

**Kahneman's *Thinking, Fast and Slow* from the
Standpoint of Old Behavioural Economics**

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Abstract

Daniel Kahneman's bestseller *Thinking, Fast and Slow* presents an account of his life's work on judgment and decision-making (much of it conducted with Amos Tversky) that has been instrumental in the rise of what Sent (2004) calls 'new behavioural economics'. This paper examines the relationship between Kahneman's work and some key contributions within the 'old behavioural' literature that Kahneman fails to discuss. It shows how closely aligned he is to economic orthodoxy, examining his selective use of Herbert Simon's work in relation to his 'two systems' view of decision making and showing how Shackle's model of choice under uncertainty provided an alternative way of dealing with some of the issues that Kahneman and Tversky sought to address, three decades after Shackle worked out his model, via their Prospect Theory. Aside from not including 'loss aversion', it was Shackle's model that was the more original and philosophically well-founded.

Keywords: Prospect Theory, satisficing, bounded rationality, choice under uncertainty

JEL Classification Codes: B31, D03, D81

1. Introduction

In his highly successful 2011 book *Thinking, Fast and Slow* Daniel Kahneman offers an excellent account of his career-long research on judgment and decision-making, much of it conducted with the late Amos Tversky. Kahneman was awarded the 2002 Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel for this work. He shared the prize with Vernon Smith, the experimental economics, but there is not doubt that it would have been shared also with Tversky had he still been alive. For economists, the key publication from Kahneman and Tversky's research program is their 1979 *Econometrica* paper 'Prospect Theory: An Analysis of Decision Under Risk'. Kahneman's coverage of the theory in his book includes an account of the theory's development and applications. Amongst other things, the account reveals that he and Tversky decided to try to extend their work into a psychological theory of choice after coming across a mimeographed paper by Bruno Frey that set out the psychological assumptions that underpinned the economic theory of rational choice, assumptions that were startlingly different those that their research implied would be appropriate (see Kahneman, 2011, p. 269).

It is doubtful that what Sent (2004) calls 'new behavioural economics' would have taken off and become pretty much part of mainstream economics in the past two decades if Kahneman and Tversky had not developed Prospect Theory and published it where they did. It serves a role in the theory of choice that is analogous to the contributions made by Baumol (1958), Marris (1964) and Williamson (1964) to the theory of the firm that paved the way to agency theory. These contributions offered alternatives to the profit-maximizing view of the firm that took account of the discretion permitted to managers by

imperfections in capital and product markets but were grounded in terms of mainstream notions of constrained optimization. Prospect Theory was offered as an alternative to Subjective Expected Utility (SEU) theory. Like the managerial theories of the firm, it admits imperfections—in its case, heuristics that produce biased evaluations of possible courses of action—but otherwise follows the standard constrained optimization approach. Where the managerial theories of the firm pointed to the need to devise incentive systems to ensure that managers' interests aligned with those of shareholders, Prospect Theory points to the need for policies to 'nudge' decision-makers towards more rational choices, for example by reframing the options that they face (see Thaler and Sunstein, 2008).

In showing how combining heuristics and biases with constrained optimization could generate superior empirical results, Prospect Theory did not merely earn itself a place in *Econometrica*; it also provide the key role model for new behavioural economics. However, as Kahnemmand (2011, p. 271) notes, he and Tversky submitted the paper for publication in *Econometrica* not with a view to ensuring it had an impact in economics but because '*Econometrica* just happened to be where the best papers on decision making had be published in the past and we were aspiring to be in that company'.

Prospect Theory's success within economics stands in sharp contrast to the current status of two more radically innovative alternatives to SEU theory that were developed much earlier, namely, Herbert Simon's 'satisficing' analysis of choice in the face of 'bounded rationality' and George Shackle's 'potential surprise' theory of choice under uncertainty. These 'old behavioural' contributions nowadays attract little interest despite Simon's analysis having

earned him the 1978 Alfred Nobel Memorial Prize in Economic Science and despite Shackle's theory initially attracting considerable attention at the highest levels (for example in the survey on choice in risk-taking situations by Arrow, 1951) and displaying the bold originality that might have earned him a similar award had the theory ultimately been widely adopted. Since the three approaches take issue with SEU theory in different ways, it seems worthwhile to explore whether they should be seen as inherently incompatible substitutes or whether they were potentially complementary and, if the latter, why connections were not drawn between them to produce a powerful synthesis. It is the goal of paper to provide such an exploration by reflecting on the history of these approaches and the history that might have unfolded if their originators had sought to build bridges rather than emphasize distinctiveness.

The rest of the paper is structured as follows. Section 2 sets out Simon's approach. Section 3 then explores the relationship between it and Kahneman's work. Section 4 outlines Shackle's perspective, after which sections 5 and 6 respectively consider differences and potential complementarities between it and the approaches of Simon and Kahneman. Section 7 offers concluding reflections on the continuing differentiation between these three alternatives to SEU theory.

2. Herbert Simon: bounded rationality and satisficing

Herbert Simon (1916–2001) developed his view of decision making from the mid 1940s onwards and brought his key ideas together in his 1957 book *Models of Man* and his 1959 survey article. His focus was on the information-gathering and computational challenges faced by real-world decision-makers. Uncertainty

about outcomes adds greatly to the computational demands of working out the best course of action if there is a wide range of probable outcomes for any outcome dimension, drastically compounding problems that may exist anyway due to there being many options between which to choose and multiple outcome dimensions that are valued by the decision-maker. SEU theory ignores these computational issues and also presumes that the menu of options and their associated probabilities has already been defined. If the decision-maker first has to discover what the problem actually is and which potential solutions might be available, the task of choosing does not merely expand from computation to information gathering but also requires a way of dealing with a problem of infinite regress regarding the question of what is the best way to search for the best way to formulate and solve the problem.

From Simon's perspective, optimization is impossible in typical real-world decision settings; rationality thus has to be replaced by 'bounded rationality'. The decision-maker's task then becomes one of coping with complex problems and uncertain outcomes by employing simplifying decision rules aimed at finding satisfactory solutions. His approach essentially sees choice as an experimental activity: targets are set, often with reference to what others seem able to achieve or what has been achieved in seemingly similar contexts, and these targets, or aspiration levels, are adjusted into line with attainments with a lag. A run of above-target outcomes eventually results in the target being raised, and vice versa. Risky decisions thus may not be evaluated by computing overall expected utilities but by considering which options are deemed 'too risky' or judge to offer 'good enough' prospects of meeting a target rate of return or a simple payback period criterion. Such judgments may be made by checking each

of the options under consideration for their conformity with a checklist of requirements that has served well in the past. Such checklist-based procedures are common in, for example, decisions regarding loan applications.

Although Simon emphasized that deliberative decisions would involve rule-based procedures, his later work, on decision making in chess (Chase and Simon, 1973a, 1973b), led him to recognize that decisions that sometime seem to be made purely on the basis of intuition, with little conscious processing, are actually arrived at by matching the current situation to patterns stored in the memory. Despite having limited computational power relative to the task of analysing the set of decision trees associated with feasible move, an expert player may be able rapidly trawl through a huge range of memorized cases if these are organized according to a hierarchical system, and judge the odds of particular outcomes on the basis of what happened previously in similar cases. Simon therefore offered a pluralistic view of choice: sometimes it makes sense to go with intuition (in his terms, pattern-matching), but sometimes it is wise to think carefully in terms of sets of rules. To judge the quality of a choice we should not look just at the outcome but also at whether it was arrived at in a way that was appropriate to the context. If it is hard to do consistently better than the average, the quality of choice processes that generate at least average outcomes should be judged on the cost of arriving at such decisions. Simon (1976) called this a 'procedural' view of rationality and it is a perspective carried into current literature via the work of Gigerenzer and his colleagues on what they label as 'fast and frugal' methods of choosing (see Gigerenzer and Goldstein, 1996; Gigerenzer *et al.*, 1999).

3. **Kahneman versus Simon: two different two-system views of choice**

Kahneman, too, takes a pluralistic view of choice rather than adhering to a one-size-fits-all approach. A central theme in *Thinking, Fast and Slow* is that the human brain can usefully be seen as having two systems for making decisions. System 1 operates in a fast way, taking intuitive, often subconscious decisions based on the information that is at hand as soon as it has formed an interpretation of what the question/problem at hand might be. Often this works successfully, despite System 1 using heuristics that produce systematic biases in what we do compared with what would be deemed rational from the standpoint of statistical decision theory. Sometimes, however, it can be highly dysfunctional, particularly when it results in choices that address questions that *can* be addressed, rather than the questions actually asked, rather as happens when students jump to conclusions about questions on examination papers. System 1 can be over-ruled by System 2, which engages in deliberative thinking and is ready to consider alternative interpretations, gather more information and not rush to conclusions. The trouble is, according to Kahneman, System 2 is lazy; it is prone to fail to intervene when this would be to the advantage of the decision-maker and it is not guaranteed to take its thinking capacity as far as it might on occasions where it does intervene. So, if we opt to deliberate rather than relying on our intuition we may still not produce as good a decision as we might have done with a bit more thought.

From Simon's standpoint, the failure of System 2 to over-rule System 1 as often as it might or to push deliberation further is something that should be explained in terms of satisficing in the face of bounded rationality: System is short of cognitive resources and is understandably prone to say 'That will do'.

But Kahneman does not try to link his work up to Simon's on this issue. Instead, he mentions Simon's work in a way that renders him immune to any suggestion that he has ignored Simon and yet gives no clue about the view of choice for which Simon received his Nobel award. Kahneman (2011, p. 21) acknowledges that the two-systems approach is widely used in psychology and he makes favourable mention of Simon's work on System 1-style choices by expert chess players (*ibid.*, pp. 11–12, 237, and an endnote on pp. 449–50, where he credits 'Simon and his students at Carnegie-Mellon in the 1980s for laying the foundations for our understanding of expertise'). He even lauds Simon as 'perhaps the only scholar who is recognized and admired as a hero and founding figure by all competing clans and tribes in the study of decision making' (*ibid.*, p. 237). However, his only wider comment about Simon's work is in an endnote to that remark where he writes (*ibid.*, p. 466) that:

Simon was one of the towering intellectual figures of the twentieth century. He wrote a classic on decision making in organizations while still in his twenties, and among many other achievements he went on to be one of the founders of the field of artificial intelligence, a leader in cognitive science, an influential student of the process of scientific discovery, a forerunner of behavioral economics and, almost incidentally, a Nobel laureate.

Clearly, we cannot accuse Kahneman of not having the humility to recognize Simon's standing, but we can reflect on his failure to consider how his own work might look if he actually discussed, in the main body of his book, the analysis of

decision making for which Simon received the 1978 Nobel award. To put it simply: while Kahneman and Simon would have little to argue about regarding intuition, heuristics and biases, their views about deliberation are poles apart.

As regards Simon's attitude to Kahneman and Tversky's research, his 1983 book *Reason in Human Affairs* is particularly telling. This book is based on a series of lectures he gave at Stanford University and in the preface (p. vii) he mentions Tversky as one of his Stanford friends. In the first chapter he goes on to refer to Tversky and Kahneman (1974) when he says that

In typical real-world situations, decision-makers, no matter how badly they want do so, simply cannot apply the SEU model. If doubt still remains on this point, it can be dissipated by examining the results of laboratory experiments in which human subjects have been asked to make decisions involving risk and uncertainty in game-like situations order of magnitude simpler than the games of real life. The evidence, much of it gathered by Amos Tversky and his colleagues, leaves no doubt whatever that the human behavior in these choice situations—for whatever reason—departs widely from the prescriptions of SEU theory [fn to Tversky and Kahneman, 1974]. Of course, I have already suggested what the principle reason is for this departure. It is that human beings have neither the facts nor the consistent structure of values nor the reasoning power at their disposal that would be required, even in these relatively simple situations, to apply SEU principles.

Simon's focus was thus on complexity and computational difficulties getting in the way of attempts to choose in the manner envisaged in SEU theory, whereas Kahneman and Tversky were showing that subjects in experiments were prone not to apply sound statistical principles when they were presented with simple choices under risk that involved closed sets of information. For Simon, System 2's problem was that of having to choose how much information to gather, given a limited capacity to gather information and apply reasoning to it. From Simon's standpoint, heuristics are essential for making life manageable, whereas Kahneman's message is that heuristics are dysfunctional, something he can demonstrate because he confines his analysis to simple situations in which a rational choice can be identified. No wonder, then, that Kahneman (2011, pp. 449, 457–8, 461) is openly hostile to Gigerenzer for his positive view of what people can do using rather simple heuristics.

By the time that Simon gave his Stanford lectures, Kahneman and Tversky (1979) had already drawn some of the lessons of their heuristics and biases research into their Prospect Theory. This theory, also central feature of *Thinking, Fast and Slow* is essentially a revised version of SEU theory: it introduces a new, S-shaped value function, whose point of inflexion is at the zero point on a gain/loss scale, whereas the SEU value function is expressed in terms of diminishing marginal utility of total wealth. The S-shaped function is a way of incorporating loss aversion and the willingness of people to take big risks in order to have a chance of avoiding losses. Rather than simply using probabilities as weights, Prospect Theory works on the basis of a probability editing function that gives unduly high weight to low probability events and unduly low weight to high probability events. Prospect Theory thus disposes of none of the issues that

Simon raises but these issues do not surface in *Thinking, Fast and Slow*:
Kahneman offers no discussion of complexity and bounded rationality and instead keeps presenting his readers with examples of simple alternative bets with either/or outcomes that each have a specified probability attached.

Kahneman (2011, p. 270) tries to justify this approach by arguing as follows:

Simple gambles (such as a “40% chance to win \$300) are to students of decision making what the fruit fly is to geneticists. Choices between gambles provide a simple model that shares important features with the more complex decisions that researchers actually aim to understand. ... Every significant choice we make in life comes with some uncertainty—which is why students of decision making hope that some of the lessons learned in the model situation will be applicable to more interesting everyday problems. But of course the main reason that decision theorists study simple gambles is that this is what other decision theorists do.

By continually presenting choices as simple betting puzzles, Kahneman makes excellent use of Prospect Theory as a means of explaining a variety of observed kinds of behaviour and for pointing towards policies. But its status in relation to the System 1/System 2 dichotomy is left rather up in the air. When it is portrayed as an alternative to SEU theory, it seems to be a presentation of how a real human should be thought of as thinking when carefully computing values in the light of available information. That sounds like System 2: the calculation process is basically as in SEU theory but the values and weights are different.

However, Simon's perspective on the challenges involved in deliberation leaves no place for it as a System 2 analysis in more complex situations: if the human mind had all the necessary information on complex probability distributions, with these distributions varying across different outcome dimensions (for example, different product characteristics) for rival schemes of action, then it would be too hard to compute. If Kahneman had taken due account of Simon's work on deliberation, he would be rejecting SEU theory outright, rather than, in effect, trying to modify it to address empirical anomalies.

In reality, System 2 thinking will involve selective gathering of information using simple stopping rules when searching, while alternatives will end up getting ranked not by computing overall value scores but by cognitively workable decision rules, such as those recognized by Tversky (1969) and studied empirically in the adaptive, information-processing view of choice offered by Payne *et al.* (1993). In other words, it is not that System 2 decision making would result in the mind computing rankings somewhat differently from what SEU theory would lead one to expect; rather that process of reaching any ranking has to be computationally completely different in order for it not to founder in complex situations.

If Kahneman had given due credit to Simon's work, he would have needed to say the Prospect Theory is a heuristic device for predicting the kinds of decisions that real-world decision-makers will make when:

- (a) they do not go beyond an initially simple view of the problem (in Kahneman's terminology, if they act as if 'What You See Is All There Is' in a

System 1 manner, without looking for a wider range of alternatives or trying to get more information about ranges of possible outcomes); or

(b) they have opened up the problem in a System 2 manner by searching for alternative interpretations, possible solutions and probable ranges of outcomes, but have then, via some earlier stages of elimination, managed to turn it back into a simplified risky choice situations that permits them to weigh up a few rival outcomes for a few rival schemes.

Of course, had Prospect Theory been offered with these caveats, its appeal to modern mainstream economists with 'new behavioural' inclinations would have been far less: conventional rational choice theory may have empirical limitations, but it concedes no place for simplification by the decision-maker. Presenting Prospect Theory in relation to simple betting puzzles kept it well clear of non-standard notions such as intuition or deliberation involving a whole sequence of sub-decisions.

But the whole value-adding-up idea behind Prospect Theory looks questionable from Simon's satisficing perspective: if we recognize the complexity of situations about which real-life deliberation is undertaken, and if we also take seriously Kahneman's notion of loss aversion, the implication might be that his empirical work pointed towards decision-makers being viewed as considering whether the odds were 'satisfactory' in terms their favoured decision rules and excluding options that failed their tests of adequacy, including the kind of 'safety first' requirements examined by Roy (1952).

4. George Shackle: possibilities and potential surprises

Like Simon, George Shackle (1903–1992) proposed an analysis of choice that takes into account the limited cognitive capacities of human decision-makers and throws up the issue of infinite regress. He developed his analysis via a series of articles in the 1940s into his 1949 book *Expectation in Economics* and reiterated it in other books (such as Shackle, 1961, 1979) and numerous further articles. Central to his analysis is the limited attentive capacity of the human mind and the absence of any mechanism to ensure attention gets allocated optimally. While he emphasized the ability of the human mind to imagine novel possibilities by combining existing elements (Shackle, 1979), he also recognized that many connections that might be made in the imagination do not get made when people are forming expectations, so surprise is a fact of life. People often find themselves dealing with outcomes they have not imagined and they are also prone to worry about imagined situations that never materialize.

Shackle rejected the idea that decisions in the face of uncertainty are or should be made by computing overall expected values in which probabilities are used as decision weights. If a decision is to some extent unique and/or has the capacity to change the decision-maker's future choice environment in an irreversible manner, then, for Shackle, it is illogical to approach the decision problem in probabilistic terms. Instead, he suggested, the decision-maker should consider which outcomes should be seen as possible and then judge the degree of possibility with reference to the extent to which each particular outcome could be prevented by the taking place of other events. If nothing can be imagined as a potential barrier to an outcome, then it is to be regarded as a perfect possibility, and its taking place would cause no surprise. By contrast, if

the decision-maker could imagine all manner of insuperable obstacles to an outcome, their answer to the question of how surprised they would be if it actually eventuated would surely be 'I'd be astonished'. Outcomes in the way of which the decision-maker could see some possible barriers would be assigned rather smaller degrees of potential surprise, depending on how potentially problematic these barriers seemed.

It is in assigning potential surprise that a problem of infinite regress lurks: an imagined barrier to an outcome could be removed by the taking place of another event, but that event might be precluded by yet another event, unless the latter were precluded by another, and so on. Finite powers of the imagination preclude going very far along such a regress, leaving the decision-maker with uncertainty, possibly seeing fewer serious obstacles than really warrant concern or being overly worried about some imagined obstacles to desired outcomes. This may sound essentially the same as Simon's notion of bounded rationality with uncertainty arising due to finite computation, but Shackle's view of uncertainty can be seen (for example, as in Dunn, 2000) as entails more than this. It is not merely a matter of following through the implications of possible sequences of events in a given environment comprising a complex system of interconnected components whilst lacking some of the requisite scientific knowledge regarding their relationships. Rather, the problem is that the system itself changes depending on how other decision-makers use their imaginations to come up with surprising new ideas.

How decision-makers form conjectures regarding rival schemes of action is thus shaped by their finite powers to imagine possibilities and follow their implications, but it is their finite powers of attention that come into play next to

shape their choices. Shackle portrayed decision-makers as focusing on a single gain and a single loss outcome for each option under consideration. From the standpoint of SEU theory, this seemed irrational, since it involves leaving aside conjectures about other possible outcomes. This is a sin that Prospect Theory does not commit: the probabilities are twisted and values attached to probable outcomes are twisted by loss aversion, but all probable outcomes are presumed to be taken into account—even though typically the examples offered only involve pairs of outcomes.

Shackle argued that large gains or losses are more attention-arresting than small ones and that the less surprising an outcome appears in prospect, the more it will capture our attention. He encapsulates this idea via an ‘ascendancy function’ partitioned into iso-ascendancy curves for gain and loss outcomes. His model shows how a U-shaped potential surprise curve—which must not be mistaken for an inverted probability distribution—representing the relationship between a scheme of action’s imagined outcomes and their assigned degrees of potential surprise will have two points of tangency, one in the gain zone and one in the loss zone, with the ascendancy function. In Figure 1, the bold line shows the potential surprise curve for a particular scheme of action and its most attention-arresting loss outcome is at point *A*, and its most attention-arresting gain is a point *C*.

It will be evident from Figure 1 that as well as being different from SEU theory in its replacement of probability with potential surprise and via the focusing process that it envisages, Shackle’s theory is also a radical departure in that it centres on gains and losses rather than portraying outcomes as having implications for the decision-maker’s total wealth. Shackle portrays gains and

losses as being relative to a 'neutral outcome' (N in Figure 1). The neutral outcome is seen as where the decision-maker stands at the point of the decision, or where the decision-maker could readily be if current wealth is augmented by being invested in a risk-free asset.

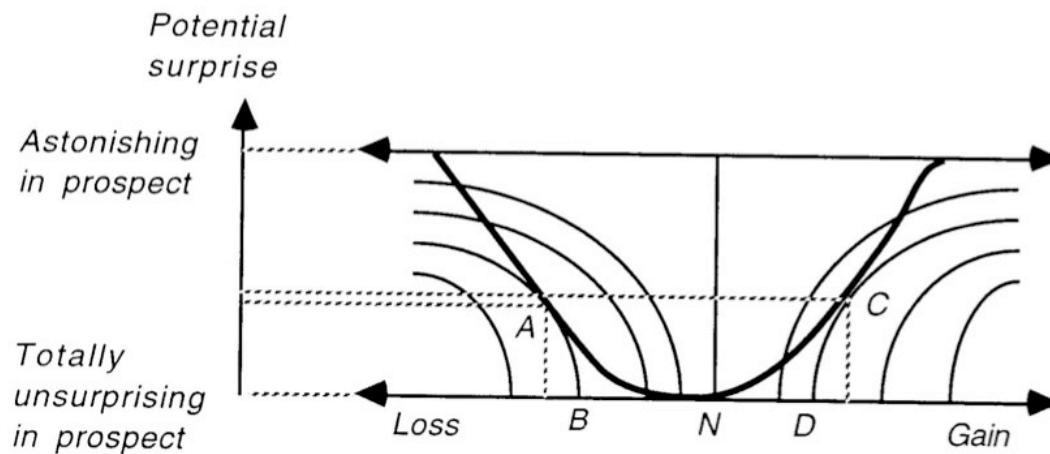


Figure 1: Focus outcomes for a single scheme of action

As Ford (1983, Ch. 4) observes, the focusing process predicted by Shackle was a major reason for the ultimate rejection of the potential surprise model despite the considerable interest that it attracting considerable interest from leading decision theorists in the 1950s: from the standpoint of SEU theory, it seems irrational not to employ all of the information one has assembled about an uncertain option. Shackle's chances of getting his theory accepted were also wrecked by discontent with the way he dealt the problem of how the decision-maker might compare the pairs of focus outcomes for each possible scheme of action in order to rank the rival schemes. Shackle was trying to avoid presuming this is done without adding up values assigned to the respective focus gains and

losses, weighted by their respective degrees of potential surprise. As a consequence, he ended suggesting that the pair of focus points *A* and *C* implied by the tangencies between the potential surprise curve and the ascendancy function are equivalent in attention-arresting power to outcome prospects of *B* and *D* if they were imagined as perfect possibilities. He therefore labelled *A* and *C* as ‘primary focus outcomes’ and *B* and *D* as ‘standardized focus outcomes’. He was then able to represent a scheme’s attractiveness by its location as a single point on a ‘gambler preference map’ of the kind shown in Figure 2.

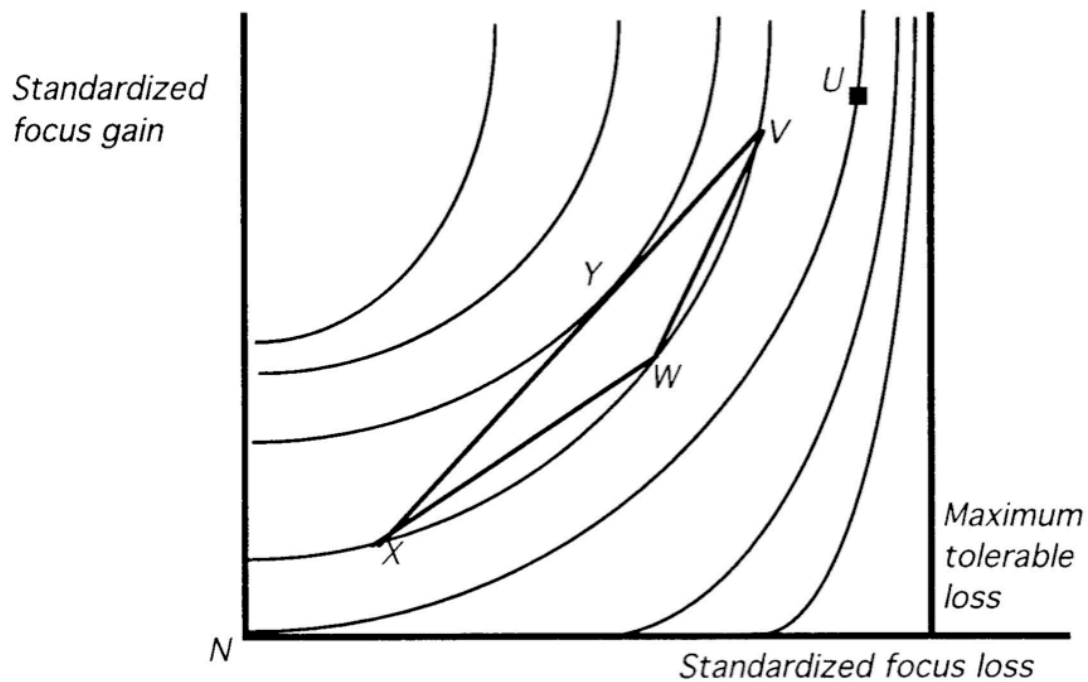


Figure 2: Shackle's 'gambler preference map'

In Figure 2, scheme *U* is no more or less appealing than the neutral scheme for that context and is less attractive than schemes *V*, *W* and *X*. Even better prospects can be achieved by following a mixed strategy that blends *V* and *W* or *W* and *X* or, better still, by combining *V* and *X* to get *Y*. This ranking process

did not seem intuitively to be like real-world decision processes due to the standardization stage for focus gains and losses; worse still, Shackle's theory also had trouble explaining diversification beyond two-asset portfolios.

5. Shackle and Simon

Given that Shackle and Simon presented their alternatives to SEU theory around the same time and in multiple publications and both offered analysis that recognized the limitations of human cognitive processes, we might have expected them to refer to each other in their writings in the 1950s and early 1960s. They could even have combined forces to argue a pluralistic position that allowed for intuitive, rule-based or Shackle-style decisions depending on the decision-maker and context. But, as far as I know, they neither referred to one another nor sought to build bridges, and the line showing maximum tolerable focus loss in Figure 2 owes nothing to Simon despite looking as though it could have been an application of his thinking (it also reminds one of Roy's (1952) 'safety first principle').

Simon is unlikely to have been unaware of Shackle's work, given the coverage it received in the early 1950s, such as in Arrow's (1951) survey article. Simon's close colleagues Richard Cyert and James March, who applied his satisficing perspective extensively in their 'behavioural theory of the firm', were well aware of Shackle's theory and clearly viewed it as compatible with their own: they refer to his (1949) *Expectations in Economics* (actually as *Expectations in Economics*) when arguing against traditional profit-maximization theory, saying that 'Shackle and others have argued that the theory grossly exaggerates both the computational ability and, more importantly, the usual computational

precision of human beings' and then mention Shackle's ascendancy function 'by which the attention value of a project is determined' (Cyert and March (1963, p. 46).

Shackle may well have been unaware of the enthusiasm that Cyert and March had for his theory, for (as far as I know) there is no reference to their book in his work, not even in his 1970 textbook on the theory of the firm, *Expectation, Enterprise and Profit*. However, if he had been aware of the behavioural theory of the firm, it would have presented him with a dilemma: if he embraced Cyert and March as potential allies, he would be supporting a theory that portrayed managers as trying to avoid or limit uncertainty rather than as confronting it like heroic entrepreneurs. Moreover, as in Simon's work, the focus of Cyert and March's analysis of the firm is on routine decisions rather than crucial choices.

The absence of citations by Shackle to Simon, Cyert and March in his work, and the lack of evidence in the Shackle archive in Cambridge that he had any correspondence with them might seem to imply he was unaware of their work. This impression is compounded when we see Shackle (1972, pp. 84–6) briefly writing about 'bounds of rationality' rather than considering Simon's notion of 'bounded rationality'. However, Shackle was actually aware of Simon's views on decision-making. When Simon brought together some of his key papers as his 1957 book *Models of Man*, it was Shackle (1959) who reviewed it for the *Economic Journal*. The review is, however, frustrating in relation to bounded rationality and satisficing, for it mainly lambasts Simon for over-use of mathematic and focuses on what he says about causality (though most of his remarks regarding the latter seem to be address to Herman Wold's views on the topic). The only chapter Shackle seems to have particularly liked was

'Bandwagon and Underdog Effects of Election Predictions' and there is no comment whatsoever regarding Simon's analysis of search, decision rules and satisficing, or the concept of bounded rationality. He concludes by saying that Marshallians and Keynesians will find it 'both impressive and irritating' and that 'It is unmistakably of top intellectual quality and packed with instruction'.

Shackle's review of *Models of Man* looks particularly peculiar given that in the year after the book was published he came remarkably close to restating his own theory in satisficing terms. In his De Vries Lectures, Shackle (1958, p 66, emphasis in the original) wrote that:

Rather than *minimax* our losses, is it not more reasonable to fix for them some *maximum tolerable* numerical size, to avoid any action-scheme which would bring losses larger than this within the range of 'too possible' outcomes, and *subject to this constraint* to choose that action-scheme which brings within the range of possible or 'sufficiently possible' outcomes as high as possible success as we can find?

In the early 1980s I developed this idea further and found Shackle receptive to satisficing versions of his theory when I communicated with him about them (see Earl, 1983, pp. 106–107). However, no such enthusiasm for Simon's view of choice was evident a few years later when Shackle (1985) wrote a book review note, again for the *Economic Journal*, on Simon's (1983) *Reason in Human Affairs*. What he reports is consistent with the passage of the book quoted earlier in this paper, namely, that 'Simon dismisses of SEU, not on grounds of its being

meaningless (as I would) but because no human mind could encompass the task [of constructing subjective expected utilities for rival courses of action]’.

6. Shackle versus Kahneman: possibility versus probability

Kahneman writes as if oblivious of Shackle’s model and critique of SEU. Indeed, he probably is, for Shackle is rarely referred to within the literature with which he and Tversky engaged. If *Econometrica*’s referees for the Prospect Theory paper had been familiar with Shackle’s theory, they would probably have realized that Prospect Theory is far less novel than it has been taken to be (and is presented as being in *Thinking, Fast and Slow*): the S-shaped value function of Prospect Theory, with its point of inflexion at a reference point that separates gains from losses, is something that Kahneman and Tversky (1979, p. 277 justify in terms of how perceptual processes work (i.e., we judge the strength of stimuli in terms of their difference relative to a point to which we have habituated); there is no mention whatsoever of Shackle’s work thirty years earlier. (Later, in a discussion of ‘Goals as Reference Points’, Kahneman (2011, pp. 303–4) also failed to make any link with Simon’s theory and the role played in it by aspiration levels.) Prospect Theory’s leap forward is not the partitioning of gains and losses around a reference point but its incorporation of edited probability weights (overweighting of small probabilities and underweighting of large probabilities) and loss aversion (implied by the empirical finding that ‘Losses are weighted about twice as much as gains’ (*ibid.*, p. 349)). With Shackle’s model having already avoided ‘Bernoulli’s Error’ (the title of chapter 25 of *Thinking, Fast and Slow*), Kahneman may be unwittingly wise to have implicitly downplayed that aspect of his contribution by saying that ‘The concept of loss aversion is certainly

the most significant contribution of psychology to behavioral economics' (*ibid.*, p. 300).

Kahneman should have considered Shackle's critique of probability when interpreting evidence of how people deal with uncertainty. A large part of *Thinking, Fast and Slow* is devoted to conveying the message that decisions are often compromised by failures in statistical inference or failures to look at relevant probabilities. Decision-makers commonly misunderstand how to combine probabilities: if an object or person has probabilities of being in two categories, they are likely to conclude that it has a higher probability of being in a combination of these categories than in either one of them. (In the case with which Kahneman begins chapter 15, a person with a background as a feminist activist was commonly seen as more likely to end up as a bank teller involved in feminist activities, than as just a bank teller.) They will be prone to extrapolate about the probable outcome of a venture on the basis of their experience with it so far (as Kahneman himself confesses to having done in estimating how long it would take to complete a partially developed curriculum design project) rather than seeing what lessons can be derived from a wider pool of cases that come into the same category. They will be susceptible to the 'planning fallacy', finding it easier to imagine things unfolding according to plan than to consider the combined likelihoods of many possible events that could derail their plan. These cognitive shortcomings can result in overconfidence and failures to abandon projects whose odds for success, if only they were examined, would not look good.

At the heart of Kahneman's focus on the statistical side of decision-making is the view that good outcomes depend on a combination of judgment and luck. A

competent decision-maker does not act as if What You See Is All There Is (or WYSIATI as Kahneman frequently abbreviates it) or make errors of statistical inference, and such a decision-maker weights risks by their probabilities. However, judgment can only go so far, and this is why one is left with probabilities: things beyond one's control or imaginative capacities can affect outcomes, so all one can do is look at the odds of success. If an outcome has a low probability, we will need a lot of luck because it is the kind of outcome in whose way lie many potential barriers.

Kahneman is clearly quite frustrated with the reluctance of some people to accept his view of the importance of taking the trouble to get the best base-rate probability information that is available and use it for decision weights. He ruefully notes how a lawyer who specializes in medical malpractice cases may have a good idea of the probabilities of winning and the sizes of settlements and yet make an assessment of whether to try to take a particular case to court in terms of its singular features and his or her assessment of whether it will be possible to beat the odds. Each case may indeed be unique but from Kahneman's perspective it appears that the rational way to decide how to proceed is to consider whether its special features are such that it can be said to belong to a specific class of cases whose outcome odds are different from the wider set of which it is also a member. A lawyer who fails to weigh outcomes by their probabilities is likely to be suffering from an illusion of control and committing the planning fallacy.

From Shackle's standpoints, there clearly *is* a role for base-rate probabilities if one is taking a decision in the face of uncertainty, but it is not the role assigned by Kahneman. Base-rate information signals the need to be

concerned about the possibility that unfolding events may get in the way of particular outcomes. Detailed base-rate information about the kinds of things that can go wrong may therefore be useful for forming expectations, since we can reflect upon whether or not such factors could apply in the context at hand. For example, if we know that long-term curriculum design projects often get disrupted due to team members suffering from health or marital difficulties, then we might take a close look at the health and marital situations of team members when trying to work out how long it could take to finish the project. Likewise, a lawyer's range of experience with particular classes of cases could be used in assessing whether a particular case has a lot or relatively little potentially standing in the way of getting a successful outcome for the client if the case goes to court.

Shackle's perspective does not rule out the possibility that human minds may be prone to failures of the imagination and therefore to be prone to optimism bias due to not recognizing particular possibilities or taking them sufficiently seriously or turning a blind eye to possibilities that could get in the way of outcomes on which hearts have been set. On the contrary, Kahneman's heuristics and biases research complements his analysis perfectly by providing a means of understanding and anticipating what the human mind will go through when sizing up how imperfect a possibility a particular outcome is. The glue to bind the potential surprise and heuristics and biases together could have come from Hayek's (1952) *The Sensory Order*, which portrays the process of characterizing things as involving the brain trying to find matches between incoming sets of stimuli and sets of neural connections that have been stored from past experiences. For the brain to be able to do this sufficiently rapidly, it

there *has* to be some kind of priming processes (rather than priming being seen, as Kahneman would have us see it, as a cause for concern) rather than a random sequence in which any past set is viewed as a contender. Hence it would not be surprising if frequently fired-up sets of neural connections (for example, those pertaining to outcomes that we lust after) crowded out those that have not been recently activated and therefore bias how we characterize the possibilities we are considering.

The history of economic thought could have come out quite differently if Shackle had been aware of the heuristics and biases findings (for example via Tversky and Kahneman, 1974). He could have tried to incorporate them into a revised version of his theory as part of his attempt to revive interest in his theory of choice via his 1979 book and papers from around this time. Unfortunately, all he did was continue to reiterate his long-standing messages. This was not surprising, given his age and his distance (living on the remote Suffolk coast at Aldeburgh) from any university library.

7. Conclusion

For Kahneman (2011, p. 288), the success of Prospect Theory is a consequence of its ability to offer strong predictive power despite leaving out particular aspects of reality that other theories have included (here, his specific contrast is with Regret Theory, a fourth alternative to SEU): as he puts it, 'Richer and more realistic assumptions do not suffice to make a theory successful'. This paper, however, supplements Sent's (2004) analysis of how psychology made its rather limited way into modern economics. It has emphasized how readily Kahneman's research, and Prospect Theory in particular, fits into the mainstream framework

compared with the older and bolder behavioural contributions of Simon and Shackle,

The goal of this paper has not been to downplay the significance of the empirical insights that Kahneman's research has yielded; the life's work summed up in *Thinking, Fast and Slow* is a remarkable achievement and the book is an excellent way to learn about what Kahneman and Tversky discovered. Rather, the paper has been offered mindful of lessons from Kahneman's work. In particular, it aims to guard against the prospect that readers of *Thinking, Fast and Slow* will succumb to WYSIATI and thereby will fail to take the trouble to go beyond what it has to say.

The evolutionary pathway carved out by any discipline is not simply a matter of how theories differ in their predictive capabilities. It is also the result of the connections that researchers make or whose possibility they choose to signal for others to make. Different possible disciplinary histories could emerge depending on the connections the researchers make. The actual set of connections made, and the actual pathway taken, depends on more than just the creative insights that researchers have about which connections to try to construct. Connections can only be made between elements of which the researcher is aware, but search for potentially useful element may fail to take place if the researcher can construct an exciting, or at least acceptable, set of connections the elements already at his or her disposal. It is possible that a researcher may be well aware of alternative sets of connections that could be constructed and yet strategically chose not to bring this alternative vision out into the open because it might call into question the set being advocated or make the researcher's contribution seem far less original. In some cases, the history

that onlookers observe may result from the combination of creativity, ignorance and strategy as determinants of the connections that get made by some researchers and then feed into the connection-making processes of others.

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