from history. But one thing is tragically clear. Not only was Post Keynesianism originally founded on weak and undeveloped methodological foundations, but also, by the close of the century, 'Post Keynesian' economics had still failed to provide itself with an agreed and sufficient set of common core principles around which dissidents could gather. This omission might well prove fatal.¹⁶

Since then the lure of a 'general theory' has become almost universal. By the 1980s, general equilibrium theorists such as Frank Hahn were dominating the Faculty of Economics at Cambridge. By the close of the twentieth century, the distinctive Cambridge tradition in economics - stretching from Marshall through Keynes, to Robinson and Kaldor - had dramatically declined in influence. Cambridge no longer regarded itself as the vanguard, and sought instead to emulate the leading neoclassical departments of economics in the United States of America.

¹⁶ See Harcourt (1982) for a relatively early consideration of this problem. Much later, Walters and Young (1997) claimed that Post Keynesianism lacked a coherent foundation. The responses of Arestis et al. (1999) and Dunn (2000) involved a belated and as yet incomplete endeavour to provide Post Keynesianism with a coherent methodological and theoretical core. Notably, this was attempted by excluding approaches - such as Sraffian economics - that were formerly described as Post Keynesian. It also involved the adoption of the 'critical realism' of T. Lawson (1997) and others. But there has been significant disagreement among Post Keynesians on whether Lawson's dismissal of mathematical economics and econometrics should be followed.

16

THE TRIUMPH OF BARREN UNIVERSALITY

The unities, sir . . . are a completeness - a kind of universal dovetailedness with regard to place and time.

(Charles Dickens, *Nicholas Mckleby (1839)*)

In preceding chapters we have observed the growing lure of general theorising in economics in the nineteenth and twentieth centuries. The idea became established that the principles of economics must be universal in scope: they must apply to all types of economic system and to all historical periods. As Philip Mirowski (1989) and others have discussed in detail, the desire of economists to emulate physics and other seemingly universal 'hard sciences' is part of this story. Developments in mathematics were also important. The development of the integral calculus and the ascension of the field theory concept encouraged and enabled the search for universals (Potts, 2000). These ideas penetrated economics in the 1870s and began to power an institutionalised engine of formalisation that accelerated after the Second World War and eventually transformed the whole subject. Historicism did not survive this transformation. This formalist revolution eventually converted 'the whole of economics into a branch of applied mathematics' (Blaug, 1999, p. 276).

There are many examples of attempts to show or claim empirical regularities in economics, analogous to the fundamental constants of physics. For example, part of Milton Friedman's (1956) rhetoric for his quantity theory of money was the assertion of the existence of a stable relationship between the stock of money and prices (Mayer, 1997). The promise of such a fundamental and transhistorical regularity in economics was hard to resist for an economics imbued with the spirit of universalism and the metaphors of physics. Modern monetarism was thus born.

As the claims of universality for mainstream economics became ever more forceful, pressure was imposed on any subdiscipline in which some elements of institutional and cultural specificity had been retained. Postwar economic history dwindled in independence and stature to the point where it felt necessary to prove its virility by adopting mainstream econometric techniques. Similarly, development economics had emphasised the importance of cultural and institutional differences, until it too was taken over by the proselytisers of the 'rational

peasant' as a manifestation of universal 'rational economic man'. The universalising thrust has become so powerful that it has affected not only every branch of economics but sociology and politics as well.

At the core of this drive for universality within economics was the idea of the utility-maximising agent. One of the major tools of general theorising in twentieth-century economics has been its concept of rationality. As the scope of the concept has been both broadened and its content refined, the claims of mainstream economics to a general theory have marched forward with increasing confidence, to the point where they lay claim to the territory of the entire social sciences and beyond.

Earlier neoclassical economists, such as Alfred Marshall and Vilfredo Pareto, made it clear that economics was concerned with the more deliberative and calculative aspects of human behaviour. Marshall (1949, p. 17) wrote that 'the side of life with which economics is specially concerned is that in which man's conduct is most deliberate.' Pareto (1971) saw economics as being concerned with 'logical' actions, namely those where means are logically related to ends. Pareto (1935) also devoted himself to a quite separate science of sociology, claiming that this, in contrast, dealt with 'non-logical' action. From both the Marshallian and the Paretian points of view, economics was not an all-encompassing social science. It was concerned with particular kinds of activity or behaviour.

Philip Wicksteed defined the domain of economics differently. He argued that the distinctive feature of 'an economic transaction is that I am not considering you except as a link in the chain' (Wicksteed, 1933, p. 174). In other words, economics was the study of relatively impersonal transactions. However, although the lines of demarcation were different, economics was still confined in its scope. A legitimate place was accorded to other social sciences.

As discussed in chapter 14, Lionel Robbins (1935, p.16) began to change things radically with his new definition of economics as 'the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses'. This forced sociologists such as Talcott Parsons onto a different tack. Sociology was to be the study of the formation of ends, economics of the means to attain given ends. A key difference in this new demarcation was that there was no longer a domain of social activity that was in principle free from the clutches of 'economics' as Robbins had defined it. Robbins explicitly denied that economics was concerned with specific domains of enquiry, such as money, prices and markets. The perception of a boundary between the 'economy' and 'society' was no more: Henceforth, economics and sociology were both to concern themselves with different aspects of all human activity.

It then became easier to separate completely the concept of utility from the idea of price or monetary value. Although money values could occasionally be used as surrogates for levels of utility (with the assumption of a constant marginal utility of money) this did not have to be so, especially in the field of pure theory. Furthermore, Paul Samuelson (1938) and others insisted for a time that economics could base itself on the claims of 'revealed preference' alone, and did not need to invoke any psychological theory of human behaviour (Lewin, 1996). Following behaviourist psychology, Samuelson argued that all that mattered was the

behaviour itself. Explanations of psychological processes were not required. Like sociology under Parsons, mainstream economics then saw itself as largely independent of any psychological postulates.¹

Similarly, the assumption of deliberate or conscious choice was also regarded by some as inessential and unnecessarily restrictive, and removed from the theory (Machlup 1946,1978; Friedman,1953). As Ian Little (1949, p. 90) remarked, as a result of these developments, 'a theory of consumers' demand can be based solely on consistent behaviour' rather than consumer propensities or plans. All that was required was that behaviour appeared to be consistent: in which case a fixed preference function could be imputed that would satisfy the standard axioms of utility theory.

Once the core axioms of mainstream economics were reduced essentially to 'consistent behaviour' then the door was open to the removal, not only of psychology, but also of real economic and social institutions from the picture. By the 1960s this process was largely complete. Not only economics but also sociology was affected.² This chapter assesses the consequences of these developments in the social sciences.

The theory of 'rational choice' has been held up as the theoretical jewel in the neoclassical crown. It comes in various versions, but the central idea is that we may model individual behaviour in terms of a given preference function, in which agents maximise their 'utility'. This function specifies the amounts of utility yielded from each combination of specific inputs. Each input enters as an argument in this function. These inputs can be standard consumer goods or services but can in principle include other items, such as the 'human capital' of the consumer, or the utility of others, or the available 'social capital' (Becker, 1996). It is assumed that individuals make the 'rational choice' that maximises their utility according to the options available. The whole approach is to explain human behaviour simply on the basis of such preference functions, given limited resources and other constraints.³

Note that this general approach does not even tell us whether the individual will behave in a selfish or altruistic way. In his modern guise, rational economic man is not necessarily a selfish hedonist. The possibility of a type of 'altruism' is admitted because the individual may have a preference function in which extra utility is gained from the enhanced utility of others (Collard, 1978). The giving of a gift can mean a net gain in utility for the giver: the loss of utility resulting from the loss of the gift is compensated by a gain in utility resulting from the observation of the increased utility of the recipient. In this way, 'altruism can

1 Although Samuelson's 'revealed preference' theory is now widely regarded as a failure (Majumdar, 1958; Sen, 1973; Wong, 1978), it nevertheless had this lasting effect.

2 Mouzelis (1995, p. 5) complained of the surfeit of 'transhistorical, universalistic statements' in sociology which do not take into account 'history and context' and 'tend to be either wrong or trivial'. One of his prime exemplars was rational choice theory.

3 In contrast, Buchanan (1969) and others have argued that a choice is only meaningful if there is a possible alternative. We must have been able to 'act otherwise'. According to this view, the utility-maximisers of neoclassical economics are more like programmed automata than real humans.

be ostensibly 'explained'. Rational choice theorists do not have to confine their models to greedy agents who simply maximise their own assets⁴

This relentless quest for universality has led to what is described by its practitioners as 'economic imperialism'. This refers to the invasion of other social sciences by utility-maximising 'economic man'. It is argued that the core assumptions of neoclassical economics should be applied to a wide variety of fields of study, including politics, public administration, sociology, anthropology, psychology, history and even biology, as well as economics itself. The case for the conquest of other social sciences and biology by neoclassical economists rests on the presumed universality of such ideas as scarcity, competition and rational self-interest.⁵

However, in their enthusiasm for economic imperialism, the advocates of the universal rational economic organism eventually settled on a definition of rationality that was unfalsifiable. The concept had become so elastic that any circumstance could fit it. This outcome is explored further below.

MAKING PREDICTIONS

Utility theory is often justified on the claim of its capacity to make predictions. According to Milton Friedman (1962, p. 13) 'economic theory proceeds largely to take wants as fixed'. The economist then makes predictions on the basis of this assumption. The legitimacy of this abstraction then allegedly rests on its 'power to predict'. Countless models have been developed on the basis of the utility-maximising or 'rational' choice. Some of these models generate falsifiable predictions. Others do not.

Consider the attempts to apply rational choice models to political phenomena. Some early models in this vein predicted a zero turnout in democratic elections. The reasoning was as follows. With a sufficiently large number of voters, the costs of voting outweigh any positive marginal expected benefit of the electoral outcome to the voter, so there would be no net incentive to vote (Riker and Ordeshook, 1968). The fact is that large numbers of people do vote voluntarily in elections. The prediction of the model is manifestly false.

However, although the particular model may be falsified, this evidence does not in any way refute utility maximisation or rational choice theory. It refutes one model only, which is based on the assumptions concerning the particular specification of the utility function. For instance, it was assumed that people gained utility from political outcomes, and not from simply exercising their duty to vote. Subsequently, political theorists have had little difficulty in constructing

⁴ However, in this formulation, the 'altruistic' agent is still maximising his or her own utility. It could be argued that true altruism would occur only if we gave to others and made ourselves worse off in net utility terms.

⁵ Prominent extensions to biology include Becker (1976b) and Hirshleifer (1977, 1985).

On 'economic imperialism' see also Radnitzky (1992), Radnitzky and Bernholz (1987) and the critiques in Nicolaides (1988) and Udéhn (1992).

different rational choice models that generated predictions that got closer to voting turnouts in the real world. For instance, it could be assumed that people are getting a substantial amount of utility simply from placing their vote. Tune the utility function appropriately, and we get a closer approximation to the empirical data on real world behaviour.

The point being made here is not that rational choice or utility theory is either refuted or confirmed by the evidence. The point is that utility theory can be used to make falsifiable predictions, but only when particular auxiliary assumptions are made. As Mark Blaug (1992, p. 232) observed: 'The rationality hypothesis by itself is rather weak. To make it yield interesting implications, we need to add auxiliary assumptions.' These add-on assumptions may concern the shape and arguments of the utility functions, the nature of the constraints, the existence of uniformities between agents, and so on. It is these additional assumptions that do the predictive work, not the assumptions of rationality or utility-maximisation *per se* (Shaper, 2000). By this argument, utility theory is not necessarily wrong. But it is manifestly inadequate. Utility theorists demonstrate these inadequacies themselves when they always have to bring in additional assumptions to make any meaningful empirical prediction.

Typically, mainstream economists disfavour the use of *ad hoc* assumptions. Their aim is often to remove all *ad hoc* assumptions, in the pursuit of universality. However, it is only with such additional and *ad hoc* assumptions that rational choice theory can become operational and falsifiable.

Take another example. Many economists believe strongly in the 'law of demand' - it holds that demand curves are always downward sloping. However, on its own, utility analysis does not show this. It is a familiar textbook exercise to show, with indifference curves, how demand schedules for goods may slope either upwards or downwards. We have at least the theoretical possibility of 'Giffen goods', of which more is bought as their price rises.⁶

Whether Giffen goods can exist in the real world is in dispute. George Stigler (1987, p. 24) boldly asserted that the 'law of demand' is 'really true of all consumers, all times, all commodities'. Reviewing much of the evidence on this, Blaug (1992, pp.140-7) likewise showed that no unequivocal case of a positively sloped demand curve has ever been found. However, this is not a triumphant vindication of neoclassical microeconomics, but instead an illustration of its theoretical shortfall. If the empirical evidence is as conclusive as Blaug has suggested, then the theory must be criticised for failing to be so conclusive in explaining it. Without additional assumptions to close off the perceived theoretical 'anomaly' of upward-sloping demand curves, no adequate basis for the 'law of demand' can be found in standard utility theory.⁷

6 In addition, Stiglitz (1987) has shown how the introduction of deficiencies of information can overturn the 'law of demand'. General equilibrium theorists have also exposed problems in deriving aggregate demand relationships on the basis of individual preferences (Kirman, 1989; Rizvi, 1994a).

7 In a very interesting but neglected paper, Heiner (1986) criticised mainstream economics for failing to find a theoretical justification for a universal 'law of demand'. Heiner himself provided such

A related case is the tenacious belief that the demand curve for labour is always downward sloping. Not only goods, but also factors of production such as labour, are said to yield to the universal law of demand. But the theoretical arguments in favour of this are again limited. The downward-sloping demand curve for labour is derived in part from the presumption of the diminishing marginal productivity of labour. But again there is no reason given to support the notion that the conditions giving rise to this effect are universal.

Despite this theoretical lacuna, the belief in universal downward-sloping demand curves for labour is often used as a theoretical basis for the ideological pronouncement that wage increases can 'price workers out of a job'. An anecdote shows the tenacity of this belief among economists. In 1995 the economist David Card was awarded the John Bates Clark Medal by the American Economic Association for his detailed empirical work showing that modest increases in the minimum wage had little or no discernible effect on the employment of low wage workers (Card and Kruger, 1995). This evidence undermined the notion of a downward-sloping demand curve for labour. It received hostile and extreme criticism. Card was lambasted in *Business Week*, in the *Forbes* journal, and by leading economists such as Thomas Sowell and Nobel Laureate James Buchanan (Deaton, 1996, p. 13). Card's work was regarded as heresy, and declaimed as scientifically unsound. For a significant group of economists, the sanctity of the allegedly universal 'law of demand' had to be retained, in labour and non-labour markets alike.

THE NON-FALSIFIABILITY OF THE THEORY

Perhaps a fundamental 'predictive' claim of utility theory is that the substitution effect is negative. Again, the detailed argument can be found in the neoclassical textbooks. This shows that if a price increase occurs, and compensation is made for any change of 'real' (i.e. utility) income, then the demand for the good or service will decrease. Conversely, a price decrease will lead to a demand increase, under similar compensatory conditions. The proof of the negativity of the substitution effect follows directly from the assumptions of the theory (Hicks, 1939).

Can the negative sign of the substitution effect be used to predict human behaviour? Is it a falsifiable prediction? Regrettably, the answer to both questions is no. *Any observed behaviour can be fitted into the theory.* If the price increases and demand also goes up, then that does not contradict the theory. In this case it could simply be said that the 'real' income (measured in terms of utility rather than prices) is not constant. If we were to make an adequate income compensation, and assume a sufficiently lower 'real' income before the price change, then the

a theoretical justification, using something very different from utility analysis. Another justification of the 'law of demand' without using utility theory is found in Becker (1962). For another novel approach see Hildenbrand (1994). Yet these arguments are rarely cited: they remain neglected and underdeveloped.

apparent anomaly would disappear. We are free to make a wide range of assumptions concerning the imagined compensation. The compensation has to be such as to place the individual at exactly the same utility level, before and after the price change. But we do not know this utility level, or the shape of the indifference curve!

The compensation is thus a thought experiment, rather than an investigation into processes in the real world. It is difficult to make reality an adjudicator in this thought experiment, because we cannot directly measure utility. The high degree of compensatory discretion makes the theory untestable in terms of its behavioural predictions. The result may have the aesthetic appeal and the apparent universality of a mathematical theorem, but it does not enable us to make any prediction that can be falsified by any possible outcome in the real world.

Experimental psychologists such as Daniel Kahneman, Paul Slovic and Amos Tversky (1982) have thrown down experimental challenges to expected utility theory. More broadly, since the 1980s there has been a spectacular growth in interest in 'experimental economics'. Many people have interpreted the behavioural evidence gathered by the experimenters as a violation of the standard axioms of expected utility theory. Much of this evidence, particularly concerning choices under risk, has led some mainstream theorists to reflect critically upon the standard assumptions of their theory. This evidence is important and it should be taken seriously.⁸

However, if we were to think that the evidence itself refutes or falsifies the core axioms of utility theory, then we would be mistaken. The reason is that the standard core of utility theory is *non falsifiable*. As Sidney Winter (1964, pp. 309, 315) argued in an early and neglected article: 'any behavior can in one way or another be rationalized as maximizing behavior.' Lawrence Boland (1981) expanded on this theme in another important paper. With the provocative title 'the futility of criticizing the neoclassical maximization hypothesis', his essay was first widely misinterpreted as a defence of a theory that the mainstream economists had already accepted and taken for granted. Consequently, Boland's paper is now largely forgotten.⁹

In fact, it is better understood as a *critique* of the maximisation hypothesis. In his paper, Boland asked if any conceivable evidence would refute the maximising assumption. He then showed that such an attempt at falsification could never work:

Given the premise - 'All consumers maximize something' - the critic can claim he has found a consumer who is not maximizing anything. The person who assumed the premise is true can respond: 'You claim you have found a consumer who is not a maximizer but how do you know there is not something which he is maximizing?'

(Boland, 1981, p. 1034)

⁸ For summaries of the issues and debates in experimental economics see Kagel and Roth (1995). The debate is taken further by Binmore (1999), Loomes (1998, 1999) and Starmer (1999a, 1999b). ⁹ See Boland's (1996) own later reflections on the misinterpretation of his argument.

Given that we can never in principle demonstrate that 'something else' (perhaps unknown to us) is not being maximised, then the theory is ultimately invulnerable to any empirical attack. To show empirically that nothing is being maximised we would have to measure every possible variable that could impinge upon humanity, from the changing of the weather to the twinkling of the stars. Clearly, this would be an endless and impossible task. As Boland (*ibid.*) concluded: 'The neoclassical assumption of universal maximization could very well be false, but as a matter of logic we cannot expect to be able to prove that it is.' Boland showed that the neoclassical assumption is not falsifiable. But he also rightly points out that it is not a tautology. It is not a tautology because it is *conceivably false*. It might be the case that nothing is being maximised. But we can never know.¹⁰

The arguments of Winter and Boland have been much neglected. They do not rule out the role of evidence in evaluating the theory, but they show that the evidence alone cannot be decisive. Boland also warns us that utility maximisation is not 'tautological'. Strangely, some critics regard the allegation of 'tautology' as a damning weakness. On the contrary, a tautological theory, whether it is 'empty' or not, must be accepted as valid. By saying that utility maximisation is not a tautology we are admitting the possibility that it is false, although no single piece of evidence can show that it is untrue.¹¹

In some respects, Boland's argument resembles the so-called Duhem-Quine thesis.¹² This thesis derives from the work of the French physicist Pierre Duhem and the American philosopher Willard van Orman Quine (Harding, 1976). According to this thesis, it is not possible to falsify a single hypothesis because we are always faced with a tangle of related and connected hypotheses. Consequently, we can never be sure that the main hypothesis is being targeted and tested on its own, and that other auxiliary hypotheses are not complicating the picture. Boland, Duhem and Quine all pointed to the multiplicity and

¹⁰ Boland can be misunderstood, unless his strong Popperian inclinations are acknowledged. He alleged that it is 'futile' to criticise the theory because it is 'non-falsifiable' and thereby 'metaphysical'. By the famous Popperian criterion, this also means that it is 'non-scientific'. This is the understated and impish outcome to his argument. Where Boland was vulnerable was not in the demonstration of unfalsifiability but in his excessive faith in the Popperian criterion. For Boland, 'criticism' would usefully be directed at falsifiable statements only - and the main means of criticism would be empirical falsification. In response, Caldwell (1982) showed that Boland's demonstration of the 'futility' of criticising the hypothesis rested upon an overly narrow notion of criticism. Caldwell argued convincingly that it is also possible to criticise some non-falsifiable statements, for instance by looking at their underlying assumptions. Caldwell was right to suggest that the appraisal of theories must deploy a number of additional criteria, and not pin everything on falsification. Nevertheless, Boland's central result - that no imaginable evidence can in principle falsify the theory - still stands.

¹¹ Etzioni (1988) made a strong case that commitments to moral values should be considered as part of a theory of human behaviour. However, his allied critique of utility theory was weakened by the allegation that it is 'tautological' (p. 28) and that neoclassical theory applies to market behaviour (p. 3). As noted above, the former proposition is false. In addition, knowledgeable economists such as Clower (1994, 1999) have rightly rejected the latter proposition.

¹² Some of the implications of this thesis for macroeconomics were discussed by Cross (1982). Cross usefully reviewed some of the attacks on the Duhem-Quine thesis and concluded that it has 'withstood criticism' (p. 322).

interconnectedness of possible causal influences behind any empirical phenomenon in the real world, and the general difficulty of isolating and testing them all.

Just as we cannot isolate every connected and auxiliary hypothesis, we cannot consider all the possible hypothetical variables that could be maximised. As a result it can be argued that there is no experimental or other phenomenon that cannot in principle be 'explained' by the theory. Nothing lies outside its scope. Even the so-called anomalies revealed by experiments with human subjects can be explained away. If experiments show that some consumers appear to prefer a monetary reward that is less than the expected outcome, or appear to have intransitive preference orderings, then we can always get round these problems by introducing other variables.¹³

For instance, if an experiment shows that option A with an expected value of \$4 is preferred to option B with an expected value of \$5 then we can simply assume that there are additional attributes of option A (for example, we may enjoy losing, or gain pleasure from seeing others win) that are consistent with the view that it yields higher overall utility for the subject. Likewise, an experiment may seem to reveal preference intransitivity, by showing that while X is preferred to Y, and Y is preferred to Z, Z is preferred to X. Even this result can be explained away by showing that the three pairwise comparisons did not take place under identical conditions, or were separated in time or space. Accordingly, the consumer could have 'learned' more about his or her true tastes during the experiment itself, or other factors may account for the apparent intransitivity. All we have to do is indicate in some way that the two Zs in the above comparisons are not quite identical. The two Zs could be slightly different in timing, substance, or their informational or other contexts. We then get the result: X is preferred to Y, Y is preferred to Z1, and Z2 is preferred to X. Transitivity is no longer violated.

It is also claimed that preference reversals are inconsistent with expected utility theory. 'Preference reversals occur when individuals are presented with two gambles, one featuring a high probability of winning a modest sum of money . . . the other featuring a low probability of winning a large sum of money' (Slovic and Lichtenstein, 1983, p. 596). Assume that a subject is faced with a choice between \$15 with certainty, and \$1,000 with a probability of 2 per cent. Experiments with real subjects indicate that in such situations the first, \$15 option is sometimes chosen (Kahneman et al., 1982; Slovic and Lichtenstein, 1983). This is despite the fact that the expected value of the second option is higher at \$20. However, preference reversals also fail to falsify expected utility theory, once we accept that utility is not necessarily measured in terms of the monetary payoffs in the experiment. If we assume an added disutility associated with the choice of a risky and low probability outcome, then the theory that people are maximising their utility is not overturned by these experiments. In general, a

¹³ Hausman (1992, ch.13) documented several attempts to explain the apparent anomalies that have been revealed by the experimenters, notably by pointing to other possible sources of utility.

risk-averse actor may not maximise expected monetary value but still be maximising expected utility. By appropriate functional manipulation, the choice of \$15 can be made perfectly consistent with the maximisation of expected utility, rather than the maximisation of the expected monetary value of the payoff.

Experimental economists such as Vernon Smith (1982) and others have addressed the problem of the possible absence of a linear correlation between utility and monetary payoff. In particular, the possibility of additional, subjective utilities - unrelated to the monetary payoffs - has to be diminished. The money payoffs have to 'dominate' the decisions of the agents. To make experiments 'work' in the sense of a close presumed correlation between overall utility and monetary payoff, Smith proposed a number of 'precepts' of experimental assumption and design constituting an 'induced value procedure'. These precepts include nonsatiation, sufficiently large and obvious rewards, restriction of communication between subjects, and so on. But Smith (1982, p. 929) himself was the first to admit that these precepts cannot guarantee any correspondence between observable monetary rewards and preferences that, in principle, are 'not directly observable'. In short, we can never know if the precept has been effectively applied. Accordingly, the most judicious application of Smith's precepts will not banish the problem of non-falsifiability. There is no way of showing that a close correlation between utility and experimental reward has been achieved. The idea that Smith's precepts 'work' is an article of faith, placed so far under relatively little methodological scrutiny.¹⁴

THE BARREN UNIVERSALITY OF RATIONALITY

Accordingly, a problem with the standard rationality assumptions is not that they lack empirical correlation, but that they could cover every conceivable decision situation and every possible causal mechanism underlying choice. Insofar as there may be common features of every decision situation then it may be possible to extract universal and meaningful propositions. Nevertheless, some important and specific features or causal mechanisms may be excluded by concentrating solely on the common features of every decision situation. In fact, the degree of universality involved is so great that it goes beyond the parameters of mere human decision.

Recent theoretical and experimental studies confirm a high degree of universality, beyond the confines of human society. Experimental work with rats and other animals (Kagel et al., 1981,1995) has 'revealed' that animals have downward-sloping demand curves, supposedly just like humans. Gary Becker (1991, p. 307) has argued extensively that: 'Economic analysis is a powerful tool not only in understanding human behavior but also in understanding the behavior of other species.' Similarly, Gordon Tullock (1994) has claimed that organisms - from bacteria to bears - can be treated as if they have the same

¹⁴ For a critical methodological discussion of some of Smith's precepts see Siakantaris (2000).

HOW ECONOMICS FORGOT HISTORY

general type of preference function that is attributed to humans in the microeconomics textbooks. They are all regarded as utility-maximisers. Accordingly, core concepts are not only applied to all forms of human society since the origin of our species, but also to a large portion of the animal kingdom as well. Seemingly, we now have 'evidence' of the 'rationality' of everything in evolution from the amoeba onwards. This suggests that such assumptions are telling us very little about specifically human societies, least of all about the unique complexities of modern human civilisation.

For the neoclassical economist, the fact that utility theory can 'explain' a wide variety of types of economic behaviour is regarded as a strong vindication of this general approach. I take a different view. First, the sheer generality of a theory tells us nothing of its explanatory power. We could conceive of different general theories, such as that we all are programmed by aliens from outer space, or that we are all pawns of God. These would be quite general in their scope and could be applied in principle to any behavioural manifestation. But we would rightly be sceptical of their explanatory value. A theory does not explain anything unless it points to an underlying causal mechanism. In the case of individual behaviour, explanations must thus relate to mechanisms of the human psyche and human interaction and perhaps draw upon psychology, anthropology, sociology and other disciplines. This is precisely what many advocates of utility theory refuse to do. They take the utility functions as given and consign the job of grounding them theoretically to somebody else. By this refusal they indicate that utility theory itself cannot provide a real explanation.

Arguably, human societies are partly differentiated from other animals in terms of developed institutions and cultures. If utility maximising behaviour is not confined to humanity, then these uniquely human elements are downplayed within or absent from the universal picture. Furthermore, whether true or false, this picture can tell us relatively little about historically specific human cultures or institutions. That is the unintended achievement of the exponents of ubiquitous rationality and economic imperialism. The specific causal mechanisms through which human culture and institutions mould and constrain human agents remain largely unexplored in this paradigm. Essentially, there is no adequate theory of human agency at the core of the standard theory. It identifies no uniquely human causal mechanism. Its very weakness stems from the universality it has achieved through axiomatics and deduction, rather than an investigation into causal relations in the real world.

It was noted in chapter 1 that while the laws of physics may be general, they impose restrictions on the type of supplementary theory that may be used to explain particular phenomena. This is not the case with utility maximisation, because any particular behavioural model is compatible with it. It can be made compatible with both selfishness and altruism, with both calculative and unreflective behaviour, with intelligence and stupidity, with both humans and non-humans. The lure of a general theory has led economists to worship a deductive theory that tells us very little about reality.

WEAK CRITICISM AND FALSE APPROVAL

However, many critics of mainstream economics have taken a different line of attack. In a classic critique of formalism in economics, Terence Hutchison (1938, p. 27) argued that the basic postulates of 'pure theory' necessarily suffered from a 'complete lack of empirical content'. Many similar remarks have been made by heterodox economists, before and since. I take a different view. The problem with these assumptions is not primarily their lack of empirical corroboration. It is that they are vessels into which any empirical content can be filled. The problem with the theory is not that it lacks empirical validation but that any conceivable fact about behaviour, from church attendance to suicide, can be fitted into the theory.¹⁵

Just as some critics of neoclassical theory wrongly claim that its basic postulates have been falsified, some of its exponents misleadingly suggest that they have so far been confirmed. Jack Hirshleifer (1985, p. 59) went so far as to write: 'Ultimately we must be ready to abandon the rationality paradigm to the extent that it fails to fit the evidence about human behavior.' However, this apparent concession to empirical confirmation in fact conceals a methodological misunderstanding. Hirshleifer did not have to worry, because no conceivable evidence can 'fail to fit' some tortured version of the theory. Both Hirshleifer and the critics of the rationality paradigm share the flawed supposition: that evidence can in principle refute the theory. Both supporters and critics of neoclassical theory have perpetuated the myth that it is susceptible to decisive empirical testing.

As a result, the mainstream theory is not wrong because it is empirically inaccurate. It is not unrealistic in the sense that it fails to fit the data. Any data can be fitted into it. Hence no data can refute the theory. It cannot be displaced simply by an appeal to the evidence. The experimental evidence of preference reversals and other choice 'anomalies' may lead us to search for a different and better theory, but it does not in principle refute the old version based on utility and rational choice.¹⁶

Critics such as Hutchison (1938) and Alfred Eichner (1983) based their criticism on an untenable and empiricist view of science that denies that some nonfalsifiable and 'metaphysical' assumptions are essential to any science. In fact, all sciences depend upon some propositions that are untestable. No theory can be composed entirely of empirically validated elements. Prior concepts are required to make sense of any fact. These prior concepts cannot all be 'tested' empirically. In any case, any 'test' itself relies on prior concepts or categories. As a result, all sciences must unavoidably make extensive use of some untestable and metaphysical assumptions.

¹⁵ This is no joke. See Azzi and Ehrenberg (1975) and Hammermesh and Soss (1974).

¹⁶ I am not arguing that evidence is unimportant. Although evidence cannot falsify the theory, the accumulated evidence may provide a context in which the theory is more readily questioned. See Loomes (1998, pp. 485-6).

HOW ECONOMICS FORGOT HISTORY

Immanuel Kant (1781, p. 7) revealed that human reason 'begins with principles which it has no option save to employ' but which 'are no longer subject to any empirical test'. Accordingly, he recognised a role for metaphysics. Subsequently, in the heyday of positivism, the idea that metaphysics had any place in science was challenged. But from the 1950s, positivism itself was subjected to strong philosophical attacks. In particular, Willard van Orman Quine (1951) successfully overturned the view that all scientific and meaningful statements had to be based upon empirical experience. The outcome was 'a blurring of the supposed boundaries between speculative metaphysics and natural science' (p. 20). Eventually, Karl Popper also recognised that some metaphysical propositions are essential to science (Ackerman, 1976, pp. 30-1). The indispensable role of untestable and metaphysical assumptions is now widely accepted by philosophers.¹⁷

For this reason, the Hutchison-Eichner empiricist criticism of mainstream economics is untenable. In practice, furthermore, their denial of the essential role of non-falsifiable assumptions in any theory would disable any of their own attempts at theoretical construction. Given that it is practically impossible to test all assumptions, any theoretical construction - including theirs - would reveal hidden, ad hoc assumptions, privileged to lie beyond empirical test. For reasons outlined above, every theory must involve some untestable assumptions. Hence any theory built on the claim of complete testability would be highly vulnerable to critique by its own canon.

However, this does not mean that 'anything goes' and that all criticisms are disabled. There are powerful theoretical criticisms of the rationality assumption (Simon, 1957; Lane et al., 1996). Essentially, the theory lacks adequate theoretical concepts to discriminate, understand and properly explain key phenomena. A problem with the standard assumptions of rationality and expected utility maximisation is their lack of specific theoretical and conceptual content, pertaining to specific causal mechanisms involved in the human psyche and in the structures of specific real world economic institutions.

To repeat: the empirical evidence is valuable and important, but it cannot be used to show that the theory is false. In recent years, there have been attempts to apply models of rational, utility-maximising behaviour to a wide variety of phenomena, even beyond the sphere of commerce and markets. Models of utility-maximising behaviour have been applied to politics, marriage, religion, suicide, and much else. Such attempts have been widely resisted. Many tried to defend their academic discipline or subdiscipline from the 'economic imperialism' of rational choice models. However, the widespread failure to recognise the non-falsifiability of 'rational' maximising behaviour has weakened many such counter-arguments. They appealed to evidence: it was mistakenly argued that rational choice models did not fit the facts. On the contrary, models of utility-maximising behaviour can always be adjusted to fit the facts. The attempt to resist

¹⁷ In the methodology of economics see, for example, Caldwell's (1982) critical discussion of positivism and Blaug's (1992) account of the role of Lakatosian 'hard core' assumptions.

the incursions of rational choice theory by claiming otherwise was bound to fail. In this instance, appeals to evidence cannot be decisive.

In development economics, for example, there was a debate in the 1970s over whether peasants were or were not 'rational'. Critics of this idea appealed to 'evidence' of 'non-rational' behaviour, without realising that no evidence can strictly falsify the theory. With opponents weakened by their own theoretical position and methodological misunderstandings, the rational choice theorists seemed to win the argument (Popkin, 1979). Similarly weak defences were evident in sociology and political science, as rational choice theorists invaded their territory. Again and again, attempts were made to resist the incursions of utility and rational choice, on the grounds that its assumptions are not 'realistic'. But no theory can employ assumptions that exactly mirror the real world. Such attempted rebuttals of rational choice theory are methodologically flawed and ultimately doomed.¹⁸

The moral here is that mistaken claims concerning the testability of rational choice theory led its opponents to attack it with weak arguments. It would have been much more fruitful if both sides had admitted that the theory was unfalsifiable and then debated its explanatory value in particular circumstances. Instead, these controversies were entirely confined to claims and counter claims concerning empirical validation. At that primitive level the issue is simple: the assumptions of utility theory cannot be falsified.

Once again the weaknesses of empiricist criticisms of ahistorical theory are underlined. As we have seen, the failures of empiricism have been widely discussed in German economics, at least since the *Methodenstreit*. By contrast, Anglophone social science was overwhelmed by positivism and has been largely unaffected by these issues. As a result, the criticisms of rational choice theory have been severely weakened.

THE DECONSTRUCTION OF RATIONALITY

However, having almost conquered the social sciences, some of the rational choice theorists have become bored with their own weapons of victory. Ironically, it is beginning to be possible, even fashionable, for mainstream economists to question some of these core assumptions. Perhaps because mainstream economists have lost the capacity to police their own disciplinary boundaries, in search of a new separate identity they have begun to question their own *raison d'être*. As the sociologist Kyriakos Kontopoulos (1993, p. 90) has pointed out: 'Ironically, economists become less economic at a time when sociologists seem to become enamored with rational choice theory.' Accordingly, some economists are now deconstructing rational economic man. As economist Robert Sugden (1991, p. 783) put it:

¹⁸ A selection of the relevant literature could include: Baron and Hannan (1994), Coleman (1990), Coleman and Fararo (1990), Frank (1992), J. Friedman (1995), Green and Shapiro (1994), Hirsch et al. (1987), G. Miller (1997), Orchard and Stretton (1997), Udén (1996).

There was a time, not long ago, when the foundations of rational-choice theory appeared firm, and when the job of the economic theorist seemed to be one of drawing out the often complex implications of a fairly simple and uncontroversial system of axioms. But it is increasingly becoming clear that these foundations are less secure than we thought, and that they need to be examined and perhaps rebuilt.

One reason for this change of heart is the rise of game theory. In certain types of game the very definition of rationality becomes problematic. Nevertheless, the response of mainstream economists to these problems has largely been to become immersed in the technicalities, rather than to give the economic agents at the core of the theory of human behaviour some real institutional and cultural flesh and blood. Some still cling tenaciously to the principles of rationality, in a manner that is reminiscent of Ptolemaic astronomers, fitting the evidence of the apparent circular movements of the stars into complicated models (Koestler, 1959). Others are not inclined simply to 'save appearances'; they express their misgivings but fail to look for an alternative paradigm.

For some, the move to game theory has led to the questioning of core assumptions. For others it has reinforced the idea that economics itself is a formal game, with little connection to reality. If a theory makes no claim outside a single domain, then there is no aim to use the theory to explain other real world phenomena. The interest in the theory is typically in its mathematical content, rather than its usefulness to help understand reality. Accordingly, there is a move away from former attempts to build a universal theory (which turned out to be unfalsifiable), to the building of exemplifying theories that are not designed to be put under any empirical scrutiny whatsoever. There is a move from universal to 'what if?' theories. As Donald McCloskey (1991, p.10) put it, much of modern economics involves 'a search through the hyperspace of conceivable assumptions'. Step by step, much of economics is becoming disengaged with the real world. Instead of looking at real institutional structures and mechanisms, it has become more and more involved in the elaborations of mathematical technique.

For those of us concerned to try to understand the real world, this does not help us get closer to real economic institutions. Even if it has useful applications in this direction (Schotter, 1981), game theory has inherent limitations. Roy Radner (1996) argued that the game-theoretic analysis of institutions is thwarted by problems of uncertainty about the logical implications of given knowledge, and by the existence of multiple equilibria. Furthermore, as Dennis O'Brien (1998, p. 27) has caustically remarked:

in the intoxication of intellectual knitting patterns for which game theory provides scope, important information has to be pushed to one side because game theory seems to have little capacity for dealing with industrial data. With very limited exceptions, the striking characteristic of game theory treatments of industrial economics is the lack of contact with reality.

This goes not only for the assumptions about the structure of the game but also the conception of the human agent that is found within game theory. Herbert Gintis (2000) has criticised the over-use within game theory of the simplistic assumption of selfish 'economic man'. Even more crucially, as Ariel Rubinstein (1991, p. 923) has argued:

Deductive arguments cannot by themselves be used to discover truths about the world. Missing are data describing the processes of reasoning adopted by the players when they analyze a game. Thus, if a game in the formal sense has any coherent interpretation, it has to be understood to include explicit data on the player's reasoning processes.

Similarly Cristina Bicchieri (1994, p. 127) observed that 'a description of the players' reasoning processes and capacities as well as a specification of their knowledge of the game situation was missing in most game-theoretic models. We may draw the inference from these remarks that such processes as cognition and learning - that always take place in, and are moulded by, a specific cultural and institutional environment - are absent from much of game theory. In short, game theory lacks sufficient empirically and historically grounded content. It may develop to overcome these limitations, but there is little sign of this as yet.

What is required to make economic theory sensitive to institutional and cultural realities is the combination of some general, methodological principles to guide analysis, with detailed and empirically grounded theories of how specific types of institution and socio-economic system function. The development of such a theoretical framework is far from complete, but some rudimentary elements will be outlined later in this book.