

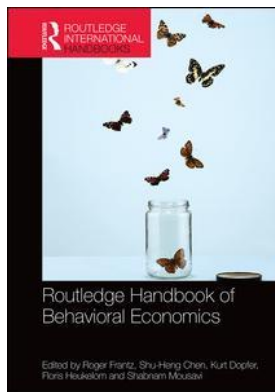
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PART I

Scientists in the field of behavioral economics

Introduction

The first and last articles in Part one are of a broader nature about the history of behavioral economics: Peter Earl, “The evolution of behavioural economics,” and Morris Altman, “A bounded rationality assessment of the new behavioral economics.” Earl’s chapter, which opens the book, goes into more detail about the history/evolution of behavioral economics. Alfred Marshall is older than “old” behavioral economics, but the others Earl discusses are old behavioral economics: Simon, Leibenstein, Baumol, Winter, Katona, and Shackle. The “new” behavioral economics that he discusses is Kahneman and Tversky, and Thaler and Sunstein. The *New York Times* in 2001 may have implicitly declared the “old behavioral economics to be irrelevant,” but Earl obviously disagrees. Altman’s chapter is much more focused on the relative strengths and weaknesses of two approaches to behavioral economics. The old behavioral economics of Simon or the new behavioral economics of Kahneman and Tversky. Altman also discusses Vernon Smith, Akerlof, and others. Altman appreciates both the old and the new behavioral economics but when push comes to shove he prefers Simon’s approach.

There are two chapters on George Katona. Richard Curtin was a student and friend of Katona where both worked at the University of Michigan. Richard Curtin’s chapter, Chapter 2, “George Katona: a founder of behavioral economics,” reviews Katona’s career in behavioral economics beginning in the 1940s, but does more than that. He shows how Katona’s idea of frames of reference preceded Kahneman and Tversky’s idea of framing by 25 years, and how his ideas of intervening variables preceded Akerlof’s ideas on intervening variables by 50 years. These facts should not go unnoticed by anyone interested in the “real” history of behavioral economics. Hamid Hosseini’s chapter, “George Katona’s contributions to the start of behavioral economics,” Chapter 10, shows that Katona was writing about psychology and business decisions as early as 1945, and that in 1977 the American Psychological Association acknowledged Katona with developing a new field of research bridging the gap between psychology and economics. Hosseini also shows the various ways in which Katona’s research methodology and underlying assumptions were different in several respects from the then standard neoclassical theory. Finally, Hosseini discusses why economists affiliated with the Cowles Commission did not appreciate Katona’s work, while others such as James Tobin acknowledged the debt owed to Katona by the economics profession.

Ken Boulding is the subject of Stefan Kesting's chapter, Chapter 3, titled "Ken Boulding: the image as a precursor to framing?" Similar to Curtin putting Katona's work in historical perspective with younger behavioral economists, Kesting shows how both Boulding's image, and Kahneman and Tversky's concept of framing rely on mental accounting, with Boulding preceding the latter by about 25 years. Chapter 4 focuses on Harvey Leibenstein, who similar to Boulding began writing about behavioral economic themes in the 1950s. Leibenstein's work in the area of behavioral economics began with his 1950 article on the social context of individual decision making, specifically how others' behavior served as intervening variables between prices and quantities demanded. Akerlof discussed this in 2007, while Vernon Smith discussed it in his book, *Rationality and Economics* in 2009, both writing almost 60 years after Leibenstein's 1950 *QJE* article. Of course, the focus of the chapter is Leibenstein's X-efficiency theory, a theory which attempted to ask what are the implications for economic theory when we drop the assumptions of perfect rationality, maximizing behavior, and efficiency as meaning only allocative efficiency. Leibenstein dropped all three and opened the "black box" which is the neoclassical/non-behavioral economics firm. These three are also the foundations of the then neoclassical theory, and which helped open the conversation for a later advancement, not a beginning, of behavioral economics.

There are four papers discussing the work of Herbert Simon. One is Altman's paper which we have already mentioned. The paper by Grebel and Stützer, "Schumpeter, Kirzner, Knight, Simon, and others: behavioral economics and entrepreneurship," Chapter 13, mentions Simon's theory of bounded rationality as the reason why entrepreneurs are not globally rational. The third paper is by Esther-Mirjam Sent, Chapter 5. Sent's paper discusses Simon's contributions, including his work on: complex hierarchical systems; aggregation, causality, and identifiability in econometrics; cognitive psychology; artificial intelligence; bounded rationality; and his differences with Kahneman and Tversky. Chapter 12 is by Manuel Scholz-Wäckerle, a Senior Lecturer of Socioeconomics in Vienna University, and is about "meso behavior." Meso behavior is defined/described as

a particular kind of economic behavior that is not integral part of the homo oeconomicus model. This behavior is called meso because it is neither part of micro- nor of macroeconomics alone and it is shaped systemically through interactive socio-economic associations. Thereafter meso is characterized through structure as well as process components of dynamic change.

In the fourth section it is shown that Veblenian and Schumpeterian agents are basically acting in terms of Simon's approach to satisficing behavior, a "precondition for . . . meso-structured behavior." Scholz-Wäckerle also discusses meso behavior in the writings of 2009 Nobel Prize winner Elinor Ostrom. Elinor Ostrom is also featured in Chapter 11, "Behavioral rules: Veblen, Nelson-Winter, Ostrom, and beyond" by Georg Blind.

Two chapters are on the founders of experimental economics: Reinhard Selten and Vernon Smith, Selten's work beginning in the late 1950s and Smith in the early 1960s. Selten received the Nobel Prize in 1994 and Smith in 2002. Selten's work was applied to both fully rational and not-so-fully rational players. His work with C. C. Berg in 1970 described framing, 11 years before the same concept was published by Kahneman and Tversky. In Chapter 6 Rosemarie Nagel et al. describe Selten as a "dualist" because he presented economics from both a normative (assuming rationality) and a descriptive (conducting experiments and developing behavioral models) approach. In Chapter 7 Shabnam Mousavi presents Vernon Smith and Gerd Gigerenzer on

ecological rationality and heuristics. She compares Smith and Gigerenzer with respect to ecological rational, bounded rationality, heuristics and experiments.

Floris Heukelom provides chapters on two more recent names in the history of behavioral economics: Richard Thaler (Chapter 8), and Daniel Kahneman (Chapter 9), winner of the Nobel Prize in 2002. Heukelom takes us chronologically through Kahneman's very productive career, saying that "it is easy to observe that the central idea in Kahneman's work is that human decision making is best understood as the combined outcome of two cognitive systems," which Kahneman refers to as System 1 and System 2. Heukelom also takes us chronologically through Thaler's career, saying that

one could argue that Thaler's economic world view has been remarkably constant over the course of his career of now almost forty years. Economic theory tells us how we should behave in the economy, and economists should be more concerned with finding out if and when people behave along those lines. If not, economists should devise ways to help individuals do so.

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THE EVOLUTION OF BEHAVIOURAL ECONOMICS

Peter E. Earl

Introduction

There are many ways in which one might tell the story of behavioural economics. It has a much longer history than many of its current proponents realise, a history that behavioural economics itself can be used to understand (for an early attempt to offer a reflexive analysis of the state of behavioural economics, see Earl, 1983a). If judging purely from the advance reviews by Chip Heath and Daniel Kahneman of Richard Thaler's (2015) book *Misbehaving*, one would believe that it was Thaler who invented behavioural economics. This might indeed be true for what nowadays typically passes for behavioural economics. However, such claims contrast sharply with the perspective offered by Baddeley (2013), who begins her textbook with a survey of psychological perspectives on choice that goes back to eighteenth-century contributions by David Hume and Adam Smith. Yet, despite such a long historical sweep, Baddeley's account is very light on what Sent (2004) calls 'old behavioural economics'—that is, behavioural economics pre-Thaler or recent behavioural contributions in the spirit of the 'old' approach. The same can be said of Cartwright (2014), who similarly sees behavioural economics as beginning with Adam Smith's (1759) *Theory of Moral Sentiments*.

In the present survey, the focus is not on how 'new' behavioural economics has evolved but on the earlier contributions that have been left behind rather than being integrated with the new approach. The inclusion of 'evolution' in the title provides a clue to the approach that is taken and why the starting point is the publication of Alfred Marshall's (1890) *Principles of Economics*, ninety years before Thaler's (1980) seminal paper in the first issue of the *Journal of Economic Behaviour and Organisation*. Marshall not only built his analysis on his knowledge of actual behaviour, as a behavioural economist would, but he is increasingly being recognised as one of the founding fathers of evolutionary economics, a research programme that is both closely related to 'old behavioural economics' and is instructive for understanding how what is viewed as behavioural economics has changed since 1980.

Marshall and evolutionary analysis

Marshall's thinking was greatly influenced by evolutionary biology (see Hart, 2013). This arm of biology views the evolution of species populations as arising via the following process: (i) genetic

mutations occur, (ii) mutations affect survival chances of the organisms in which they are embodied, and (iii) a mutation may be passed down to later generations if the organism in which it is embodied breeds and its progeny survive into adulthood. Inspired by evolutionary biology, Marshall ended up concerned with the struggle of firms to get established and remain competitive in a world where gradual change, not equilibrium, was the order of the day. He thus likened the competitive struggles of firms in an industry to those of trees within a forest where many plants fail to get enough sunlight and nutrition to enable them to grow to maturity. He did not go as far as later writers, most notably Nelson and Winter (1982), who assigned to routines in economic and social systems a role akin to that of genes in biological system as the key elements that get passed from one generation to the next. Innovative routines may give a firm a competitive edge over rivals, enabling it to earn greater profits; indeed, radically different routines may greatly disrupt an established order that had been evolving steadily. If new routines are retained and come to be employed more widely (for example, via internal growth of the firm or by being spread over a wider geographical area via a franchise system), then the new way of doing business may account for a growing share of economic activity. This will continue until routines that are even better suited to passing the test of the market are developed and applied. Where Marshall emphasised gradual change, modern evolutionary economists have emphasised, via Schumpeter's (1943) notion of 'creative destruction', the possibility that the history of economic systems may be punctuated by revolutionary shifts in which one way of doing business comprehensively renders another obsolete.

There are obvious parallels between the evolutionary gradualism of Marshall's analysis and the notions of 'normal science' and the use of 'scientific research programmes' in scientific inquiry, and between the idea of game-changing new business modes and Kuhn's (1962) analysis of 'scientific revolutions'/'paradigm shifts'. However, if we apply the perspective of evolutionary economics to the market for economic ideas, it becomes apparent that good ideas may fail to be transmitted down the generations if they fail to survive the selection process, including the process of selection into the educational equivalent of franchise manuals; namely, the textbooks that themselves have to survive the test of the market. What is retained and replicated via today's textbooks in behavioural economics thus could be—and, indeed, is—very different from what might have been in them if the evolutionary mutation and selection processes had worked differently. A key factor here is the role of purposive individuals as creative agents and marketers of new potential routines for doing economics: unlike in biology, the mutations do not happen randomly. Instead, routines and personality-related factors may affect which elements are used to create novel contributions and how they are presented to potential adopters.

Marshallian behavioural economics

Marshall's own legacy illustrates the haphazard processes at work in the market for economic ideas. Most economists think of him as a key player in the development of the marginalist, supply-and-demand framework of mainstream equilibrium economics, not as a pioneer of evolutionary economics. Such received wisdom is the result of not reading Marshall at first hand and of the efforts of Marshall's successor, Pigou, and those whom he influenced, to remove the evolutionary aspects of Marshall's thinking from what they passed on from his *Principles* (see Hart, 2013). However, Marshall also deserves to be recognised as one of the pioneers of behavioural economics. His approach was not to employ convenient axioms in the manner of an 'armchair economist' but to build his analysis on what he had been able to discover from business people about the way that business worked. This led him to place learning—by consumers, about what they needed and how to source it reliably for a reasonable price, and by producers, about production methods and how to win customers for their output—at the heart of his analysis

(Loasby, 1978; Hart, 2013). He also viewed firms as organisations, not as black boxes. He emphasised the managerial succession problems faced by firms, along with the forces of inertia in large, well-established businesses that could make it easier for new firms to start winning customers despite having limited resources, experience or reputation (Marshall, 1923: 317–18).

Although Marshall's way of thinking was not retained by his Cambridge successor, Pigou, it resurfaced from the late 1930s onwards in the work of members of the Oxford Economists' Research Group (OERG) (see Wilson and Andrews, 1951; Young and Lee, 1993), particularly in the work of P. W. S. Andrews (1949). Central to this approach was getting to know business managers and administering questionnaires to them. The data thereby obtained raised questions about the interest-elasticity of investment and the role of marginal revenue and marginal cost in pricing choices. Decades before the 'contestable markets' revolution in industrial economics led by Baumol et al. (1982), it was clear to the Oxford post-Marshallians that competition was much on the minds of managers, even if they did not have large numbers of existing rivals. The managers seemed ever-fearful of the possibility of cross-entry by firms diversifying from other sectors where the capability requirements were rather similar to those required in their own lines of business. This led them to focus on developing long-term goodwill relationships with customers and not to be greedy with profit margins, in order to deter potential entrants.

Marshall argued that prices in an industry track the average costs of the industry's 'representative firm' in the long run, falling with accumulated experience and growth in the scale of production. The Oxford economists reworked this in terms of prices being the result of simple decision rules being used for dealing with complex competitive situations. They saw price-setting as involving the use of a conventional mark-up on 'full' or 'normal' costs. Firms would use their own costs as proxies for the costs of prospective entrants, adjusted in light of any information they had about their relative standing among existing players. Hence more (less) efficient firms would tend to operate with bigger (smaller) profit margins. The tendency for costs and prices to fall in real terms through time would be enhanced not merely by the effects that external competitive pressure had on the rate at which firms discovered better ways of doing things but also by what Andrews called 'internal competition'; that is, workers trying to improve their promotion prospects by demonstrating their abilities to see ways of improving productivity.

Although Andrews's research led him to emphasise the power of competition and to criticise models of imperfect/monopolistic competition (Andrews, 1964), he emphasised the use of targets by profit-seeking firms as a means of dealing with uncertainty. Failure to meet target levels of sales would normally result in experimentation with different marketing strategies rather than risking spoiling the market via destructive price competition. His highly discursive analysis was rich in content and saw simple decision rules as effective for dealing with the complexity of the manager's choice problem. However, it was typically seen as lacking in rigour and was misunderstood by mainstream economists (Irving, 1978).

In the US, the project closest in style to the OERG's behavioural research was Lester's (1946) investigation of firms' labour hiring practices and whether they were consistent with marginal productivity theory. This met with much the same fate as the Oxford work, even though the key critic, Fritz Machlup, was not actually opposed to behavioural research in principle. Machlup's (1946) critique was based on the view that Lester should have done a longer field study, more akin to an anthropological piece of research, before reaching his heretical conclusions (see Lavoie, 1990).

Bounded rationality and the behavioural theory of the firm

The work of the OERG and the Lester–Machlup debate were both noted in the early pages of Cyert and March's (1963) *A Behavioural Theory of the Firm*. Just as in Marshall's work, this took

an organisational view of the firm, and it gave a key role to simple decision rules, showing how they could be used in modelling firm behaviour via computer simulations. This book was a logical economics descendant of Simon's (1945) management classic *Administrative Behaviour* and was born out of close interaction between Cyert, March and Simon at Carnegie Institute of Technology, later Carnegie-Mellon University, in Pittsburgh.

Simon had provided a more formal ground than the post-Marshallians for building a theory of the firm around decision rules. He argued that the human mind lacks the cognitive capacity to do the kinds of computations that would be required for optimal decision-making, especially in the face of organisational deadlines. Simplifying decision rules have to be used to avoid getting bogged down when engaging in problem-solving: one decision typically contains many sub-decisions—such as how, and how long, to search for possible solutions, and what to infer about the prospective performance of rival possible solutions—rather than just a choice between given means to given ends. In the face of inevitable 'bounded rationality', decision-makers have to engage in 'satisficing' behaviour, setting targets ('aspiration levels') and searching, initially locally, until a prospective means of meeting them is discovered. To survive in a competitive environment, where optimal choices may be impossible to discover or to identify as such even if they have been made, the decision-maker simply needs to find solutions that are good enough, given the strength of the competitive pressures, as Alchian (1950) had noticed.

Soon after *A Behavioural Theory of the Firm* was published, Harvey Leibenstein (1966) offered the first of his many papers (collected in Button 1989) on what he called 'X-efficiency'. He was trying to get economists to shift from viewing inefficiency in terms of deadweight losses caused by relative prices being distorted by monopoly power, towards something more akin to the lay-person's view of it as a situation in which a firm was operating with higher costs than were necessary. Though he did not portray it as such, his thinking can be viewed as bringing together elements of Marshall's view (of how firms differ in what they know about production methods, and the role of managers in shaping a firm's performance) and the Carnegie School's view of firms as composed of individuals each pursuing their own interests.

Like Cyert and March, Leibenstein recognised that employees in an organisation might make the most of any opportunities to enjoy a quiet life. Internal competition among workers could be attenuated if workers suspected that efforts to stand out by being unusually industrious would be matched by their peers or would result in them being penalised socially for acting as rate-busters. However, if competitive pressure increased, productivity increases might follow. With their idea of 'organisational slack', Cyert and March had also recognised that firms could be achieving lower profits than they might have earned, and operating with higher production costs than they might discover under pressure. They saw this slack as resulting from lags in the adjustment of aspirations into line with higher attainments, combined with the different interest groups in the firm being reluctant to incur the downside risks of trying to extract more for themselves when they were meeting their aspirations. Leibenstein's X-inefficiency notion complemented this view but he emphasised more the scope for reducing unit costs via better management and better knowledge of best-practice methods (or improving on them) rather than merely changing the distribution of returns to the different members of the coalition that made up the firm.

These views of the firm implied an approach to economic policy rather different from Thaler and Sunstein's (2008) liberal paternalism. The latter centres on using gentle nudges to steer consumers towards better choices. However, if limited search and experimentation result in needlessly low performance levels, then the way to stimulate productivity improvements is to put in place policies that make it harder to reach aspiration levels and/or to prevent firms from selecting their normal default options. Policies based on offering rewards could be of limited use

if firms do not notice or respond to the incentives that are offered (Cyert and George, 1969). The ‘old behavioural’ approach is consistent with Hayekian policies of market liberalisation and the corporatisation, privatisation or outsourcing of public sector activities, which are all aimed at increasing competitive pressure. But it could also imply that regulatory policies could be used to jolt firms into discovering ways of operating in a leaner and fitter manner. For example, Loasby’s (1967) field research on how the UK’s regional policies affected managers’ decisions showed that when firms were denied Industrial Development Certificates for their preferred locations this prompted their managers to have a major rethink, as a result of which they discovered better ways of running their firms. Nowadays, we might recognise that environmental regulations could have benefits, not merely in terms of the environment but also via the pressure that they posed on firms to find better ways of doing what they do.

As is evident from the thousands of studies of efficiency and productivity employing data envelope analysis and stochastic frontier analysis techniques, econometricians have proved open to the idea that firms differ in efficiency and frequently may not be operating at best-practice levels of productivity. (After a rather slow start in the twentieth century, research specifically aiming to measure *X*-efficiency has taken off strongly in the new millennium—about 175 studies are reported in a survey by Frantz, 2015.) But the same cannot be said for economists, despite the potential contradiction between advocating neo-liberal policies to improve economic performance and believing that firms should be modelled ‘as if’ they maximise profits. Adopting the satisficing view entails adopting a general framework that asserted that choices are based on decision rules (including rules for adjusting aspiration levels) that can take many forms. One can guess what these rules might be in a particular situation, and model their implications for behaviour, but to know whether one’s analysis might be a good approximation it would be necessary to begin by studying the kinds of rules people actually use in the context in question—and it might be the case that people are using a diverse set of decision rules. This is very different from the standard approach of trying to derive results from preference orderings and production functions of a very generalised kind and assuming that consumers or firms are all alike.

It is important to note that the behavioural economics of the 1950s and 1960s was not offered with calls that mainstream economists needed to start again from scratch. Leibenstein considered himself to be a neoclassical economist who was simply asking his peers to acknowledge the presence of selective rationality and *X*-inefficiency, and to try to take account of these phenomena in their work. Simon viewed his bounded rationality/satisficing approach as a constructive contribution to mainstream economics (see the correspondence from Simon to Earl quoted in Earl and Peng, 2012). But it was impossible to incorporate it into the increasingly tightly defined core of microeconomic theory because it clashed with the view that all economising behaviour should be viewed as an act of constrained optimisation. To argue that in *some* situations satisficing was a rational way of choosing, whereas in simple, pre-defined choices optimisation would be feasible, would be problematic for a ‘one size fits all’ approach to economics. Hence, the only way for rule-based behaviour to be rendered acceptable to the mainstream economist was by modelling computational limitations as an additional constraint and then theorising in terms of optimal decision rules. This was done by Baumol and Quandt (1964) and is essentially what has happened with modern-day models of bounded rationality.

From an evolutionary standpoint, Simon’s view of decision-making poses an even more fundamental challenge to the mainstream. Winter (1964) realised that if humans take time to gather and process information, then those who try to speed up and simplify their choices by using decision rules might be able to out-compete those who try to find optimal solutions, since the latter’s choices may be out of date by the time they materialise. The firms that survive

competitive selection processes might thus be the satisficers, not those that sought to optimise. It could thus be unwise to model markets ‘as if’ populated by firms that maximised profits.

This view contradicts the famous claims of Friedman (1953) (which had been based on a misinterpretation/misrepresentation of Alchian, 1950—see Kay, 1995) that competitive pressures would ensure firms ended up maximising profits even if they did not actually do the kinds of calculation presumed in the orthodox theory of the firm. However, the ‘as if’ approach was kept alive via Day’s (1967) paper, where it was argued that, via a succession of iterative adjustments, satisficing firms could, sooner or later, stumble upon the best choices. Such a conclusion required the choice environment to be static, which, as Winter (1971) point out, it would not be in a world of Schumpeterian innovating entrepreneurs. But since the mainstream economists were focused on static equilibrium configurations and had not bought into Schumpeter’s world-view, they felt they could ignore Winter’s contributions (if they were aware of them) and appeal to Day’s paper if the need arose to reject critiques based on satisficing ideas. Ironically, Day himself went on to spend much of the rest of his career making major contributions to the analysis of technical change and chaotic, dynamic systems, consistent with Winter’s perspective.

Thus, although Simon was awarded the 1978 Alfred Nobel Memorial Prize in Economic Sciences for his analysis of decision-making in organisations, and although Cyert and March’s behavioural theory of the firm has achieved well over 20,000 citations on Google Scholar, the Carnegie approach to behavioural economics, like that of the Oxford post-Marshallians, failed to become incorporated into mainstream economics. (Leibenstein fared no better at persuading mainstream economists to adopt his X-efficiency approach, despite his 1966 paper notching up over 4,500 Google Scholar hits.) However, the mainstream is going to find it hard to argue with the burgeoning empirical findings surveyed in Frantz (2015). The Carnegie School’s citations mostly ended up coming via research in management and organisational behaviour. The main long-term carry forward of the ideas of Simon, Cyert and March within economics was to be via the evolutionary analysis that developed from Nelson and Winter’s (1982) book *An Evolutionary Theory of Economic Change*.

Post-Marshallian inputs have also been significant to the evolutionary research programme, in the form of the analysis of corporate growth and industrial organisation offered by Penrose (1959) and Richardson (1972). Both base their analysis on detailed case knowledge (see Finch, 1999) and emphasise that firms differ in their capabilities, with Penrose also highlighting how limits to the rate at which managers can learn affect the rate at which firms can grow successfully. However, both of these contributions have had a bigger impact in research on business strategy, with many of their citations being as foundations for the ‘resource-based view of the firm’ and coming from business school scholars rather than from economists.

Change of focus: behavioural analysis of consumer choice

Focused as they were on opening up the black box of the firm, the Carnegie School did little to extend their behavioural analysis of decision-making to the realm of consumer behaviour. That it might be wise to do this ought to have become apparent after the publication of Lancaster’s (1966) reformulation of standard consumer theory into a model of household choices framed in terms of the characteristics offered by products. Viewing choice in this way was an aspect of Marshall’s thinking (Loasby, 1978) and was also proposed by Ironmonger (1972) in a book based on his Cambridge PhD dissertation that predated Lancaster’s much better-known work. Framing choices in terms of selecting from rival bundles of characteristics provided a way of making sense of how consumers could deal with novel products (as more efficient new means of producing outputs on various existing characteristics axes). However, with a large range of products that

promised significantly different combinations of characteristics outputs, consumers would face a major computational challenge if they were aware of all of their possible options and tried to weigh up all of the different combinations of characteristics in order to work out which product offered the best mix. In reality, their search processes might limit the scale of the information-processing task by causing them to stop well short of discovering all the available options and, if even a partial list left them with information overload, they might cope by applying simplifying rules and routines.

Potential for a Carnegie-style behavioural theory of the consumer was seized in marketing and approaches based on problem-solving decision cycles rapidly found its way into marketing textbooks (Nicosia, 1966; Engel, Kollat and Blackwell, 1968) and monographs (most notably Bettman, 1979) before being attempted in economics (Earl, 1983b). Although presenting consumers as if they went through problem-solving decision cycles with the aid of simplifying decision heuristics, the information-processing view of choice came to recognise that precisely which procedures were used would depend upon the context of choice (Earl, 1986; Payne, Bettman and Johnson, 1993). In some situations, a checklist or a process of filtering out products in terms of a priority-based set of aspiration levels might lead to a decision despite there being many options, whereas in other cases such 'non-compensatory' procedures might be used to produce a short-list from which it would then be possible to make a selection by means more in keeping with Lancaster's (compensatory) view of performances on characteristics being traded off against each other. With only a few relevant characteristics and a few rival products, it might even be possible to choose not merely by working out overall evaluations in a manner akin to that envisaged by Lancaster but also with such evaluations being a function of some assessment of how social referents would view the selection of each option, weighted by the chooser's motivation to comply with the such social pressures, as presumed in the Fishbein and Ajzen (1975) model of behavioural intentions that has been frequently employed in the marketing literature.

These contributions failed to have any impact on how economists typically viewed consumer behaviour. This is not surprising: they emphasise the impact of the context of choice on decision-making processes, often rejected the principle of gross substitution (i.e., the idea that 'everyone has their price') and present choices as commonly being made in a filtering manner without all available information necessarily being used. Mainstream economists did not merely squander the potential for the characteristics-based approach to consumer choice to bring together economics and marketing (recognised by Ratchford, 1975); they even resisted replacing their traditional utility functions with the kind of characteristics-based approach that Lancaster and Ironmonger had offered, and it was not discussed in orthodox textbooks.

The most significant modern research on decision-making that tries to understand how ordinary people cope with the complex challenges of real life is arguably that of psychologist Gerd Gigerenzer and his colleagues on 'fast and frugal decision-making' (Gigerenzer et al., 1999; Gigerenzer and Brighton, 2009). This can be seen as a revival of the evolutionary perspective that emerged from Winter's (1964, 1971) challenge to the constrained optimisation paradigm. Like the 'new' behavioural economics, it assigns a key role to simple heuristics but its focus is on the vital role that heuristics play in facilitating effective decision-making. This contrasts sharply with the 'new' view that heuristics produce biased judgments that result in needlessly poor choices. Just as Kahneman (2011) has little to say about Simon's contributions, except insofar as Simon's work on expert chess players is consistent with his view of choices that are based on 'thinking fast', so he relegates any remarks about Gigerenzer's research to endnotes. In the 'new' behavioural economics of consumer behaviour, the focus has been on finding inherited heuristics that make all humans 'predictably irrational' (Ariely, 2009) in the same way, whereas the 'old' approach (such

as Earl, 1986) was more like that of a clinical psychologist focusing on how individuals' personally constructed systems of rules for coping with life could in some cases prove dysfunctional. Where the 'old' approach was open to marketing's strategy of segmenting consumers into groups with similar modes of operating, the 'new' approach has, in effect, used empirical evidence of heuristics and biases as the basis for continuing with the mainstream 'representative agent' method.

Confidence and uncertainty

The behavioural theory of the firm portrayed corporate decision-makers as engaging in 'uncertainty avoidance' and attempts to eliminate uncertainty via measures (for example, lobbying policymakers) aimed at achieving a 'negotiated environment'. However, Keynes (1936, 1937) had earlier suggested that when faced with situations in which 'we simply do not know' about the future, people tend to use simplifying procedures, such as copying the behaviour of those believed to have better capacities for choosing, or simply extrapolating the past into the future, or making leaps into the unknown on the basis of 'animal spirits' if the surrounding mood was one of confidence. Keynes's emphasis on the psychological underpinnings of investment and business cycles has carried into modern-day behavioural economics far better than the 'old behavioural' analyses considered previously, as is evident via its influence on Akerlof and Shiller's (2009) book *Animal Spirits*.

Before Keynes's writings on confidence came significantly to influence behavioural economics, it was psychologist George Katona who was well known in that connection (for a survey of Katona's life and work, see Wärneryd, 1982). Katona pioneered surveys of consumer sentiment at his Survey Research Centre at the University of Michigan, having realised that, in an affluent economy in which consumers enjoy discretionary spending, consumption demand depends not merely on the ability of consumers to spend but also on their willingness to do so. The animal spirits idea thus also needed to be applied to consumer choice: with consumer durables often being discarded before they are worn out, the timing of purchases could be affected by consumers being uncertain about their job prospects and abilities to service credit commitments. Business cycles could thus be driven by shifts in consumer sentiment ahead of any shift in the animal spirits of the business sector.

In *The Powerful Consumer* (1960), and in many of his other publications, Katona argued that the evidence shows that corporate advertising cannot control consumer demand (contrary to Galbraith, 1958) and that macroeconomic policy measures could be rendered ineffectual by consumer sentiment. Katona's work had widespread impacts, provoking both academic and business researchers to construct indices of consumer confidence. His ideas were well-established in economic psychology and in the kind of behavioural economics that was being done in the 1980s (see the macroeconomics volume of the handbook edited by Gilad and Kaish, 1986). However, Katona's view of the importance of the psychology of saving has not carried into 'new' behavioural economics. Akerlof and Shiller do not refer to him. For 'new' behavioural economists, the focus for applying psychology to saving behaviour is not on modelling shifts in consumer sentiment but on using nudges to ensure that consumers achieve the self-control that is necessary in saving up for retirement.

Modern behavioural economics has also failed to employ contributions by G. L. S. Shackle, one of the earliest converts to Keynes's view of the significance of confidence as a determinant of aggregate spending. Shackle's approach was more like modern contributions in that it employed psychological concepts within formal models (his life and work are surveyed in Earl and Littleboy, 2014). Shackle (1939) swiftly set out to understand how entrepreneurs decide, in the face of uncertainty, whether or not to embark on what he came to label 'crucial experiments'—choices

that could have irreversible major consequences and which do not lend themselves to analysis in terms of statistical probabilities. In the absence of probabilistic knowledge, decision-makers have to use their imaginations to derive conjectures about what might be possible and what could get in the way of imagined possibilities. Bounds to human imaginative capacities open up potential for surprise, and Shackle saw expectation-formation as involving reflection on how surprising imagined possibilities would be if they actually occurred. Assessments of potential surprise took the place of probabilities in Shackle's analysis but he did not view them as being used in an additive manner.

In his early work on potential surprise, Shackle presaged the Carnegie School's view that decision-makers use aspiration levels to discriminate between acceptable and unacceptable outcomes, for he saw the problem of choice being resolved with reference to thresholds of tolerance for poor outcomes and for potential surprise. However, after a decade of developing his ideas, he ended up with a different approach, more akin to Kahneman and Tversky's (1979) prospect theory (see Earl and Littleboy, 2014, chapter 8). In contrast to the subjective utility models that were offered as the mainstream way of dealing with absent data on objective probabilities, Shackle (1949) presented decision-makers as framing their conjectures in terms of gains and losses relative to a reference point. He also offered a theory of attention which predicted that, for each scheme under consideration, the decision-maker would end up focusing on one gain and one loss, thereby ignoring both less dramatic possibilities and outer possibilities that were harder to take seriously. Following such cognitive simplification, the decision-maker would rank the rival schemes on the basis of these pairs of 'focus outcomes', with Shackle's 'ascendancy function' view of the allocation of attention seeming to imply an S-shaped utility function akin to that in prospect theory.

Although Shackle's approach to choice under uncertainty initially attracted attention from leading economists such as Arrow and Klein, this interest was short-lived and economists adopted the subjective utility approach instead. Shackle's work did not go unnoticed by those who developed the behavioural theory of the firm, being cited favourably by Cyert and March (1963). However, Shackle made no attempt to try to align himself with the Carnegie School despite potential complementarities between the bounded rationality perspective and his view of the limitations to imagination and of focusing induced by finite attention. Rather, he objected to Simon's way of discussing rationality in terms of 'fully posed' problems (Shackle, 1969: 100). Later, Shackle (1985) attacked Simon for rejecting expected utility theory for its failure to address computational complexity rather than because the probability notion makes no sense in situations in which people do not repeatedly face the same kind of problem.

With the shift of the 'old' behavioural approach towards consumer behaviour, Shackle's framework was adapted into a satisficing, characteristics-based framework by Earl (1983b, 1986). Today, however, Shackle's analysis has become part of Austrian and post-Keynesian economics, whereas 'new' behavioural economists employ prospect theory, seemingly unaware of Shackle's contributions. As Kahneman (2011: 278–9) reports, the reference point idea around which prospect theory was built came to him and Tversky as a result of realising that, contrary to the assumptions of the subjective expected utility model, they had rather vague ideas about their total wealth but could more readily assess the implications of outcomes in terms of changes in their wealth.

The contrast between Shackle's failure to win converts for his potential surprise view and the success of prospect theory is striking. But so, too, is the extent to which the latter is based on a watered down view of the nature of choice—as were the experiments of Kahneman and Tversky that underpinned its assumptions (such as the impact of the endowment effect on the shape of the utility function). Genuine uncertainty, computational challenges and emotionally charged hopes and fears were all absent.

In not admitting any role for focusing or filtering process in choices involving complex payoff matrices, prospect theory fails to encompass bounded rationality. Moreover, it assigns no role for the imagination to consider what might be possible or for life-changing choices that entail ‘crucial experiments’. Loss aversion and the endowment effect came from experiments that revealed contradictions between what people would pay to get something and what they would then require as compensation for parting with up after they had been given it. From Shackle’s perspective, choice experiments would need to entail high stakes, but he would have had very mixed views about the pioneering attempt of Slonim and Roth (1998) to do this by spending their research dollars on studying an ultimatum game in a low-wage economy. Shackle did not just reject probability; his view of the role of the imagination in the choice process also led him to reject game theory on the basis that real-life games frequently involve potential for surprising the opposition via innovative ploys and for differing conjectures being constructed about the underlying payoff matrix.

Conclusion: behavioural economics as a tragedy of missed opportunities

On 9 February 2001 Herbert Simon died. Two days later, a pair of articles in the *New York Times* signalled that ‘old’ behavioural economics had been forgotten and the term ‘behavioural economics’ now applied only to the ‘new’ approach. Such coverage signified that the new approach had become mainstream. One of the articles (Lowenstein, 2001) was about Richard Thaler’s long but ultimately successful attempt to get his ideas established. The other (Uchitelle, 2001) was about the work of David Laibson, of a younger generation and a rising star at Harvard. Neither article mentioned Simon at all. Simon had collected his Nobel Prize but had not focused his efforts on changing economics, whereas Thaler had been tireless at doing this despite initially succeeding in making an impact more in marketing and finance. Though heretical in his use of anecdotes, he succeeded by devising a version of behavioural economics that could be accepted by the mainstream by enabling it to deal with anomalies through a twisted version of the rational choice model. Others followed, and textbook franchises were established. ‘Old’ behavioural economics did not enjoy such evolutionary fitness.

All this seems tragic to ‘old’ behavioural economists. Instead of creating a general view of choice based on the application of rules and routines that may, depending on context, be fast and frugal or dysfunctional, most modern behaviourists have ended up with a focus on the systematic and predictable incompetence of consumers. Where once the behavioural theory of the firm offered potential for doing industrial economics mindful that firms are complex evolving organisations, we have modern behavioural industrial economics focusing on how firms behave strategically to exploit the failings of consumers—unless prevented from doing so by policies emerging from behavioural law and economics. And research has focused predominantly on closed decision problems, often with simple payoff matrices involving insignificant (or, if larger, merely hypothetical) betting choices, rather than on the kinds of situations in which decision-makers use their imaginations to envisage possibilities.

In terms of behavioural economics itself, the fact that this has happened should be no more surprising than the failure of ‘old’ behavioural economics to become part of every economist’s core theoretical toolkit. Academia is not populated by humble maximisers of the growth of socially useful knowledge in their research fields. Rather, academics are boundedly rational individuals who have their own goals to pursue and incomplete, heuristics-driven knowledge of relevant literatures. In the face of time pressure, what matters is knowing enough to produce papers that referees, with similar limitations, will deem acceptable. Search rules and cognitive heuristics may ensure that economists fail to discover alternative approaches and that they form

biased assessments of the merits of what they encounter or of their own research. The modern behavioural economist ends up doing behavioural economics in the modern way, which typically means doing it oblivious of earlier traditions or their extension into modern evolutionary economics. Textbook writers have a key role in determining whether a more radical grand synthesis will emerge and become widespread.

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