Abstract:
What needs to be constructed first is a new set of frameworks for thought and action (which we may call, following Hahn, a set of theories and policies) that will allow the development of a new coherent pattern of routines. Elaborating notions of equilibrium based on rationality and excluding uncertainty (a self-liquidating combination in Knight’s view) is unlikely to greatly improve our understanding of how the economy works. As evidence for this we may consider the exiguous treatment of firms, markets, and the organisational structure of economies in so much economics, because all of these rely on institutional arrangements which serve to support the particular abilities of the human mind, and mitigate its disabilities. Moreover, we may recognise, as Marshall did and Schumpeter did not, the essential interplay between regularities and the creation of novelty which has been, I believe, an emergent focus of DRUID.
Imagination, Illusion and Delusion

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*Introduction*

A somewhat less provocative title for this paper would be ‘Imagination and illusion’, for although the ability to create new ideas is a crucial factor in human progress there is no reason why what is imagined, either as an explanation of existing phenomena or as an innovation, should be correct or feasible; indeed most of what is imagined is illusory, for some fundamental reasons to be examined almost immediately. Moreover I wish to argue that illusion is not merely the opportunity cost of imagination; some illusions may have important beneficial consequences, rather than, or as well as, the undesirable or even dangerous consequences which are implied in the word ‘delusion’. That itself is not a proposition that sits comfortably within economic theory, though it is far from novel; but I have preferred to be unequivocally provocative by making a direct reference to Richard Dawkins’ book *The God Delusion*, in which he argues that a particular and pervasive illusion (which takes varying forms) has had and continues to have major undesirable consequences. I have no intention of discussing Dawkins’ particular thesis, although I would just like to make an observation which would, I hope, occur to any well-trained economist: we should at least enquire about the opportunity cost of his prescription to discard all religious ideas. G. K. Chesterton, an earlier polemical writer on the other side of the argument, once observed that ‘when people stop believing in God they do not believe in nothing; on the contrary they believe in anything’, and there is abundant evidence that the consequences of some of these other beliefs may be extremely unpleasant. Dawkins’ alternative, of course is science, but on present indications this would not be the majority choice.

Moreover what Dawkins wants from science seems to require a particular set of psychological and sociological conditions which support a distinctive set of institutional arrangements, which may be summarized as the Mertonian norms of communalism, universalism, disinterestedness, originality, and scepticism (Ziman 2000). Science is a belief system; its claim to superiority over other belief systems depends on its institutional framework. These norms are not guaranteed, as Ziman recognises, and as is quite frequently demonstrated – indeed they are not easily derived from standard representations of interaction between self-interested rational agents; but without them the belief that science produces reliable knowledge may well itself be a dangerous delusion (Ziman 1978). Fortunately, economists often conform more closely to these norms than they would if they acted according to their own theories; explaining why would require a different paper. However, that has not been sufficient to avoid significant illusions within both economic systems and economics as a discipline, with potential (and sometimes realised) consequences that may justify the label of delusion.

Both the importance of imagination and the possibility of delusion arise from the human condition, and specifically from the motivation and means of improving that condition by improving our knowledge, because it is impossible to achieve demonstrably true knowledge about our universe or ourselves. There are three problems. First, as David Hume (1875, p. 33) pointed out, empiricism is not enough, for the validity of any generalisation from a collection of instances, however extensive and carefully observed this collection may be, rests on an assumption that the unobserved instances, from the past as well as the future, would conform to this generalisation, and this can never be more than an assumption. Indeed, much of the
history of science has been driven by the apparent failure of apparently well-attested universal propositions. We are always reliant on appearances – which are constructed by our brains. As Ziman (1978) argues, some of these appearances, having been well-tested, may be extremely reliable; but even science cannot attain certainty, and many of our mental constructions are not rigorously tested. Second, logic cannot be decisive. Although it can establish clear connections between premises and conclusions – provided that these premises and conclusions are precisely-specified, which is sometimes difficult – it cannot generate novel premises; as Hume (1978, p. 164) wrote, ‘no kind of reasoning can ever give rise to a new idea’. The third difficulty was pointed out by Hayek (1952, p. 185). To be a complete and accurate representation, any system of knowledge must be at least as extensive as the set of phenomena to which it applies; and the capacity of the human brain, though very great, is tiny in comparison with what lies outside it – which includes several billion other human brains for a start. It is therefore an illusion to believe that any comprehensible human representation can be correct and complete. Indeed, if one can imagine a single brain which had the capacity to encompass all phenomena, then (as has been pointed out) there would be no need for the generalisations which systems of knowledge provide, because every instance could be separately handled.

It is the essential impossibility of complete knowledge that makes the growth of knowledge so important, and makes an understanding of the process of knowledge creation and revision so desirable. The absence of any such understanding is, in my view, the most important deficiency of the predominant method of economics, a deficiency which has been carried over into endogenous growth theory. The development of modern economics entailed the discarding, sometimes inadvertent but sometimes deliberate, of the understanding which had already been achieved by earlier economists; but this understanding is a valuable resource, on which I shall draw. In the latter part of this paper I propose to discuss the close connection between the process of knowledge creation and the potential for error, including potentially major error. First, however, I wish to consider some important characteristics of human knowledge.

The Nature and Development of Human Knowledge

Adam Smith’s ([1795] 1980) theory of the growth of knowledge respected the limitations set out by his friend Hume. His objective was to explain the emergence of science as an increasingly distinctive category of knowledge from the interaction of human cognition and human motivation. We develop knowledge by creating patterns which we impose on particular groups of phenomena: this is the founding principle of all explanations, whether based on genetics, neuroscience, sociology, organisation theory, or whatever (though not all explanations are clearly expressed in this way). Smith characteristically begins by considering the motivation which drives this development. This motivation clearly cannot be included within a preference function as that is normally conceived in economics, because this function assumes the agent’s knowledge of all relevant objects of preference (though the concept of a preference function is an excellent example of a pattern which is created and imposed on events by economists, and which leads to both knowledge and illusions – notably about the range of motivations and the shaping of wants). For Smith the prime motivation is the discomfort, or worse, occasioned by exposure to phenomena which cannot be accommodated within existing schemes of order, matched by the delight at inventing
some new form of order which will encompass them. In Smith’s language it is the imagination which is first confounded, but then creates a novel pattern which banishes this particular confusion and gives delight. Building up tension and then relieving it is a standard device in artistic creations; it is therefore appropriate that Smith draws attention to the aesthetic appeal of such satisfying patterns.

Because cognition and imagination are scarce resources, knowledge is always domain-limited, although some domains may be fairly extensive. Smith explains how, as the pressures of daily existence are relaxed room is created for more protracted investigations by some members of a community, attempting to go beyond the limitations of mundane knowledge into what gradually comes to be identified as philosophical enquiry, and the subsequent gradual emergence of particular fields of enquiry, in which both problems and solutions tend to become more specific; and he shows how this differentiation of both focus and explanatory principle in turn accelerates the growth of knowledge – which was to become the fundamental principle of the Wealth of Nations. Thus Smith’s explanatory system not only copes with the three problems of knowledge but establishes science as a natural product of human nature and human development. This development we may reasonably call an evolutionary process in the wider sense of the term, not only because it shows how one field of knowledge grows out of another, but because it works by conjecture, selection and perpetuation. These conjectures, generated by the imagination to resolve its own confusion into acceptable order, group phenomena into categories and postulate links between events, in both cases by conceiving new schemes of connection. ‘Philosophy is the science of the connecting principles of nature’ (Smith [1795] 1980, p. 45) and therefore, on Smith’s reasoning, it is necessarily ‘one of those arts which address themselves to the imagination’ (Smith [1795] 1980, p. 46). Smith’s own philosophical system, of course, exemplifies the principles which it expounds.

In the course of his account of the development of astronomy, Smith draws attention to what we might call a crucial experiment. This is provided by the work of Copernicus, where both the motivation to invent a new theory and the reception of that theory are driven by imagination. The Ptolemaic system had been amended to fit the observations; but that was not good enough for Copernicus, because the accumulated complications which had been introduced by these amendments had left the motions of the planets in apparent confusion. He therefore felt it necessary to invent a new system in which ‘these, the noblest works of nature, might no longer appear devoid of that harmony and proportion which discover themselves in her meanest productions’ (Smith [1795] 1980, p. 71). Copernicus’s new arrangement, which, as Smith observes, drew on suggestions from earlier philosophers (this is a general feature of new theories, because of the nature of human cognition), restored harmony and proportion, but at the cost of defying the immemorial sensory knowledge of a stationary earth; and Smith comments that nothing ‘can more evidently demonstrate, how easily the learned give up the evidence of their senses in order to preserve the coherence of the ideas of their imagination, than the readiness with which this, the most violent paradox in all philosophy, was adopted by many ingenious philosophers’ (Smith [1795] 1980, p. 77).

Smith never discusses the truth of astronomical theories, though clearly the succession of theories which he presents implies that every theory before Newton had been eventually judged insufficient – because it no longer satisfied the imagination. In
acknowledging how effective Newton was in producing ‘a system, whose parts are all more strictly connected together, than those of any other philosophical hypothesis’, he observes that he has been ‘insensibly drawn in, to make use of language expressing the connecting principles of this one, as if they were the real chains which Nature makes use of to bind together her several operations’ (Smith [1795] 1980, pp. 104, 105). This, he clearly implies, is an illusion: Newton’s system is the product of Newton’s imagination, and its appeal is to the imagination of its readers or hearers. (It failed to satisfy Leibniz; and physicists are still puzzled by the phenomenon of gravity.) Nevertheless it is an illusion that allows people not only to stop worrying but to make effective practical use of Newton’s ‘connecting principles’.

This theme is, appropriately, taken up in Smith’s Lectures on Rhetoric when he discusses the appropriate forms of communication for different purposes. (Note that the effectiveness of any particular form is domain-limited.) Smith argues that a body of what we would now call scientific knowledge may be presented in two ways: we may either give specific explanations for distinctive elements, ‘or in the manner of Sir Isaac Newton we may lay down certain principles known or approved in the beginning, from whence we account for the several phenomena, connecting all together by the same chain. The latter, which we may call the Newtonian method is undoubtedly the most philosophical, and in every science whether of morals or natural philosophy etc. is vastly more ingenious and for that reason more engaging than the other’ (Smith 1983, pp. 145-6). Smith immediately observes that it was Descartes who first used this method, and demonstrated its effectiveness by gaining wide acceptance by ‘the learned in Europe’ for a system which ‘does not perhaps contain a word of truth’. A powerful appeal to the imagination is extremely persuasive, but it is not a sufficient criterion of truth. This does not, we may note, lead Smith to qualify in any way his recommendation of the method.

That the appeal of an apparently well-ordered system may be based on illusion is a theme that recurs in Smith’s writing. These illusions may turn into delusions, notably among those who believe that they have the knowledge and means to direct economic systems; this is a major theme of the Wealth of Nations. They may also ruin individual lives, as in his account of the ‘poor man’s son, whom heaven in its anger has cursed with ambition’ and who not only ‘admires the condition of the rich’ but devotes his life to achieving such a condition, only to find that he has foolishly sacrificed many genuine pleasures ‘for what, when he has got it, can afford him no real satisfaction’ (Smith [1759] 1976a, pp. 181, 182). (Henry V, in Shakespeare’s play (Act IV, Scene 1), observes that the ‘thrice-gorgeous ceremony’ which surrounds a king is poor recompense for the continual burdens of kingship: this may, of course, be Shakespeare’s view rather than Henry’s.) But Smith immediately proceeds to argue that this human tendency to confound the appearance of order with the real advantages which we desire is ‘the deception which rouses and keeps in continual motion the industry of mankind’ (Smith [1759] 1976a, p. 183). It is worth remarking that Smith’s account of the motivation for economic development is not very different from Schumpeter’s – except that Schumpeter does not consider whether the driving passions of his entrepreneurs are also founded on a delusion about the pleasure which might result from success; nor does Schumpeter explicitly recognise how many entrepreneurial schemes are themselves delusions. Economists are reluctant to recognise the prevalence of failure in all human activities – which, of course, sits awkwardly with the concept of rational economic agents – and therefore to investigate
its significance for the organisation of economic systems and for economic policy; this is true even in evolutionary economics.

It is clear that Smith’s theory of economic development is not founded on rational expectations and cannot deliver Pareto improvements. It can, however, deliver genuine improvements through a beneficial illusion, because ‘the same love of system, the same regard to the beauty of order, of art and contrivance, frequently serves to recommend those institutions which tend to promote the public welfare’. Thus ‘we sometimes seem … to be eager to promote the happiness of our fellow-creatures, rather than to view to perfect and improve a certain beautiful and orderly system, than from any immediate sense or feeling of what they either suffer or enjoy’ (Smith [1759] 1976a, p. 185). Of course not every beautiful and orderly system is very effective in improving happiness, as the record continues to show.

In *The Wealth of Nations*, Smith followed his own prescription from his *Lectures on Rhetoric* by stating at the outset his fundamental proposition that the division of labour, by allowing each person to focus on a limited range of phenomena, is the central organisational principle of the growth of knowledge. We have already observed that this proposition originally appeared in the development of his account of the emergence of science as a distinct field of activity and its progressive differentiation into fields of study, each of which, as he notes, develops its own criteria for satisfying the imagination of those who are working in each field – criteria which are quite often rejected by outsiders. Transferring this idea from science to the economy (Smith [1776] 1976b) is a notable example of a classic move in developing new knowledge, which always requires a connection from some element already in the brain of its originator: Copernicus’s replacement of the earth by the sun as the focus of spheres which rotate about it is an outstanding example.

It is accompanied by two additional transfers, from the *Lectures on Rhetoric* and *The Theory of Moral Sentiments*, to supply a necessary complement to the principle of differentiation. For each individual, the opportunity cost of specialisation is the consequent dependence on other people for essential means of well-being, or even survival. Smith’s principal proposal for relieving this dependence is the development of a near-universal system of exchange; but he realises that exchange is not a simple logical consequence of specialisation, as it later became in standard price theory. He points out that extensive exchange is specifically human, and requires a specifically human explanation. The immediate explanation he finds in the human delight in persuasion, which he associates closely with the development of language (Smith [1776] 1976b, p. 25) and the uses of language which he had discussed in his *Lectures on Rhetoric*, to create appealing patterns. As we know, in human societies not everyone bothers with exchange; force is a familiar alternative, which dispenses with the need for persuasion, or anything beyond the most basic language. Though it is not unreasonable to begin an analysis by postulating the fact of exchange, it is unreasonable to analyse exchange by assuming economic agents with characteristics which would have frustrated the development of any extensive system of exchange.

Smith was well aware that the development of language is necessarily a social phenomenon; it both presupposes and encourages social interaction and the recognition of others as people with distinctive identities, and this recognition of distinctive and relevant identity is proclaimed in the opening paragraph of the *Theory
of Moral Sentiments. (It seems significant that the differences between primate species in the relative size of the brain is correlated with the extent of social activity among members of that species.) Not the least important consequence of this relationship to others is the human propensity to observe and imitate apparently successful behaviour, thus greatly facilitating the diffusion of knowledge and skills – the failure to explain which is a major lacuna in much economic theory; and since this is much easier for those who are participant observers in society, defection is not obviously a dominant strategy.

The attractions of an apparently integrated theoretical system are central to George Shackle’s account of the development of economic theory between the wars, in which Shackle reinvented Smith’s theory of science. (He was delighted when Andrew Skinner pointed this out to him.) ‘A sense of order and consistency is needed … to make practical life possible’ (Shackle 1967, p. 286), and so we need patterns. But beyond that is a psychic need, which is to set our minds at rest. ‘So long as we have a satisfying conceptual structure, a model or a taxonomy which provides for the filing of all facts in a scheme of order, we are absolved from the tiresome labour of thought, and the uneasy consciousness of mystery and a threatening unknown’ (Shackle 1967, p. 288). Therefore theories ‘are things to be held and cherished’ (Shackle 1967, p. 289); and, as Smith argued, if they can no longer be made to serve as protection from ‘a threatening unknown’ they must, if at all possible, be replaced by forms of order which will serve the imagination. (As has often been demonstrated, familiar systems of thought may also be defended by the denial of evidence or by assailing the motives of critics.) Though these new forms, as Smith and Shackle alike insisted, are imaginative creations, they may appear to be accurate descriptions of real phenomena and real relationships, or at least of apparently significant aspects of them. Sometimes the detailed fit may not matter, as Friedman argued in a once-famous paper; unfortunately whether or not it does matter is only discovered if the inaccuracy has serious undesired consequences.

Since this is essentially a psychological argument, it is appropriate to refer to George Kelly’s (1963) proposition that we all develop (or adapt from others, as noted above) ‘interpretative frameworks’ on which we rely to make sense of our surroundings: what is called ‘experience’ is therefore a personal construction of events, with a ‘range of convenience’ the limits of which are discovered only when they are inadvertently transgressed, as with scientific theories: indeed Kelly’s fundamental decision is to treat humans as scientists, in defiance of contemporary psychological practice. According to Kelly, it is precisely this process of individual development which makes each of us a person – a proposition which is consistent with both Marshall’s (1994) and Hayek’s (1952) theories of the mind. This allows for substantial differences between people (and between sciences) in the frameworks on which they rely, and also for crises when events resist interpretation in ways to which people have become psychologically committed.

As a clinical psychologist, Kelly was particularly interested in cases where people are unable either to make sense in familiar ways or to accept any alternative; but similar effects may be observed in groups within which particular patterns of interpretation have become essential to support the premises and procedures which ensure cohesion. Such institutional crises may afflict organisations, or academic disciplines. Although Kelly was sharply critical of the contemporary practice of psychologists who denied
their subjects the status of man-as scientist, attempting to predict and control, he did not extend his analysis of personality crisis to his own profession – or to any other.

The underlying problem is the pervasiveness of uncertainty, in Frank Knight’s (1921) sense of the absence of demonstrably correct procedures for assigning probabilities, for enumerating a full set of possibilities, or indeed of determining the truth of any broad proposition. Why uncertainty is pervasive has already been outlined. Now as Shackle (1969, p. 224) pointed out, ‘[t]he boundedness of uncertainty is essential to the possibility of decision’. But any boundaries on which we rely are themselves necessarily uncertain; they are a pretence of knowledge, but a necessary pretence. However, to fully admit this pretence would paralyse action – as it sometimes does, for example in the recent unwillingness of banks even to consider making loans to other banks. So in order to act we must conceal our pretence, and conceal our concealment – a practice which has been examined for many years by Chris Argyris (e.g. Argyris 2004), who has emphasised its pervasiveness in formal organisations, as has been recently demonstrated. (It is, naturally, not unknown in economics.) The simplest way of bounding uncertainty is to rely on the comfort, and apparent sufficiency, of wide-ranging theories, as Shackle argued; and one might conjecture that a genetic propensity to do so (which is a propensity to seek illusion) would have some evolutionary advantage for a population, although it could be disastrous for many individuals within that population. This conjecture does not require group selection (though that is now increasingly accepted as a valid concept): the most successful individuals would be those who relied on wide-ranging theories that happened to work well.

This conjecture may be supported by Knight’s analysis of the implications of uncertainty. If indeed there were demonstrably correct ways of listing possibilities and assigning probabilities to them, then calculation would always be possible for everyone; therefore there could be no entrepreneurial ventures, no advantage on which any individual could base a profit (though rents could be derived from scarcity), no contracts of employment, and no money. Indeed, Knight suggests, humans would function as automata, and some of our distinctive characteristics would be redundant. In particular, there would be no role for intelligence, which Knight defines as the ability to sort phenomena into categories by the creation of patterns which seem appropriate to a particular problem or a particular purpose, usually without much attention to the limitations of their domain of application. It is therefore appropriate to define phenomena as unintelligible, as I did earlier, if they cannot be assigned to a category, either pre-existing or newly-imagined. This brings us back to Smith, and to Shackle.

It also brings us to a fundamental question about the grand project of science. If the ultimate aim is to explain everything, that implies a fully determinate universe; but then the often-stated ambition to predict and control is incoherent, because if everything is predictable it is already too late to control it. Thus, in the words of Alexander Koyré quoted by Prigogine (2005, p. 63) ‘though there is a place for everything, there is no place for man’; and that clearly implies no place for man within an economic system – a point to which we will return shortly. Prigogine argues that a place for man has been restored by thermodynamics, which entails irreversibility and self-organisation, the outcomes of which are not predictable. He goes on to argue that this implies change through evolutionary processes of variation
and selection, which I will not discuss in this paper, though I will signal the possibility of both illusion and delusion within any kind of evolutionary theorising. Thus there is room for human imagination to have real effects, and therefore for pattern-making capabilities and the motivation to deploy them to confer advantages on particular individuals and particular groups, while it leads other individuals and groups astray. As Hayek (and Marshall) argued, competition is a discovery process; and in this process individual failure is the norm, even for those who achieve success.

All theories are patterns, in which certain concepts are selected, and arranged in particular ways; and in the process many other concepts are not selected and many other arrangements omitted. Normally a few of these omissions are deliberate and a great many are never considered, because every organisation and every science develops a set of premises and procedures (to use Herbert Simon’s phrase) which economise on cognition and promote coherence within that organisation or science. The philosopher-mathematician Whitehead ([1911] 1948, pp. 41-2) argued that only by no longer thinking about most of what we are doing is it possible to create novelty. However, there are opportunity costs. ‘When the compass of potential knowledge as a whole has been split up into superficially convenient sectors, there will be no knowing whether each sector has a natural self-sufficiency. … Whatever theory is then devised will exist by sufferance of the things which it has excluded’ (Shackle 1972, pp. 353-4) – and we no longer think about these exclusions (if we ever did) in order to make progress.

I learned long ago that the reason why research and development is predominantly development is that for a project to be successful it is necessary to investigate many of the things that were – necessarily – excluded from the theory or the experiment on which the project was founded; and I learnt it from people who were very well aware that this is what they were doing, and sought for guidance about exclusions that might prove decisive, especially the exclusions that were inherent in their academic or professional training. (That the predictability of technology is achieved by painstaking construction of specific conditions in which the desired patterns can be achieved has been argued by Nightingale 2004). They also knew that, although there are many useful procedures which can aid their investigations, there are none that are demonstrably correct. Imagination is essential, but much that is imagined is illusory. A major task of science, and of the management of innovation, is to seek out and remove illusions.

It should also be a major task in policy studies. However, although economists are often concerned, at least nominally, with the application of their theories to policy, they usually seem to be much less inclined to undertake a rigorous search for what has been excluded because it lies outside their subject – or rather outside their trained competence. Because they thereby avoid exposure to many challenges they are naturally more susceptible to the aesthetic appeal of their theories, which often seems to depend on what is believed to be their scientific character, and inclined to imagine extensions and applications which preserve, and preferably reinforce, their internal coherence. This aesthetic appeal seems to frame the predominant conception of the subject. As Arrow (1974, p. 16) observed, economists tend to regard themselves as the guardians of rationality; and surely nothing can be more fundamental than a universal principle of rationality. We have already noted Hume’s answer to that; Knight’s proposition that a world of perfect rationality would be populated by
automata is even more disconcerting. I propose to consider two examples of theoretical systems which have been used as the basis for recommendations which are particularly susceptible to question once one begins to consider some of the things which have been excluded.

Choice theory and equilibrium

In my judgement there is nothing in economics to match the appeal to the imagination of the general equilibrium system as extended by Arrow and Debreu. (Nash equilibrium, though it has a pleasing symmetry, does not come close.) It links the individual directly to the universal, being apparently based on the independent fully-reasoned choices of every individual and delivering a complete and conclusive account of a fully-specified economic system. Everything, it seems, is accounted for in a Copernican model of harmony and proportion. (All game theory imposes an isolation from the environment which is obviously artificial – as, of course, is the intention in organised games.) Moreover it yields a pair of universal theorems for policy: every perfectly competitive economy is a Pareto optimum, and every Pareto optimum may be delivered by a competitive economy. In a collection of articles Frank Hahn (1984), one of the most intelligent general equilibrium theorists, vigorously asserted the value of this theory against its critics as an effective way of organising thought and defining issues, including the limitations which are imposed by what has been excluded; but he was also a determined, and sometimes fierce, critic of those who ignore these limitations, with a particular emphasis on macroeconomics.

Hahn’s (1984, pp. 308-9) summary of these limitations, although not quite comprehensive, is admirably crisp. The general equilibrium model is ‘only half a theory anyway, since there was (and is) no rigorous account, derived from first principles, of how the Arrow-Debreu equilibrium comes to be established’. (There is a logical obstacle to constructing such an account: outside equilibrium there must be some agent who is not optimising, but optimisation is a necessary assumption in proving the existence of equilibrium.) The half-theory that exists ‘could not account for money or the stock exchange; there were … no increasing returns possible; there was no theory of actual exchange; the number of firms was taken as exogenous, and one would require set-up costs to make sense of firms anyway; information was symmetric and complete; labour was sold like peanuts are sold; unborn generations implausibly made themselves felt on current markets, and there were far too many markets anyway’. To this he added a little later: ‘We know next to nothing about expectations, and that is why we take the step of demanding that they be rational’ (Hahn 1984, p. 313). Thus the absence of knowledge becomes the justification for assuming the completeness of knowledge.

Strictly speaking, the existence of a Walrasian competitive equilibrium as a concept is consistent with any kind of expectations, however erroneous, as long as every agent is prepared to commit to a full set of contracts on the basis of the expectations that each of them happens to have. However one can then make no claims either for the durability of this equilibrium or for its optimality, which was an essential objective of the project to supply microfoundations for macroeconomics that Hahn was primarily criticising. However the desire to make clear policy recommendations is a dubious justification for insisting that every agent has the correct model of the economy; it seems highly unlikely than anyone ever has, or ever will have. Moreover this
assumption is a formidable obstacle to understanding how economic systems work. ‘The circumstance that we must always make public choices does not seem to me to lead to the conclusion that we must always cook the books’ (Hahn 1984, p. 311). On the contrary, Hahn asserted, as was argued earlier in this paper, that before making public choices it is highly desirable to give some serious thought to the possible effects of what has been left out.

However, a powerful deterrent to doing so – which I suspect can influence us all – is the fear of losing an integrated system which both serves to protect us from the unknown (and therefore from the disorders which Kelly set out to explain) and which has its own aesthetic appeal. The two motives may be mutually reinforcing: as Einstein, and others, have observed, if a theory looks right, it seems natural to believe that it is right. No theoretical system in economics has looked so right as the enhanced Walrasian system in supplying an internally-consistent explanation of an economic system as the well-ordered product of human action. Everyone is rational, they all make their own decisions independently, and the result is perfect harmony. Moreover, the concept of Pareto optimality has the happy effect of allowing us to admire this harmony while still conceiving schemes for redistribution – provided that one avoids recognition of the fact that any actual scheme of redistribution is unlikely to compensate all losers and therefore cannot meet the test of Pareto improvement.

As an obvious contrast, Marshall’s theoretical system looked very untidy; there was no neat explanation of how the various partial equilibria might fit together, and these equilibria themselves were not at all well defined. One particular difficulty was the absence of a long-run equilibrium for the firm; but when Pigou supplied this it appeared to lay bare Marshall’s failure to understand the conditions of perfect competition. This failure – extremely embarrassing for a Second Wrangler in the Cambridge Mathematics Tripos – might be thought to explain his mysterious suggestion that organisation, of various kinds, should be considered as a factor of production, rather than as a source of monopoly; only such a failure could explain why he not only stated that ‘the part which man plays [in production] shows a tendency to increasing return’ (Marshall 1920, p. 318) but included increasing return in his analysis of the ‘normal’ long run.

In the course of exposing Marshall’s confusion, Samuelson ([1967] 1972, p. 39) delivered a magisterial rebuke. ‘Increasing returns is the enemy of perfect competition. And therefore it is the enemy of the optimality conditions that perfect competition can ensure’. However, this is a proposition which is comprehensible only within the Walrasian system – and not necessarily within Walras’s own system of thought, which was intended to include economic progress; if it is assumed to be directly relevant to policy, then we have progressed from illusion to delusion, because Samuelson has the argument the wrong way round: the optimality conditions derived from the theory are the enemy of improvement. The counter-argument had already been forcefully made by Schumpeter (1943, p. 83): ‘A system … that at every given point of time fully utilises its possibilities to the best advantage may yet in the long run be inferior to a system that does so an no given point of time, because the latter’s failure to do so may be a condition for the level or speed of long-run performance’. Richardson (1975, p. 353) simply dismissed the theoretical focus on perfect competition as ‘a denial of Smith’s central principle erected into a system of political economy’, because that central principle is a theory of continuous development.
through the generation of knowledge as a consequence of the expanding division of
labour – a theme developed by Marshall and Allyn Young (1928).

In fact, the optimality conditions which perfect competition can ensure exist only
within the Walrasian equilibrium. As Hahn pointed out, there is no Walrasian account
of how this equilibrium is to be attained by a working production economy. Nor can
there be. Walras realised that unless every producer chooses the equilibrium quantities
from the start, some at least of the resulting trades will not be at equilibrium prices,
and so there will be a redistribution of resources which will invalidate the original
calculations of equilibrium. At this point Walras abandoned his ambitious project to
explain first adjustment, and then growth. The ‘competition’ within a model of
perfectly competitive equilibrium, which guarantees optimality, lacks every essential
feature of the competition which is necessary even to achieve equilibrium, still less to
produce change. Both require a process of discovery – which of course uses resources
which are then not available for other purposes. That in turn requires that ‘the
participants hold uncertain and divergent beliefs about the possibilities of success’
(Richardson 1975, p. 359); many, perhaps most, of these beliefs will be wrong, and
consume resources wastefully, as judged by a Walrasian model. Since this discovery
process may be expected to reduce the welfare of most competitors, it must depend on
‘non-rational’ motivations. (It might even generate involuntary unemployment, which
has been judged inconsistent with rationality – which is strictly correct, as rationality
has been defined.) We may also add that this process of discovery requires
institutions, and is often incompatible with anonymity: as Hayek pointed out, we need
to learn who will serve us well, and as Marshall and Simon both observed, we need to
learn with whom we can co-operate effectively for what purposes.

It is, of course, not impossible that a theoretical structure which is inherently
incapable of directly explaining how a system works may nevertheless have
substantial uses; but these uses depend strictly on the sufferance of what has been
excluded, and that should not simply be assumed, as has so often been done. But at
the very heart of this theoretical system lies a fundamental paradox. Because what the
theory produces is an equilibrium allocation of all inputs and outputs, in which all
possibilities have been taken into account, the contracts into which every agent enters
are strictly derivable from the initial conditions of the model. Within this equilibrium
there is no scope whatever for any choice by any agent. It is therefore difficult to
accept ‘choice’, in its normal sense, as a valid description of what agents are supposed
to be doing; indeed the theory illustrates very effectively Knight’s proposition that in
the absence of uncertainty we simply have automata. The complete set of contracts
might therefore just as well be specified by a single individual; then we would have
perfect central planning – which would also be performed by automata. There is no
place for man – not even for economists.

It is no accident that the outcomes of perfect competition and central planning are
isomorphic, nor that this apparently comprehensive theoretical system should have
been invoked to support opposing policy recommendations. The apparent obstacles to
perfect competition in actual ‘market economies’ were thought, by many non-Marxist
economists through the middle decades of the twentieth century, to be sufficient to
demonstrate the inherent superiority of planning. The subsequent development of the
contrary view that ‘market economies’ actually deliver outcomes which are very close
to those of the competitive equilibrium models reversed the presumption of inherent
superiority; furthermore, it fostered the belief that such ‘market economies’ were the natural product of independent action by rational agents. ‘In the beginning there were markets’; and the complete destruction of a planning regime was sufficient to create a new beginning. Both delusions resulted from an integrated vision in which unbounded rationality (and unbounded cognition) pre-empts any serious exploration of process, and of the institutions which are necessary for any process in human society. Hahn and Solow (1995, p. 2) have commented on the special case of macroeconomics, which ‘began as the study of large scale economic pathologies … Now … macroeconomic theory has as its central conception a model in which such pathologies are, strictly speaking, unmentionable’. They are worried by what they regard as a serious pathology of economics. We should not be surprised that such pathologies exist in economics, as they do in economic systems; they are an opportunity cost of human imagination. We might, however, be more conscious of the dangers which they bring, and make greater efforts to recognise them. The record is not good.

Finance

A potentially serious deficiency in rational-choice based equilibrium theory was pointed out by Martin Hellwig at the 1992 Conference of the European Economic Society, of which he was then President. ‘Once we accept the fundamentally non-Walrasian nature of the organization of exchanges in a monetary economy, we are led to question … the foundations of a good part of microeconomic theory and welfare economics’ (Hellwig 1993, p. 232). When we recognise, as Hicks (1982, p. 7) did in 1933, following Knight, that ‘[o]ne must introduce uncertainty, before one can introduce money’ the questions threaten to become more insistent. However, in 1997 Robert Merton and Myron Scholes were awarded the Nobel Prize in Economics for demonstrating that such questions were illegitimate. ‘The special sphere of finance within economics is the study of the allocation and deployment of resources, both spatially and across time, in an uncertain environment’ (Merton 1998, p. 323); therefore economists working in other branches of the subject can simply adopt the results of finance theory, because ‘the underlying conceptual framework originally used to derive the option-pricing formula can be used to evaluate and price the risk in a wide array of applications, both financial and non-financial’ (Merton 1998, p. 324). Correct pricing may be logically derived from correct evaluation; but that requires a demonstrably correct method of identifying all relevant possible events and assigning the correct probabilities to them. There can be no surprises, even in the form of new knowledge. This is the first time that any Nobel Prize has been awarded for alchemy: the successful transformation of uncertainty into risk, as demonstrated in the two quotations.

In his foreword to a re-issue of Risk, Uncertainty and Profit, Knight (1933, p. xiv) was ‘puzzled by the insistence of many writers on treating the uncertainty of result in choice as if it were a gamble on a known mathematical chance.’ Shackle (1972, p. 425) explains the puzzle. ‘A training in value theory can have a strange effect on the individual mind. It can give rise to a fundamental scepticism concerning the reality of “events”. The mind which has been taught to suppose that the norm of conduct is rational conduct … draws the tacit inference that … the fully successful individual would encounter nothing in life that he was not prepared for.’ That is perhaps the ultimate appeal to the imagination, and the ultimate comfort for decision-making. It is
also the ultimate illusion, for reasons which Smith would have well understood, and
the most dangerous delusion. That option theory could ensure that one would
encounter nothing that one was not fully prepared for was the theme of the Nobel
Prize Lectures published in 1998.

The final economics examinations in Cambridge used to include a three-hour essay on
a choice of subjects. Among the topics one year was the theme ‘Economics: the
science of the confusion of stocks and flows’. One might now propose ‘Finance: the
science of the confusion of risk and uncertainty’. Merton and Scholes confused
themselves, and were the first to be deluded by their own imagination. By the time
that their Prize Lectures were published (Merton 1968, Scholes 1968) they were
deply involved in guiding the decisions in what was being hailed as a spectacularly
successful financial operation called Long Term Capital Management. (As someone
later remarked, never trust a business whose title is a lie.) However, they had made a
simple logical error – so simple that it was immediately obvious to me when I
compared the lectures with the newspaper reports. In the former they claimed to have
produced the definitive system for pricing options; in the second it was asserted that
this system, which was public knowledge, could be the basis of a consistently
outperforming business. Clearly these two claims (or any equivalent combination) are
logically incompatible, as Knight had demonstrated 77 years earlier. However Merton
and Scholes were unable to make this simple deduction, which for me was an
immediate implication of Knight’s analysis – and a justification for the warning
implicit in his 1933 preface; nor were many other very clever people. (One should
always remember that very clever people are capable of being very silly – and
sometimes dangerously so.) In Shackle’s (1972, p. 354) phrase, their model of option-
pricing existed only by sufferance of the things which it had excluded: these were
many, and it happened to be the Russian default on overseas debt which produced an
unarguable refutation of Merton and Scholes’ business plan. The final score,
therefore, was Knight 2, Merton and Scholes 0.

Within a very few years, another financial model was tested to destruction. It is a
familiar proposition that the correct valuation of any business is provided by the
present value of its future cash flow; and this principle seemed easily applicable to
new internet-based retailing, the unique feature of which was that its operations
automatically generated not only a complete list of customers but a detailed analysis
of every customer’s purchases. This inherent feature provided an obvious marketing
advantage over traditional retailers, readily translated into additional value; it also
provided an obvious advantage to financial analysts, because the present value of any
such business could in principle be calculated by summing the future cash flows
attributable to each customer. As a short-cut approximation, every increase in the
number of customers could be assumed to deliver a proportionate increase in value,
even if the immediate cost of this increase was ever-growing accounting losses.
Indeed the more rapidly these losses were accumulating the faster the value of the
business grew; and this growth was appropriately reflected in the rate of increase in
the share price. Since it was demonstrably correct that these businesses constituted a
new economy which would displace very many outmoded companies, the absence of
uncertainty gave an assurance of profits. That there had been a series of ‘new
economies’ in the previous two centuries which had created past delusions was either
forgotten or irrelevant. But Knight was proved right again.
The elaborate financial instruments which translated loans to people who could not afford them into secure income for banks have provided the most recent example of the appeal of schemes that transmute uncertainty into risk; but I would like to focus on the particular case of Northern Rock. Though the apparent urgency of its management’s desire for expansion resulted in some loans which were well below the standards once generally applied – itself an indication of a confusion between risk and uncertainty – nevertheless what is of particular interest is that it was not this side of its balance sheet that caused the trouble. For rather obvious reasons, people are usually willing to lend at lower rates for short periods than for long; therefore shifting the business’s liabilities decisively into short-term borrowing seemed likely to be an excellent business plan, especially as a means of accelerating growth. Of course there was always a risk of facing periods of sharply higher interest rates, but on average the rates should be lower. What the directors had not envisaged was the simple refusal to lend at any rate. This is not a possibility that is readily envisaged in either finance or price theory, because it is incompatible with the concept of an equilibrium of optimising agents, in which there can be no unexploited gains from trade. Trade may not take place because the seller’s minimum price is above the buyer’s maximum, but both seller and buyer have a price at which they are willing to trade, computed according to the risks involved in the transaction. Even the recognition that assets may fetch much less in a forced sale does not extend to the possibility that no-one will even contemplate buying.

If all uncertainty can be transmuted into risk, then there must always be an equivalent gamble. That was an undiscussable principle in the teaching of decision theory at Harvard Business School in 1966 – and consequently always discussed in my teaching of decision theory in Stirling, a policy which reflected its summary rejection by practising managers. (See Loasby 1995, pp. 481-2.) This is also the line of reasoning that finds the concept of involuntary unemployment simply meaningless. However, although the proposition that if people do not know what to do they may decide to do nothing is incompatible with rational choice theory, it actually seems a reasonable description of human behaviour. It is, of course, what Keynes (1936) argued in chapter 12 of the General Theory, and in his reply to his critics (Keynes 1937). What is less often noted is that it is also Marshall’s (1920, pp. 710-11) summary explanation of depression in the Principles – an explanation which, as Marshall notes, is carried over from The Economics of Industry, written with Mary Marshall and published in 1879.

The inability to act because there seems to be no reliable basis for decision is the single major feature shared by Keynes’s and Schumpeter’s theories of depression; but they differ sharply about the cause. For Keynes any investment which depends on the prospects for some years ahead always rests on the pretence of knowledge, because these prospects are simply unknowable – a perspective that became much more plausible after 1914, as Shackle (1967) observed; and in his 1937 article he described the illusions which he believed were the most powerful supports at any time: that the present is a better guide to the future than the past record would suggest, that current prices accurately reflect future prospects, and that the average or majority judgement is correct (Keynes 1937, p. 214). However, as Keynes observed, these illusions may be exposed as delusions at any time; and Government action may be the only available remedy. Of course the revised prospects prompted by the exposure of any particular delusion may be equally illusory.
This is not the time to discuss the relative merits of Schumpeter and Keynes; but it is worth drawing attention to the comparison with Kelly’s theory of personal breakdown, which he explains by the inability to make the (possibly drastic) revisions of long-serviceable construction systems in the face of unimagined novelty, and to the intermediate situation of organisations which disintegrate when faced with similar challenges, or simply dwindle into extinction as the domain within which their particular theories and policies are still relevant progressively diminishes.

Conclusion

It may be appropriate to end by reflecting on Schumpeter’s (1934, p. 80) view of the practical relevance of Walras’s creation.

The assumption that conduct is prompt and rational is in all cases a fiction. But it proves to be sufficiently near to reality, if things have time to hammer logic into men. Where this has happened … one may rest content with this fiction and build theories upon it. … Outside of these limits our fiction loses its closeness to reality.

Schumpeter believed that a completely different theory was necessary to explain economic development. The danger, of course, is that if the ‘fiction’ is not recognised as fiction, it may be thought applicable to all economic activity; and it is evident from Schumpeter’s admiration of Walras that he appreciated the powerful appeal of this (apparently) supreme fiction. However, it is possible to go beyond this criticism to argue that although the assumption of well-informed rationality yields good predictions in a stable environment, it is an obstacle to understanding what is actually maintaining that stability, which is the observance of a complex interdependent set of behaviours which is largely embedded in institutions and organisations – and ultimately in human minds. A theory which is founded on the characteristics of human cognition and motivation, and the institutions and organisations which these characteristics support, is better able to explain both the creation of novelty and the disruption that novelty may cause: the response to development is unlikely to be prompt and rational, but to include both over-exuberance and disruption (both incorporated in old-fashioned theories of business cycles). Schumpeter is much more confident than Keynes about the ability of an economy, given time, to find an interlocking set of routines which is observationally equivalent to a Walrasian equilibrium and to sustain that equilibrium in the absence of any further disturbance – though he recognises that this may take years; but perhaps this confidence owes more to his desire to partition the field of economics between Walras and himself – a concept which clearly appealed to his imagination – than any particular ideas about the processes of adjustment, which (like the details of entrepreneurial action) he is reluctant to discuss.

For an understanding of processes Marshall is a much better guide, very possibly as Raffaelli (2003) has argued because of his early interest in the construction of knowledge and capabilities within the human brain. What needs to be constructed is a new set of frameworks for thought and action (which we may call, following Hahn, a set of theories and policies) that will allow the development of a new coherent pattern of routines, which allows a logic of appropriateness (a sophisticated form of stimulus
and response) to simulate rational choice for much of the time, but much more quickly and at much lower cognitive cost than the performance of elaborate formal computations. Whitehead’s principle matches rather well Marshall’s sequential process of consolidating novelty into automatisms, thereby creating space for new novelties. That this may give the appearance of rationality may often be a beneficial illusion, giving us the confidence to act in circumstances where uncertainty is often much greater than we recognise.

However, scientists should not be content with appearances, and especially not with a notion of rationality which does not correspond with the process of scientific discovery. Kelly’s proposal to consider humans as scientists suggests something very different from the practice of modelling humans as rational agents; it suggests alternative ways of investigating familiar questions, and opens up questions which could otherwise hardly be investigated at all. Elaborating notions of equilibrium which are based on rationality and excluding uncertainty (a self-liquidating combination in Knight’s view) is unlikely to greatly improve our understanding of how the economy works. As evidence for this we may consider the exiguous treatment of firms, markets, and the organisational structure of economies in so much of economics, because all of these rely on organisational and institutional arrangements which serve to support the particular abilities of the human mind, and mitigate its disabilities. Moreover, we may recognise, as Marshall did and Schumpeter did not, the essential interplay between regularities and the creation of novelty – which has been, I believe, an emergent focus of DRUID.

References


