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the case of G. L. S. Shackle**

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Abstract

This paper deals with the intellectual environment in which George L. S. Shackle's theory of decision making was formulated and first discussed. Shackle's approach had a great impact on decision theory in late 1940s and early 1950s being the single formalised attempt to discard the probability framework in the description of behaviour under uncertainty - a goal shared by Knight and Keynes. Against Shackle, Arrow defended the use of probability theory in decision making, by denying that the Knightian distinction between risk and uncertainty had any behavioural significance, and paving the way to Savage's Foundations of Statistics as the new mainstream reference. Through an assessment of the reception of Shackle's theory the paper presents the way a number of theoretical economists, psychologists, and mathematicians were interested in the viability of a formally structured alternative to theories of behaviour using probability statements to describe uncertainty. The paper aims to show that the lively but concentrated discussion on alternative decisional criteria Shackle was part of is crucial to understand the multifarious developments observed in modern decision theory in the last twenty years or so. Indeed, as discussed in a twin paper by Basili and Zappia, Shackle's theory was a much more viable alternative to subjective expected utility than both its contemporary critics and modern decision theorists have recognised.

KEYWORDS: decision theory, uncertainty, Shackle

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1. Introduction

This paper deals with the intellectual environment in which George L. S. Shackle's theory of decision making was formulated and first discussed. Shackle published his first volume on decision theory, *Expectations in Economics*, in 1949, that is, in the years of the birth of the modern probabilistic approach to decision theory epitomised by the expected utility approach of von-Neumann and Morgenstern's *Theory of Games and Economic Behaviour*. In his volume Shackle proposed an approach intended to dispense with the probability calculus. The volume was followed by a series of articles in the 1950s through which Shackle took part in the discussion on the incoming mainstream. However after the 1961 restatement of his theory in the volume *Decision, Order and Human Affairs*, Shackle's idiosyncratic view was no longer referred to in the debate among decision theorists.

The great impact of Shackle's non-probabilistic approach in the 1950s is testified to by Arrow's (1951) survey of the state of contemporary decision theory. Arrow (1951, p. 404) identified three developments in the formalisation of the theory of choice that represented "dramatic breaks in continuity and have given hopes of a much clearer understanding of the problem." There were, first, von Neumann and Morgenstern's axiomatic treatment of choice in risk-taking situations (1947); second, Wald's transformation of the theory of statistical inference into a "special form" of the problem of rational behaviour under uncertainty (1950), and, third, Shackle's "new formulation of the whole problem of uncertain anticipations and actions based on them." Arrow singled Shackle's theory out as the sole formalised one among the theories aiming to discard the probability framework in their description of behaviour under uncertainty – a goal pursued by Knight and Keynes among others.

In the following years Savage's volume, *The Foundations of Statistics* (Savage 1954), started spreading its lasting influence and soon established itself as the new mainstream reference. However, Shackle's theory continued to be widely discussed in journals as one main alternative to the then consolidating mainstream. The theoretical economists, psychologists, and mathematicians who participated in this discussion were interested in the viability of a formally structured alternative to theories of behaviour describing uncertainty through probability statements. Contributors include Baumol, Carter, Edwards, Georgescu-Roegen, Gould, Niehans, and Weckstein among others. The debate over Shackle's approach culminated in a conference held at the Carnegie Institute of Technology in 1955 (Bowmann 1958), as well as in a monographic issue of *Metroeconomica* in 1959. Still in 1961 Ellsberg

(1961: 648) quoted Shackle as a main representative of the tradition of thought inspiring his claim that Savage's framework was unable to deal with the problem of choice on the basis of highly incomplete information.

But starting from the early 1960s the influence of Shackle diminished considerably, as testified by the few, and mostly dismissive, reactions to his 1961 volume intended to answer the criticisms he had received throughout the 1950s. Needless to say, both Shackle's theory of expectations and his peculiar treatment of time have continued to be referred to by a large number of authors. In particular, Shackle has been considered by many scholars working in the Keynesian and Austrian traditions as the main representative of a decision theory capable of dealing with true, genuine uncertainty (Lachmann 1976, and Davidson 1983), and his critical approach to mainstream economics summarised in 1972 in *Epistemics in Economics* still remains the main reference of the so-called radical subjectivist approach. The point is that from the early 1960s onwards Shackle's influence has been more relevant on methodological grounds than at the theoretical level; and his readers are mainly to be found among economists working in heterodox traditions rather than among mainstream decision theorists.

Remarkably, Shackle's analysis is mostly ignored in the debate on alternative theories of decision making originating with both Ellsberg's 1961 paper on observed violations of subjective expected utility and the related experimental findings of Kahneman and Tversky (1979) which took place in the 1980s and gave birth to the most recent developments (Kelsey and Quiggin 1992). Notable exceptions were Ellsberg's 1962 doctoral thesis (only recently published as Ellsberg 2001) and the works of authors more philosophically oriented like Levi (1972 and 1974) and Gardenfors and Sahlin (1982). On the contrary, in the years under scrutiny in this paper Shackle's decision theory was mostly discussed with reference to its technical content. The focus was on Shackle's proposal to find a measure to represent the subjective perception of uncertain consequences alternative to the probability measure of both the frequentists and the subjectivists.

This paper is meant to substantiate the view that Shackle was part of a lively but concentrated discussion on alternative decisional criteria which preceded the spreading influence of Savage's *Foundations* (Luce and Raiffa 1957, Ch. 13). This discussion is crucial to understand the multifarious developments observed in modern decision theory in the last twenty years or so, since most of the current developments are intended to account for the insights of authors emphasising the role of ambiguity and uncertainty in decision making. Apart from the experimental evidence related to the paradoxical results of Allais and

Ellsberg, there has always been a cognitive uneasiness with the Bayesian assumption that one can derive probabilistic beliefs over anything uncertainty, at least since both Keynes's (1921, p. 42) discussion of "non-numerical" probabilities and Knight's (1921, p. 20) reference to "true, unmeasurable uncertainty." This normatively motivated insistence on the distinction between risk and uncertainty, and the related search for a convincing representation of uncertainty, seemed to become irrelevant after Savage's axiomatisation of the subjectivist perspective of Ramsey and de Finetti, but constitutes the real core of much of the more recent developments in decision theory (Wakker 2006).

In this regard, Shackle, definitely on the losers' side in the late 1950s and 1960s, deserves closer examination. In order to illustrate Shackle's mis-appreciated role in this story we intend to provide an accurate analysis of the existing critical literature on Shackle and of primary sources alike.¹ Our claim is that Shackle's theory was a much more viable alternative to subjective expected utility than both its contemporary critics and modern decision theorists have recognised. This claim has been discussed from a theoretical viewpoint in Basili and Zappia (2008), where Shackle's non-probabilistic conceptualisation of individual decisions under uncertainty is compared with a most interesting recent approach to uncertainty, the so-called non-additive probability approach (Schmeidler 1989 and Gilboa and Schmeidler 1994). Basili and Zappia (2008) show in particular that Shackle's criterion for choice, dismissed at his time as devoid of any convincing rationale, is of the same kind of the criteria emerging from non-additive probability theory.

The paper is organised as follows. After a brief exposition of Shackle's motivation for studying decisions under uncertainty section 2 presents the essentials of his theory. The paper then devotes three sections to assess the reception of Shackle's 1949 volume as well as the ensuing discussion in the 1950s. Section 3 presents Kenneth Arrow's critical remarks and changing attitude towards Shackle. Arrow's role in our story is of the outmost importance, not only because he gave sizable importance to Shackle, but also because his changing attitude towards the relevance of Shackle's position is illustrative of the decay of interest in Shackle's approach in the late 1950s. In his comments on Shackle, moreover, Arrow

¹ We will make reference to the correspondence section of George Shackle's papers held in Cambridge University Library and catalogued as Add. MS 7669. Shackle divided his correspondence into two main categories: letters on *Expectation in Economics* and its sequels, which he called "ee letters," and letters on other topics of interest. The "ee letters" are mostly letters to Shackle, and only a few have responses from Shackle. But it can be argued that the essence of Shackle's counter-arguments to critics of the early version of his theory is to be found in his published writings, as Shackle himself stated in his preface to *Decision, Order and Human Affair*, a volume mostly devoted to rejoinders to commentators (Littlechild 2000).

defended the crucial point which is at the basis of the mainstream use of probability theory in decision making, namely, the denial of any behavioural distinction between risk and uncertainty, a point which turned out to be magnified by Savage's axiomatic approach.

Sections 4 and 5 consider the arguments of the other critics, distinguishing between those of the early critics (1949-1950) and those of scholars sympathetic towards Shackle's critique of the mainstream but objecting to his solution (1955-1959). The focus is on two main issues. First, the reactions to Shackle's proposal to abandon probability distributions as a meaningful representation of the individual's subjective perception of an uncertain environment are examined in detail. Secondly, emphasis is placed on the criticism addressed to Shackle's peculiar method of ordering uncertain consequences in order to understand why Shackle failed to convince his critics of the viability of his proposed alternative. A second important figure in the history of post-WWII theory of decision is examined in this context. Ward Edwards, psychologist and runner of the first experiments in decision making in the early 1950s, argued from the outset that experimental evidence challenged the hypothesis that decision-makers act as if they were endowed with an additive probability distribution (Edwards 1954).²

Section 6 summarises the state of the debate on uncertainty in the early 1960s, when it appears that Shackle's influence was to be confined to heterodoxy. Section 7 briefly recapitulates the main aspects that makes Shackle's place in the history of decision theory relevant. Section 8 offers a few concluding remarks.

Preliminarily, a main question underlying the debate is worth pointing out. The discussion between Shackle and his critics unfolded in the interlude between von Neumann and Morgenstern's enunciation of a set of axioms leading to the assumption of maximising expected utility – entailing that individuals are endowed with objective probabilities – and Savage's axiomatisation – entailing that individual subjective probabilities are elicited from choices. As a result Shackle did not deal with Savage's version of the probabilistic approach at the time that his own theory had its greatest impact. Moreover, even when he started making reference to the subjective approach Shackle did not provide an accurate assessment of the subjectivist “revolution” in decision theory, at least until the 1972 volume *Epistemics and Economics*. Basically, Shackle's point was that the same critical remarks he used to make against the use of probability distributions for crucial decisions could be levelled against

² In view of the fact that Edwards's experimental evidence constitutes the main starting point of Kahneman and Tversky's (1979) work on the use of decision weights instead of probabilities, it is of great interest that Edwards (1958) commented favourably on Shackle's search for an alternative representation of probabilistic weights.

Savage as well. On the other hand, the authors discussing Shackle's theory were well aware of the developments represented by Savage's *Foundations*; to some extent this awareness is recognizable even before Savage's volume provided consistent axiomatic foundations for the subjective approach.³ As will be shown, this asymmetry sometimes makes the debate between Shackle and his critics quite odd. In retrospect, this asymmetry also reveals that Shackle's enterprise partook of utopia. The way in which Shackle refused to attribute to subjective probability theory the status of a substantial step towards the explanation of decisions under uncertainty, and continued to defend his viewpoint as necessarily non-probabilistic, probably constitutes one of the clearest examples of a dissenting economist pursuing utopian ideas in the history of twentieth-century economics.

2. Shackle's non-probabilistic approach

George Shackle joined the London School of Economics as a graduate student in 1935 at the age of 32, after leaving his employment in a bank he had been enforced to assume because of the financial situation of his family. As most of the professors and colleagues he met at LSE like John Hicks, Abba Lerner, Friedrich von Hayek and Nicholas Kaldor among others, Shackle had a catholic interest in economics, an intuitive understanding of its subject matter and was determined to resist the trend towards a mathematical presentation of economic ideas (Ford 1993). Deeply influenced by the reading of Keynes's *A Treatise on Money* and Hayek's *Prices and Production* and the ensuing debate (Shackle 1983), Shackle started working on the Austrian theory of capital under Hayek's supervision. However he abandoned this topic shortly as he came to know the new Keynesian approach in the making attending Joan Robinson's and Richard Kahn's lectures on the forthcoming *General Theory* in Cambridge in 1935. Eventually he took his Ph. D. – still under Hayek – with a thesis on the theory of business cycle in which he used the Swedish *ex ante* and *ex post* notions to elucidate the dynamics of investment. In the thesis Shackle endorsed the Keynesian viewpoint that liquidity preference, not the Austrian reward for waiting, determines the rate of interest. In

³ It is worth recalling that Savage's paper "Notes on the Foundations of Statistics" circulated in the United States as background material for a course he gave in the winter of 1951 at the University of Chicago. A first summary of Savage's viewpoint appeared in the "report" section of *Econometrica*, as abstract of the paper presented at the annual meetings of the Econometric Society (Savage 1950). Savage's subjectivist solution to choice under uncertainty was considered common ground at the International Colloquium on the Foundations and Applications of the Theory of Risk held in May 1952 in Paris (Jallais and Pradier 2005).

1937 he moved to the Oxford Institute of Statistics as research assistant to Henry Phelps Brown, after rejecting a similar offer from Jacob Marschak, the then director of the institute, apparently because he thought his own knowledge of statistical methods was poor. During the war he was a member of the team of statisticians and economists gathered under Winston Churchill's scientific adviser Frederick Lindemann. After the war he was transferred to the Economic Section of the Cabinet Secretariat until Arthur Brown appointed him to a personal readership at the University of Leeds in 1950.⁴ The year after he was offered the Brunner Chair of Economic Science at the University of Liverpool which he occupied until retirement in 1969 (Ford 1993).

Shackle's doctoral thesis, published as *Expectations, Investment and Income* in 1938, was an attempt to clarify the intricacies of Keynes's writings on the volatility of investment and the expectations of investors, and to put them in use in an "expectational dynamics" theory of business cycle. Shackle was dissatisfied with Keynes's treatment of investment in real capital: in his view, after acknowledging the role of expectations and uncertainty Keynes substantially ignored it in the construction of a stable schedule of the marginal efficiency of capital for explaining aggregate investment. Shackle considered the role of expectations exemplified in Chapter 12 of the *General Theory* of crucial relevance, but only introductory to the exact explanation of the uncertainty surrounding investment decisions and, more generally, to the question as to how individuals deal with incomplete information in order to take action. In his view only in the QJE summation article Keynes (1937) tackled the issue of "true" uncertainty properly.⁵

From the early 1940s on Shackle restricted his attention to the analysis of individual investment decisions. In a series of papers he started elaborating his rejection of the "orthodox Benthamite solution" (Shackle 1940, p. 54) of assigning a numerical probability to each conceivable outcome and valuing alternative investment decisions in terms of their expected utility value. These papers, which prelude to the publication of his main work,

⁴ In his biography of Shackle, J. L. Ford (1993, p. 686) judges Shackle's time spent in government service as one in which "he had to carry out many duties which were rather mundane or which used virtually none of his economic understanding," although what he learnt about the conduct of economic policy during those years must have confirmed his views about the limitations of economics in the real world.

⁵ In his late studies on Keynesian economics Shackle (1976) clarified that he would have expected the author of the *Treatise on Probability* to propose a detailed answer to this question, but he did not find it in the *General Theory*. As contended by Ford "for Shackle it is only in his 1937 paper that Keynes recognised the true, will-o'-the-wisp, nature of the investment demand schedule" (Ford 1994, p. 433). Basili and Zappia (2007) argue that in its critique of frequency probability Keynes's *Treatise on Probability* contains most of the elements emphasised by Shackle's non-probabilistic approach and that Shackle's assessment of Keynes on this aspect is misplaced. See also Runde (2000).

Expectations in Economics, seem to be motivated exclusively by the aim to solve a theoretical problem yet to be solved. Shackle referred, first, to Hayek who had argued convincingly that economic theory had conceded no role for knowledge in the analysis of individual preferences, and, second, to Knight, Myrdal and Keynes who had shown the inability of economic theory to take account of “our *ignorance* about the relevant consequences of different alternative actions” (Shackle 1942, p. 77). From the analyses of these authors, Shackle continued, there emerges the theoretical question of how to represent the individual decision of an entrepreneur to invest in an uncertain environment, a question which none of them had discussed in detail. Shackle’s main intent was to show that this question cannot be dealt with in a probabilistic set-up.

Most economic decisions, Shackle argued (1949a, p. 6), are crucial, unique “experiments,” namely situations where “the person concerned cannot exclude from his mind the possibility that the very act of performing the experiment may destroy forever the circumstances in which it was performed.” The fact that these decisions are non-replicable precludes the possibility of applying probabilities, both numerical probabilities according to the frequency probability theory and, he argued later, subjective probabilities.

Shackle’s argument had what is usually identified as a clear “Knightian” flavour, although we have just seen that it mostly derives from his peculiar reading of Keynes. The use of probability calculus to analyse decisions under uncertainty was inappropriate, in his view, simply because the conditions required for its application do not exist in many relevant economic contexts; a similar stance underpins Knight’s distinction between risk, or “measurable uncertainty,” and proper uncertainty, or “unmeasurable uncertainty,” a distinction which has become commonplace in the modern literature on risk and uncertainty (Wakker 2006).⁶ Shackle’s contention was that in reality individuals do not have a complete knowledge of the structure of the world – a knowledge that the construction of probability calculus assumes. Individual choices are made between alternatives which are subjective representations of alternative future sequels to actions, and not between future sequels themselves. In Shackle’s words, “choice is among imagined experiences”, a view which

⁶ Alternatively, Knight’s distinction has been interpreted as referring to insurable risk versus uninsurable risk. As the main theoretical aspect underlying this paper is whether a probabilistic representation of uncertainty can be given or not, the Knightian definition is assumed here to identify risk as a situation in which there exists an objective additive probability function on the state space, while uncertainty as a situation in which there is no, either because the state space is not accurately defined (“true” uncertainty) or because the probability function is not considered reliable by the decision maker (ambiguity).

implies that the individual is not given an exhaustive list of the alternatives between which choice should be made.⁷

In particular, Shackle argued that individuals are not capable of enumerating all possible contingencies, or states of the world. In a sense, the individual agent is able to take decisions only insofar as she creates her own choice set, but this necessarily implies that the choice set thus created is non-closed. This is the main analytical point at the basis of Shackle's theory.⁸ On this basis, Shackle developed a formal theory intended to capture both the mental processes and the non-repetitive, and often irreversible, nature of actual economic decisions. Shackle's argument was initially intended to oppose the objective frequency-ratio interpretation of probability, which he regarded as the mainstream view in the late 1940s. In 1961, however, he maintained that the same argument could apply to the subjective interpretation of probability (Shackle 1961). The fact that Savage's subjectivist revolution did not induce Shackle to re-elaborate his objection to the applicability of probability calculus is crucial for our reconstruction. This aspect of his position will be taken up in the following sections.

To dispense with probability, Shackle put forward the concept of potential surprise.⁹ First, he distinguished between distributional uncertainty variables, which can be used if "the list [of suggested answers to a question] is *complete without a residual hypothesis*," and non-distributional uncertainty variables, which must be used when "the list in order to attain formal completeness must be rounded off with a residual hypothesis" (Shackle 1961: 49-50). In order to describe the "mental state of uncertainty" of the decision-maker, Shackle maintained, "the inclusion of a residual hypothesis in his list of suggested answers is his acknowledgement that he has no basis for considering his existing list of particularised hypotheses to be comprehensive."

⁷ As emphasised by Ford (1993) an outgrowth of this viewpoint is Shackle's opinion that the interpretation of time germane to economics was inside, subjective time instead of outside mechanical time. Late in the 1950s time emerges as the second main theme in Shackle's economics (Shackle 1958a and 1972). This led him to embrace a philosophy of time and ignorance alien to the line of development of decision theory in the last fifty years (Ford 1994, pp. 476-480). This aspect of Shackle's theory will not be discussed here as it concerns a span of time which is not covered in this paper.

⁸ To economists working in heterodox traditions of thought, like the post Keynesians and the Austrians, this point has become an indispensable analytical reference in their effort to represent decisions under genuine uncertainty (for instance, see Davidson 1983). In particular, this is the crucial argument upon which the distinction between "rational ignorance" and "radical ignorance" has been drawn in the Austrian tradition (Langlois 1994 and Vaughn 1994). By this distinction, the Austrians intend to distinguish Savage's approach from the Knightian tradition. On this point see Zappia (1998).

⁹ The remaining of this section draws on Basili and Zappia (2008).

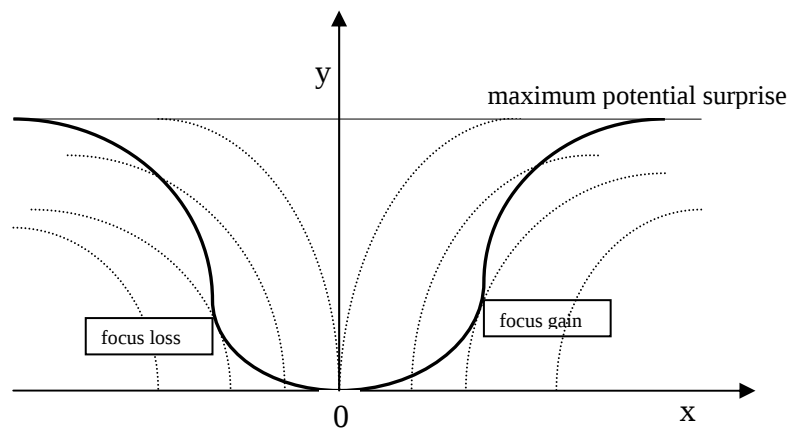
As a result, what was needed was “a measure of acceptance, of a hypothesis proposed in answer to some question, that shall be independent of the degrees of acceptance simultaneously accorded to rival hypothesis;” that is, Shackle’s framework required “a measure of acceptance by which the individual can give to new rival hypotheses, which did not at first occur to him, some degree, and even the highest degree, of acceptance without reducing the degrees of acceptance accorded to any of those already present in his mind” (Shackle 1949-50: 70). This measure of acceptance amounts to Shackle’s substitute for probability distributions, to be used under uncertainty. It is worth noting from the outset that Shackle does not reject the idea that subjective weights are attached to possible outcomes, but the idea that these subjective weights conform to the rule of probability when the possible events cannot be anticipated exhaustively, that is when a non-distributional variable is involved. As a result, the distinction between distributional and non-distributional variables shows that Shackle’s theory is essentially non-additive: subjective weights do not necessarily add up to 1. It will be argued below that Shackle showed clear awareness of this characteristic in replying to some critics of his 1949 volume *Expectation in Economics*.

Shackle’s next step was to apply this (non-additive) measure. He analyzed a decision-maker, typically an entrepreneur, who had to choose among alternative “sequels” to actions on the basis of two elements: the possible gains and losses embedded in a sequel, x , called face-values, and a valuation of the “possibility” of these gains and losses, $y(x)$, called potential surprise. The latter element can be considered as a degree of disbelief, or implausibility of the hypothesis that supports the sequel; it ranges from 0 (absence of disbelief or zero potential surprise) to a maximum value expressing impossibility (absolute disbelief or maximum potential surprise). When the decision maker chooses among alternative sequels, she re-considers the face-values of each sequel by their degree of potential surprise.¹⁰

Finally, Shackle defined a function $\varphi(x, y(x))$, called “degree of stimulus” (or “ascendancy function”). The face-values and the associated potential surprises implicit in a

¹⁰ The aim of measuring the degree of belief in a certain event by means of its opposite, the degree of disbelief, or potential surprise, is instrumental to the construction of a non-additive index. The emergence of a new unanticipated event does not necessarily reduce the degree of disbelief previously assigned to other events, as it would be if (the opposite of) this degree is measured by a probability. “By disbelief I do not now mean the absence of perfect certainty, but the positive recognition of some disabling circumstances ... and there is, in general, no limit to the number of mutually exclusive hypotheses to all of which simultaneously a person can, without logical contradiction, attach zero potential surprise” (Shackle 1952, pp. 30-31).

sequel are the arguments of this function. In Shackle's view, the higher is the prospective gain (loss) on an investment the higher is its power to attract the attention of the entrepreneur. Moreover as the potential surprise attached to a gain (loss) is increased, that gain (loss) loses its ascendancy in the mind of the entrepreneur. On the hypothesis that the function φ is continuous, each degree of stimulus generates an indifference curve in (x,y) plane, that is, the gain (loss) / possibility plane ranking outcomes. On this ranking of outcomes, Shackle superimposed the particular potential surprise curve which the decision maker assigns to some particular project in order to select the values (that is, the gain/loss pair) that maximizes the ascendancy with respect to that project, as represented in the following diagram in which the dotted lines represent the level curves of the ascendancy function $\varphi(y(x),x)$ and the bold line represents the potential surprise function $y(x)$.



Following this procedure Shackle (1953, p. 46) determined “the highest bids which the particular project in question can make for the decision maker’s attention and interest. One of these bids is the most powerful suggestion of success and the other the most powerful suggestion of disaster that the conception of project conveys.” In other words, these extreme values represent the limits of all possible outcomes of any feasible sequel, after the outcomes and their potential surprise are valued in terms of the attitude the decision-maker shows towards the uncertain situation. The criterion for choice Shackle proposed amounts then to a rule of thumb by which the decision maker takes into account both “the best possible” and the “worst possible” outcomes of each sequel, respectively called “focus-gain” and “focus-loss.” As Shackle (1953: 47) summarised: “because the project is a non-divisible non-seriable experiment, his [the entrepreneur] various hypotheses as to its outcome are mutually

exclusive and therefore there is here no logical basis for the additive procedure by which a 'mathematical expectation' is assigned to a divisible experiment."

This way of formulating a criterion for decisions can be phrased in the language of modern decision theory. Shackle's decision-maker first orders prospect revenues of an act (sequel) on the basis of both their value and reliability, and then she re-evaluates them by her attitude towards the uncertain environment. At this point, she takes into account the best and the worst outcomes involved in the feasible act, instead of calculating an expected value of the act. Eventually acts are ranked in terms of the best/worst pairs, and the decision set-up is represented by an indifference map, which Shackle called the "gambler indifference map." This map is almost analogous in shape to the indifference map of a consumer who consumes a bundle including one bad (the worst outcome, measured on the horizontal axis) and one good (the best outcome, measured on the vertical axis) (Shackle 1953, p. 48).

Shackle was well aware that he was proposing a heresy. "Those accustomed to think in terms of the actuarial calculation of the result of a divisible experiment," Shackle (1953: 42-43) admitted, "will find this point exceedingly hard to appreciate." His argument was at times extremely naïve: "Suppose, for the sake of the argument, that the decision-maker does fix his attention momentarily on one of the interior hypotheses, the idea of a small gain, by hypothesis, is perfectly easy to imagine as the outcome of the project. But his attention will not rest on that hypothesis. His mind will be immediately challenged by the thought that much larger gains are equally easy to imagine: why then stop at the relatively small one?" Moreover, his argument is weakened by the fact that he did not discuss at sufficient length the cognitive bases of the advocated procedure of selecting only two values for each sequel of action.

Nonetheless Shackle was clear that he was suggesting a procedure alternative to von Neumann and Morgenstern's maximin criterion, which Wald (1950) had transformed into a criterion for decisions under complete uncertainty. Shackle (1953: 43) first observed that "the fundamental hypothesis of von Neumann and Morgenstern's *Theory of Games* is that a decision-maker will choose that action (strategy) whose worst possible outcome is the least bad amongst the respective worst possible outcomes of all the actions open to him." Then he added: when players do not have the "comprehensive, exact and certain knowledge" supposed by von Neumann and Morgenstern's setup, "would the von Neumann and Morgenstern hypothesis be more plausible than that the decision-maker will choose that action whose best possible outcome is the best amongst the respective best possible outcomes of all the actions open to him?" A maximax criterion, Shackle maintained, should

be considered equally plausible at least. But, he concluded, “there is surely a third [criterion] which is more plausible, and of more general analytical power, than either of the two former, namely, that he [the decision-maker] will take into account both the ‘best possible’ and the ‘worst possible’ outcome of each course of action and make these *pairs* of outcomes the basis of his decision.” In what follows, this third criterion will be referred to as Shackle’s criterion.

Before moving on a few consideration on Shackle’s way to present his viewpoint are worth mentioning. Shackle’s model has been substantially elaborated in the 1940s and presented in an all-comprehensive form for the first time in the 1949 volume. In the 1950s Shackle published a second edition of the 1949 volume and a series of articles, both intended to clarify the details of the theory and to answer to critics. Most of these papers, were intended also to illustrate the domain of application of the theory, for instance to interest rates, portfolio choice and investment decisions. However, as best illustrated by his 1961 volume and noted also by the more benevolent critics (see below section 5), there is in Shackle a tendency to answer critical remarks simply by re-phrasing his main point, a fact which contributed to insulate him from the developments of decision theory in those years. Moreover after the articles of the early 1940s Shackle’s reference to the work of other authors is sparse and almost exclusively relative to those who had commented his works. For instance there is no reference to authors discussing alternative probabilistic set-up and choice under ignorance like the Keynes of the *Treatise on Probability*, B. O. Koopman, Irwin Good, and Lionell Hurwicz. Most of all, Shackle never commented explicitly on Savage’s axiomatisation of decisions under uncertainty: it comes as no surprise then that his theory is no longer referred to in the theoretical discussion from a certain date on.

3. Arrow on Shackle

To assess Shackle’s role in the development of decision theory in the late 1940s and early 1950s it is appropriate to begin with Arrow’s 1951 article on alternative approaches to the theory of choice in “risk-taking” situations. In fact, this article was probably the most referred survey on decision making of the 1950s, at least until there appeared Luce and Raiffa’s chapter on individual decision making under uncertainty in their celebrated volume on game theory (Luce and Raiffa, 1957). Furthermore, as Arrow made reference to Shackle in two other articles of his on this topic (Arrow 1959, 1972), his changing attitude will be examined before discussing the other contemporary reactions to Shackle, and taken as

evidence for our thesis that Shackle's inability to take into account the new subjectivist approach to probability contribute much to the oblivion in which his theory ended up.

As noted in the introduction, Arrow (1951, p. 404) gave sizeable relevance to Shackle's analysis because it was the single formalised theory among those put forward by a group of authors who, following in Knight and Keynes's footsteps, "do use other than probability statements in their description of behaviour under uncertainty." Arrow's comments were mostly dismissive. Arrow first discussed Shackle's view on the feasibility of a representation of uncertainty alternative to probability, and then his criterion for the ordering of alternative situations. As for the first issue, Arrow (1951, pp. 419-420) recalled that Shackle's theory "rejects the degree-of-belief theory of probability, since a distribution can never be verified even *ex post*, while the frequency theory is inapplicable where definite repetition is impossible." Then he presented Shackle's potential surprise function and discussed the rules combining potential surprises. Arrow observed that the rules proposed by Shackle were such that "there is no law of large numbers or elimination of risk by consolidation of independent events, for the potential surprise function attached to a sequence of unfavourable outcomes is as large as to any one." As a result "no amount of repetition in independent trials would lead to reduction of risks, whereas even Shackle concedes that in long runs probability rules would be applicable." However, Shackle's theory "does not lead, even in the limit, to the probability theory." Therefore Arrow's first conclusion was that "the elimination of all probability elements from Shackle's theory cannot be regarded as satisfactory."¹¹

As for the second question, Arrow (1951, pp. 432-433) summarised how Shackle had identified pairs of gains and losses and how he had elaborated an ordering among pairs, represented by the gambler indifference map. The fact that only two possible outcomes are singled out is commented as follows: "Shackle has the sound impulse to base a theory of

¹¹ There is a subtle interpretative question here which needs emphasising. Since Shackle's concept of potential surprise provides a formulation of decision weights alternative to those of an additive probability distribution there is the need to define different combination rules concerning these weights. In an attempt to give an axiomatic basis to his theory, Shackle (1949a, pp. 128-132) devised rules for combining potential surprises, intended to be analogous to those of probability theory. In particular he assumed that the probability of the union of two mutually exclusive events is equal to the smaller of the probabilities of the single events, and the probability of the intersection of two events is equal to the larger of the probabilities of the single events. Arrow's critique is addressed to these combination rules. Years later Levi (1972) summarised these rules as being appropriate to a measure that avoids the deficiencies of probability, while Shafer (1976) noted that these properties pertain to a non-additive probability function. These properties have been extensively investigated in, and indeed they constitute the foundation of modern possibility theory (Dubois and Prade 1988).

uncertainty-bearing on the necessity of the human mind to simplify a problem in order to be able to deal with it. However, his particular simplification seems to be purely arbitrary.” Hence Arrow (1951, p. 433) concluded: “this theory is not based on consideration of rational behaviour, which Shackle specifically rejects, but on an alleged inability of the mind to consider simultaneously mutually exclusive events.”

A further, important comment is contained in the 1951 article. Among other theories, Arrow discussed those that seek to relate empirical observations to probability judgments via the law of large numbers. To criticise these theories, Arrow approvingly reported Knight’s and Shackle’s point that if individuals do not have the possibility to repeat an experiment indefinitely, probabilities cannot be interpreted as long-run frequencies ratio, and hence are irrelevant for their conduct. He concludes: “this argument would obviously have no validity in the degree-of-belief theory of probability” (Arrow 1951, p. 415).¹² Savage’s *Foundations* were still to come, but Savage’s influence is apparent. Arrow (1951, p. 414) made clear that by degree-of-belief probability theory he meant a theory in which the “fundamental description of uncertainty is couched in terms involving other than probability statements, but certain postulates are placed on the behaviour of individuals under those conditions, and it is shown that they then act as if there were an a priori distribution of probabilities.” Arrow illustrated this procedure with reference to Ramsey and de Finetti’s procedure of eliciting probabilities from actions. An abstract of Savage’s paper presented at the 1950 meeting of the Econometric Society (Savage 1950) was mentioned to indicate that an axiomatisation of a subjective theory of probability built on Ramsey and de Finetti’s lines of was in the pipeline. Further, Arrow cited a 1949 working paper by Chernoff (to be published as Chernoff 1954), where, first, a number of “reasonable” basic properties that decision criteria should fulfil were stated, and, second, alternative criteria were discussed. Chernoff’s procedure was investigated in two working papers by Hurwicz (1951a) and Arrow himself (1953), and it was widely used in those years, before the advent of Savage’s approach.¹³

Made aware of this in a personal communication from Didier Dubois in the early 1980s, Shackle reacted enthusiastically (Shackle 1986, p. 62)

¹² Arrow’s evaluation of Knight’s theory matched his evaluation of Shackle’s. After reconstructing Knight’s viewpoint he commented: “Knight’s uncertainties seem to have surprisingly many of the properties of ordinary probabilities, and it is not clear how much is gained by the distinction” (Arrow 1951, p. 417).

¹³ See in particular Milnor (1954) and Luce and Raiffa (1957, chapter 13). Both Arrow and Hurwicz did not publish their results until Arrow and Hurwicz 1972, though the so-called Hurwicz’s optimal criterion for decision making under uncertainty was referred to as a suitable criterion since the early 1950s, as it was presented at the 1950 annual meeting of the Econometric Society (Hurwicz 1951b).

Arrow made reference to Shackle at least in two other articles. Both are contributions to volumes discussing Shackle's theory, presumably invited contributions. The first paper appeared in the monographic 1959 issue of *Metroeconomica* devoted to Shackle's theory ten years after the publication of *Expectations and Economics*. The paper contains only a brief reference to Shackle's work, which in the meantime had become, in Arrow's view, a negligible alternative to the consolidated mainstream. Arrow (1959, p. 13) depicted the state of the art in decision theory at the end of the 1950s as dominated by a basic structure "accepted by virtually all participants in the discussion." This basic structure is summarised through, first, a function which relates consequences to actions and states of nature, and, second, a criterion to select an action from a given opportunity set of actions. The crucial component of this basic structure, which explains how the various theories differ, is "a *weighting* of the different states of nature." "Most theories", Arrow (1959, p. 14) stated, "tend to include a range of weights for different states. If these weights satisfy certain formal rules of combination, they are referred to as *probabilities*." And then he offered his only comment on Shackle: "Shackle's concept of potential surprise is an alternative formulation of weights, with different combination rules." It is worth noting that, though apparently dismissive of Shackle, this suggestion reveals that Arrow correctly understood the point made by the early critics of the subjectivist approach like Edwards (1954) – later made explicit by Kahneman and Tversky (1979) – that the epistemic state of the decision-maker could be characterised by measures alternative to additive probability. But Shackle did not discuss Arrow's point.¹⁴

Arrow also mentions Shackle's theory in a joint paper with Hurwicz, contributed to a 1972 volume in honour of Shackle. Arrow and Hurwicz (1972, p. 1) provided "a possible characterisation of the concept of complete ignorance," defined as "a situation in which there is no a priori information available which gives any state of nature a distinguished position", and as a situation "not presupposing a fixed list of states of nature." They clarified from the outset that under these hypotheses the criterion they provided differed from those formulated within the "now more standard subjective probability framework," and that their arguments and conclusions "are much closer to Shackle's (1949) than those of Ramsey (1926), de Finetti (1937), and Savage (1954)." In situations of complete ignorance, Arrow and Hurwicz (1972, p. 2) argued, the subjective probability framework can only assign equal probabilities to all the states of nature, in accordance with Laplace's principle of insufficient

¹⁴ It is worth noting that Arrow's article is the only one among the articles published in the *Metroeconomica* 1959 issue not quoted in the bibliography of Shackle 1961.

reason. Shackle's formulation, on the other hand, permits to interpret complete ignorance as meaning that all states of nature have zero potential surprise. As a result, if a description of the world needs making finer than previously anticipated (a likely situation if complete ignorance is assumed), this can be done easily in Shackle's but not in the subjectivists' framework.¹⁵ As seen in the previous section, and as Shackle argued many times in response to his critics (see next section), this capability of accounting for new, unanticipated events is precisely one of the reasons for the use of the potential surprise function.

Arrow and Hurwicz then proceeded along the lines of Chernoff (1954) and Milnor (1954). First, they postulated a set of "desirable" properties of a rational criterion of choice. These are mostly properties regarding the relationships between sets of actions, identified as decision problems; these sets permit the identification of the minimum and the maximum value associated with each decision problem under any particular state of nature. Second, they proved that the necessary and sufficient conditions guaranteeing that an optimality criterion possesses the postulated properties are related only to the minimum and the maximum value. That is, any criterion consistent with those desirable properties ranks actions solely on the basis of their best and worst possible outcomes, and not on the basis of averages. Finally, Arrow and Hurwicz (1972: 2) commented on Shackle's theory that if the focus values of Shackle are interpreted as the minimum and maximum payoff to a given action, and the final decision among possible actions is made on the basis of the gambler indifference map, "we demonstrate that a plausible set of desirable properties for a rational criterion of choice under complete ignorance in fact leads to this special case of Shackle's theory."

The conclusion Arrow and Hurwicz reached is nothing short of surprising. As documented above, Arrow's previous analysis of Shackle's theory (1951) had been detailed but dismissive, in particular as regards the selection of the focus outcomes, termed "purely arbitrary." Furthermore the 1959 paper mentioned Shackle only as a minor detour from the main developments. In 1972, on the contrary, the reader is told that, considering that there are situations in which there is no reason to suppose that the decision maker has a reliable subjective probability distribution over possible future events, she would better follow Shackle's procedure of ranking actions in terms of only two values. The opening footnote of

¹⁵ In the subjectivist approach if the event on which choices are conditioned is not in the list of the possible states the decision maker is forced to modify the probability weights originally attributed to the states, because the weights add up to 1. As noted in the previous section, Shackle insisted that a correct representation of the epistemic state of an individual should make room for a "residual hypothesis" without reducing the weights attributed to the original list of states. However, this can be achieved also in a probabilistic approach, if the probability measure on the original list of states is subadditive.

the 1972 paper mentioning the dates at which Arrow and Hurwicz obtained their results adds to the surprise. Arrow and Hurwicz (1972, p. 1 f.) acknowledged that Hurwicz had attained the basic result of the essay in the 1951 working paper quoted above, while Arrow's contribution (circulated in the 1953 working paper quoted above) was said to consist in a considerably simplified proof of Hurwicz's result. The 1972 version, Arrow and Hurwicz add, did not represent a major development over the working papers of the 1950s. The point to stress is that Arrow and Hurwicz recognised that Shackle's criterion could be interpreted as a version of Hurwicz's criterion only in 1972, while the main result was proved in 1953.¹⁶

It is worth observing that Wald's maximin is a special case of Hurwicz's criterion. Wald (1950) discussed the decision problem as a two-person zero-sum game where the decision maker plays against Nature. The maximin strategy was shown by Wald to be a best response against Nature's minimax strategy, that is, against the least favourable a priori distribution Nature can employ. Wald's criterion is extremely conservative in a context of complete ignorance, where conservatism may make good sense.¹⁷ Hurwicz (1951) introduced a parameter intended to obviate the conservative (or pessimistic) procedure of concentrating only on states having the worst consequences. Hurwicz's criterion selects the minimum (m) and the maximum (M) payoff to each given action and then associates the index $\alpha M + (1-\alpha)m$ to each action. Of any two actions, the one with the maximum index would be preferred. If $\alpha=0$ Hurwicz's criterion is the maximin criterion, while if $\alpha=1$ it is the maximax criterion. After labelling Wald's maximin criterion as "ultraconservative," Luce and Raiffa (1957, p. 282) presented the rationale of Hurwicz's amendment in the following way: "Why not look at the best state, or at a weighted combination of the best and worst?" The resemblance with Shackle's apparently odd statement on von Neumann-Morgenstern's criterion (quoted in the previous section) is striking. Indeed Shackle's criterion is but a generalised version of Hurwicz.

It is unfortunate that Ellsberg's 1962 doctoral thesis has remained unnoticed up until very recently since Ellsberg made this point clearly. In the thesis the use of decision rules based on the best/worst pairs is justified by Ellsberg because of its normative appropriateness: he referred to Arrow's 1953 and Hurwicz's 1951 working papers and

¹⁶ An analogous understanding of the relationships between Shackle's and Hurwicz's criterion can be found in Ellsberg doctoral thesis, submitted to the Economics Department at Harvard University in 1962, but published only as late as 2001 and remained totally unnoted until then (Ellsberg 2001, p. 163).

¹⁷ It is worth noting that the literature on environmental problems, where decisions are typically irreversible and potentially high losses related to catastrophic events are to be considered, makes explicit reference to Wald-related criteria and to Shackle's focus-loss (for instance, see Perrings 2003).

remarked that “any criterion satisfying a certain subset of these requirements takes into account *only the minimum and maximum utility associated with each act.*” Hurwicz’s conditions for characterising an optimal criterion under conditions of complete ignorance, Ellsberg (2001, p. 190) noted, “serve also to ‘characterise’ Shackle’s recommendations ... [since] they provide a logical basis for his emphasis upon ‘focus outcomes’.”

It is appropriate to outline the importance of the Arrow-Hurwicz criterion for modern decision theory. The Arrow-Hurwicz criterion has originated a literature on decision making under complete ignorance (for instance, see Kelsey 1993). Savage’s subjective expected utility theory applies to situations in which the individual acts as if he had a single reliable probability distribution over the states of the world (elicited from choices). Conversely, Arrow and Hurwicz introduced the case in which the individual has no idea at all about the likelihood of states. Gardenfors and Sahlin (1982) proposed an alternative to subjective expected utility to encompass the case between these two extremes, that is the case in which the individual has opinions about the likelihood of different states, but she is not able to assign exact probabilities to them. According to their theory the decision maker has a convex set of subjective probability, which expresses the range of probabilities she considers possible. Since the subjective probability is not unique there is a set of expected utilities for each action. In this context an action a is preferred to b if and only if the minimum possible value of the expected utility of a is greater than the minimum expected value of b . Gardenfors and Sahlin called this criterion maximin expected utility (MEU).¹⁸

More recently Gilboa and Schmeidler (1994) have shown that there is a close relationship between maximin expected utility and non-additive probability theory which needs emphasising. They showed that there is an isomorphism between the representation of decision makers’ beliefs through a non-additive probability measure and a convex set of additive probability measures. Schmeidler (1989) proved that a non-additive probability representing the belief of a decision-maker averse to uncertainty can be integrated by means of a Choquet (1954) integral to generate the “expected value” with respect to the non-additive prior. But if the non-additive prior has a non-empty core of additive measures, then the Choquet Expected Utility (CEU) with respect to it equals the MEU with respect to the set of additive measures. That is, the Choquet integral of the utility function with respect to a

¹⁸ If the set of probabilities consists only of a single probability distribution, maximin expected utility coincides with subjective expected utility. If, on the other hand, it consists of all possible probability distributions it coincides with Wald’s maximin. Due to both its appealing cognitive justification in the rejection of the Bayesian approach and the axiomatisation provided by Gilboa and Schmeidler (1989) MEU is possibly the most referred criterion for choice in the literature on complete ignorance.

non-additive prior μ equals the minimum of all its integrals with respect to the additive priors taken from the core of μ . As a result, under uncertainty aversion there is no difference between the two approaches.

The interest in non-additive measure increased after Ellsberg's findings (1961), because decision-makers are shown to exhibit a pessimistic attitude towards uncertain situations and this can be rationalised through the use of a sub-additive probability measure.¹⁹ As a result, in the search for decisional criteria to be used in Ellsberg's type situations the criteria in the Arrow-Hurwicz tradition are suitable candidates. The concluding section will show why this reconstruction of the literature originated by Arrow and Hurwicz's 1972 paper corroborates the thesis of Basili and Zappia (2008).

4. Shackle and his critics: late 1940s and early 1950s

In this section we assess the discussion between Shackle and his critics on the non-probabilistic approach developed in *Expectations and Economics*. The discussion centred around the two main points which emerged from Arrow's 1951 review. As seen in the previous section, Arrow's first argument was that the subjective probability approach (in the making in the early 1950s) was a sufficient response to Knight's contention that uncertainty is probabilistically unmeasurable. Arrow's view that it is legitimate to represent individuals' beliefs by a single additive probability measure in every uncertain situations set the standard, and became the mainstream position even before the axiomatisation provided by Savage. Hence the origin of the orthodox position in modern decision theory should probably be found in Arrow's article rather than in Savage's volume. Arrow's second point was that Shackle's criteria for ordering actions had no theoretical foundation. As shown,

¹⁹ As is well known, in Ellsberg's hypothetical experiment individuals are presented with 2 urns containing white (red in the original 1961 article) and black balls. Urn II contains 50 white and 50 black balls, while Urn I contains 100 white and black balls in an unknown ratio. Individuals are asked to choose an urn to draw from, and bet on the color that will be drawn, receiving a \$100 payoff if that color is drawn, and \$0 if the other color is drawn. They usually are indifferent to bet on (i) a white draw from Urn I, or a black draw from Urn I, and on (ii) a white draw from Urn II, or a black draw from Urn II, that is, their subjective probabilities are such that $p(w_I)=p(b_I)=1/2$ and $p(w_{II})=p(b_{II})=1/2$. However when asked to bet on (iii) a white draw from Urn I, or a white draw from Urn II, and on (iv) a black draw from Urn I, or a black draw from Urn II individuals usually prefer to bet on white from Urn II and on black from Urn II, that is $p(w_{II})>p(w_I)$ and $p(b_{II})>p(b_I)$. As a result their subjective probabilities violates both completeness and monotonicity axioms (Ellsberg 1961, p. 651 f.). The non-additive approach proposes to represent the subjective probabilities as $p(w_I)=p(b_I)=k<1/2$, that is, $p(w_I)+p(b_I)<p(w_I \cup b_I)$. Following Ellsberg, the individual who prefers to bet on Urn II is said to show ambiguity aversion with respect to Urn I (Schmeidler 1989, p. 573).

Arrow changed his mind on this issue, but his recantation came too late to rescue Shackle's view from oblivion.

The material of this section is the comments and rejoinders that were published in major journals during the 1950s as well as a few volumes dealing with Shackle's theory. Shackle's correspondence included in the *Shackle's Papers* at Cambridge University Library is also used. Our aim is to show that certain critics correctly understood two aspects of the utmost importance of Shackle's theory: the implications of the non-additive character of the theory, and the possibility of interpreting Shackle's ascendancy functions as a specific distortion of the weighting function of the decision maker. It will be argued that Shackle's inability to fully appreciate the arguments of those critics is a major reason explaining why decision theorists gave up discussing his theory after the 1950s. Furthermore it will be highlighted the influence on discussion of Shackle's refusal to take into consideration Savage's new interpretation of probability theory. In particular, the appeal of Shackle's insistence that probability was not apt to be applied to unique decisions diminished considerably among commentators in the late 1950s and early 1960s.

It is worth recalling before proceeding that Shackle's theory was not only widely discussed in the 1950s, but also followed by a few contributions intended to develop it. There were contributions devoted to applications to the theory of profit (Keirstead 1953) and to multi-period investment decisions (Egerton 1955 and 1960). The influence of Shackle's ideas was explicitly acknowledged in Lachmann (1956), Fossati (1957) and Niehans (1959), among others. The relevance for a theory of expectations was discussed in Carter (1953), Gorman (1957), Gould (1957) and Foldes (1958). Even authors sceptical about the internal consistency of the theory referred to it, in the same vein of Arrow, as the only formalised alternative to probability theory in the early 1950s (for instance, see Hahn 1955-6). Moreover, in the hope of creating consensus around his theory, Shackle himself attempted to provide applications of the theory to the analysis of liquidity preference and demand for assets (Shackle 1958b) and to the theory of interest rates (Shackle 1961, chapter 27).

Immediately after the publication of *Expectations and Economics* Shackle was absorbed in two main tasks. On the one hand he answered reviewers, and on the other tried to elucidate the crucial elements of his theory. Before Arrow's 1951 survey reviewers of Shackle's volume generally showed great interest and admiration, with specific regard to Shackle's aim to provide a formalised alternative to the frequency probability tradition, clearly inadequate to deal with Knightian uncertainty. Two short notes appeared in *Economica* and were followed by Shackle's rejoinder. The note by Graaff and Baumol is

central because it set the tone of Arrow's and other mainstream decision theorists' position. Graaff and Baumol (1949: 338-39) expressed very well the excitement of the economics profession confronted with the "quite devastating criticism of the orthodox probability approach to expectations to be found in most theoretical discussions by professional economists." They also wrote (1949: 341) that the idea of potential surprise "represents a step towards what one might perhaps call a truly *subjective* theory of confidence." Graaff and Baumol viewed favourably the possibility that "the development of an essentially subjective theory of expectation, and especially of the confidence with which these expectations are held" had a revolutionary impact, similar to that which "occurred in the central body of theoretical economics when the subjective theory of value was developed."

Graaff and Baumol then moved on to criticise the details of Shackle's proposal, in particular his exclusive concern with the focus-gain and the focus-loss of each actions. As later agreed by Arrow, they contended that "it is not altogether clear whether Mr. Shackle presents the hypothesis that people concentrate on the focus-outcomes as no more than a useful simplification of reality, of the kind we are continually looking for in our research, or whether he means it to be taken quite literally." This aspect was considered of the utmost importance because "it is upon the empirical premises and not on any assertion of rationality that Mr. Shackle's analysis is based." Graaff and Baumol elaborated a simple example in which it was obvious to expect that the decision-maker would consider relevant not only the focus values but also the whole range of possible outcomes. Moreover they were unsatisfied with Shackle's suggestion that "in a one-and-for-all decision one can afterwards derive cold comfort from the knowledge that there were alternative gains and losses conceivable." Thus they concluded: "one cannot help feeling that it is somewhat rational to choose the alternative with the greater actuarial value even where, because the experiment is unique, the actuarial calculation cannot be taken literally." (Graaff and Baumol 1949: 342).

In his reply Shackle (1949b: 343) persisted in his initial position that any crucial decision – that is, a decision whose outcome "will affect the whole future course of relevant events for the individual" – is a "unique trial."²⁰ Any crucial decision is "necessarily unique" because "by making it the individual gives himself a new set of circumstances and opportunities, so that it is logically impossible for him ever to repeat the experiment which

²⁰ By unique trial Shackle (1949b: 343) meant "one which is unique in its effect on the life of the individual who makes it, that is, a trial whose character and circumstances are not sufficiently like those of other trials which he expects to make for him to treat all these trials as parts of a larger whole, with whose outcome *as a whole*, and not with those of the separate trials composing it, he is really concerned."

brought this new situation about.” Even the toss of a coin, Shackle argued, can be considered a unique trial if “it is for choice of ... innings in a Test Match.”²¹ However he could not offer a reason for the procedure of selecting two values, except for a critique of the widespread practice to apply probabilities even to single trials: “What is the source of my critics’ feeling that other elements besides the two ‘most powerful’ are taken into account of in the individual’s final decision? I think it is that they are still unconsciously thinking in terms of numerical probability and of repeated trials forming parts of a larger whole which alone is significant” (Shackle 1949b: 345).

The exchange with Graaff and Baumol set the tone of the whole debate. In private correspondence Houthakker argued: “Although I have sincere admiration for the originality and lucidity of your exposition ... I am not quite convinced by your attempt to replace the approach based on probability considerations. I think that your objections to the latter are too exclusively directed against the frequency theory of probability, which is by no means generally accepted. ... Despite your remarks in *Economica* ... I fail to see any *logical* reason why expectations which cause greater or smaller potential surprise than the ‘focus values’ should not be of importance in arriving at a decision.” He then moved on to show why, in his view, the axioms of *Expectations and Economics* failed to justify the use of potential surprise.²²

The other note published in the 1949 issue of *Economica*, by Ralph Turvey, addressed the question of the differences between Shackle’s approach and “the orthodox analysis of uncertainty.” Turvey (1949: 337-338) distinguished between the use of the frequency probability distribution, apt to deal with Knight’s risk, the use of the subjective probability distribution, apt to deal with Knight’s uncertainty, and the use of the potential surprise function. He correctly argued that, although at first sight the latter seemed to be merely an inverted subjective probability function, this was not the case because “while for any value of x maximum potential surprise correspond to zero likelihood, zero potential surprise will only mean unit likelihood in a very special case ... since the realisation of a particular value of x may cause no surprise even though that result was not considered certain.” As Turvey

²¹ The deliberate inability to distinguish between “crucial decision” and “unique decision” was the target of harsh criticism from practically all commentators, even from those who conceded that Shackle’s theory was a viable alternative to the mainstream. For instance, see Shubik 1954, Georgescu-Roegen 1958 and Ellsberg 1961.

²² Letter from H. S. Houthakker to Shackle, May 5, 1950 (Add. MS 7669, 9/1/5). An analogous position was expressed in private correspondence by Modigliani who questioned both the consistency of the axioms and the details of the potential surprise function: “I notice that in commenting in *Economica* on the review of Graaff and Baumol you failed to take up the problem they raised with their example.” Letter from F. Modigliani to Shackle, Feb. 20, 1950 (Add. MS 7669, 9/1/4).

established (1949: 338), this implied non-additiveness of potential surprise, and “there would be no meaning in performing a weighted average on the potential surprise function – hence the concept of focus gain and focus loss.”

Turvey's note opened the debate on the meaning of non-additiveness of potential surprise. Shackle (1949-50) was keen to acknowledge this property as essential, but refused to consider it as a component of the procedure of attributing probabilistic weights alternative to subjective probability, as suggested by Turvey. Shackle (1949-50: 73-74), in particular, did not recognise the key role of the distinction between frequency probability and subjective probability distributions: “If, for the analysis of choice amongst indivisible real life experiment (i.e. choice among courses of action whose outcome cannot be known in advance), we discard the notion of frequency-ratio probability, ought we not also to discard our predisposition towards additive solutions and start afresh with an open mind?”. As we will see in what follows, Turvey's suggestion was taken up again by Edwards and Weckstein.²³

5. Shackle and his critics: late 1950s

In the second half of 1950s two episodes are worth dealing with. The discussion still centred around the possibility of regarding Shackle's theory as a plausible formalised representation of Knight's commonly agreed viewpoint that uncertainty is probabilistically unmeasurable. The first, and most relevant episode was the conference on “Expectations, Uncertainty and Business Behaviour” organised by the Committee on Business Enterprise Research of the Social Science Research Council, and held at the Carnegie Institute of Technology in Pittsburgh in October 1955. The list of those who attended the conference and contributed a paper to the volume collecting its proceedings (Bowman 1958) includes Mary Jean Bowman, Albert Hart, George Katona, Herbert Simon (all members of the Committee), Kenneth Boulding, Ward Edwards, Robert Eisner, Nicholas Georgescu-Roegen and Franco Modigliani, among others. In her introduction to the volume Bowman (1958: 1) clarified that although most contributions were empirically oriented, the organisers thought that a section presenting current theoretical developments was essential, because the study of business decision making drew upon several distinct fields of learning, including economics and the mathematics of probability.

The theoretical section devoted to economics was titled "Theories of the nature and structure and expectations" and consisted of two papers. A survey of the theories of probability and expectations by Georgescu-Roegen was followed by Shackle's presentation of the potential surprise approach with application to liquidity preference. The fact that Shackle's paper was the focus of the conference discussion was severely criticised by Tjalling Koopmans, who argued in favour of the potentialities of Savage's approach. Bowman (1958: 2-3) quotes a sentence from Koopmans's intervention in the discussion: "I would like to have seen more attention given to the axiom system for decision making under uncertainty recently developed by Leonard Savage, continuing and extending the work by Ramsey and de Finetti ... concerned with decision making in general and as such as fundamental to economics as to statistics."

Although Shackle could not attend the conference his paper was circulated in advance. Bowman summarised the discussion in a letter to Shackle, which clearly shows her role in giving Shackle's theory special relevance in the programme.²⁴ In his contribution to the volume Georgescu-Roegen criticised both what he termed the "Subjectivist" and the "Ultrasubjectivist" doctrine. Georgescu-Roegen (1958, p. 21) argued that, as de Finetti himself recognised, "the reasonableness of the individual's decisions involves also the question whether his scale of beliefs reflects the properties of the random phenomena." That is, the connection between the physical reality and the subjective world requires an additional postulate or principle of some sort, which Savage did not provide.²⁵ But Georgescu-Roegen was critical of Shackle's "Ultrasubjectivist" theory as well. Georgescu-Roegen considered the notion of potential surprise "nothing but a truncated complement of belief." He then argued: "Surprise as understood by everybody and as described by Shackle is simply the state of mind created by knowledge of the actual result of an experiment. But

²³ As seen earlier this was also Arrow's 1959 interpretation.

²⁴ Bowman wrote: "I wish you could have been present for the discussion of your paper at the conference ... Unfortunately Marshack had to back out at the last minute. We were fortunate in getting Tjalling Koopmans to take his place Ward Edwards provided the most careful and interesting discussion, a copy of which I enclose Koopmans suggested that we need empirical studies to discriminate between your approach with the two focus points and the Savage approach which would emphasize a central value. ... Thank you for letting us have your paper. You may be assured that it stirred quite a bit of excitement, even though the fire was aimed at your general theory and not this application per se." Letter from M. J. Bowman to G. L. S. Shackle, November 8, 1955 (Add. MS 7669, 9/1/148).

²⁵ "For certainly a theory of probability cannot concern itself only with internal consistency of the acts of an individual, as maintained by Savage," Georgescu-Roegen (1958: 21) argued. And then he concluded "that the Principle of Insufficient Reason partly serves this purpose. But if the Subjectivist Theory includes this principle, it can hardly be distinguished from the Classical doctrine." It may be worth noting that this is Ellsberg's point as well.

this ex post state of the mind is conditioned by the ex ante degree of belief." In Georgescu-Roegen's view potential surprise could be defined simply as the complement to 1 of subjective probability and, as a result, potential surprise was not a novel concept capable of solving the problems that subjective probability left unsolved. Georgescu-Roegen was also harshly critical of Shackle's inability to formalise consistently the measure alternative to probability he was proposing.²⁶

The comments made during the discussion were summarised by Bowman in her introduction to the book, except for Edwards's remarks which were published in a note following Shackle's paper.²⁷ Edwards (1958, p. 44) started with an assessment of Shackle's theory that emphasised the similarity between Shackle's substitute for probability and a probability measure: "On a first reading, Shackle's concepts seem novel and hard to understand. However, with further thought they take on a familiar look in spite of their novelty, for in many respects Shackle's system is rather close to some more familiar systems." "Potential surprise," Edwards wanted to show, "seems to be very similar to what might be called subjective improbability - a subjective probability function standing on his head." But then Edwards moved on to deal with Shackle's insistence on the ineffectiveness of expected utility, which Edwards considered the second main feature of the theory: "Shackle's refusal to add subjective improbabilities, on the other hand, seems to me to be the most important and desirable feature of his system. A great deal of experimental evidence that bears on the additivity of subjective probability is now available and it argues against the additive property so strongly that I do not see how it is possible any longer to defend that property. Fortunately, it may be possible to develop a utility-subjective probability model that is mathematically satisfactory and that does not require subjective probabilities to add to one or anything else."

Edwards's comments are crucial for our reconstruction. First of all, non-additivity, which as seen earlier was a focus of Shackle's interest from the beginning, was justified by Edwards on the basis of experimental evidence. Given that Savage's theory emphasized the internal consistency of individual choice, to show that individuals do not care about

²⁶ In Georgescu-Roegen's (1958: 21, f.) words: "The axiomatic formulation of the theory is, however, far less clear. Axiom 1, for instance, can hardly deserve its title. ... Axiom 7 implies that the coincidence of two unusual events should not surprise one more than each occurrence taken separately. ... I hope that he will one day offer a more precise formulation of the relationship between degree of potential surprise and degree of belief than the vague wording of Axioms 1 and 3."

²⁷ Hart is reported to complain that "the notion that human beings simply cannot set up a meaningful 'subjective probabilities' and apply them to the likelihood that events may diverge from a central estimate strikes me as unacceptable ... it would leave us with such very untractable models that analysis would be paralysed" (Bowman 1958: 6).

consistency in certain situations was a significant counter-example to the subjectivist approach. In fact, this was Ellsberg's main theme of his 1961 article, in which he insisted that individuals asked to make choices within the framework of the famous urn problem were unrepentant violators of Savage's axioms. Moreover, Edwards hinted at the possibility that a suitable modification of Savage's subjective probability measure, a modification admitting the possibility of a non-additive distortion of the probability measure, would render Shackle's approach much more interesting. The utmost importance of Edwards's comments is confirmed by the fact that Kahneman and Tversky (1979) referred mainly to Edwards's experimental results in order to introduce the distortion of probability on which prospect theory is based.

In her letter to Shackle, Bowman addressed the remark made by many during the discussion at the Carnegie Institute that the concept of potential surprise was not clearly defined. "Edwards' approach seems very interesting," she wrote in informing Shackle that she had argued against Edwards "that potential surprise should not be regarded as simply the mirror image of the particular type of subjective 'degree of probability' that Edwards was supporting." But she added that "Edwards, in contrast to Savage, uses a subjective probability concept such that the probabilities are not additive."²⁸ Bowman enclosed a copy of Edwards's note, but Shackle did not make any reference to Edwards in the last version of his paper, which he eventually submitted with a new introductory section. To the best of our knowledge, Shackle never made reference to Edwards in his later works.

Not surprisingly, Shackle's attitude towards the new subjective approach is a recurring theme in the debate of the second half of the 1950s. Almost all commentators referred to the subjectivist developments, but Shackle did not make any effort to understand the development in the probabilistic approach implicit in Savage's axiomatisation. Shackle insisted that even gambling situations, if interpreted as unique experiments, would require the use of potential surprise instead of probabilities. Asked by Gerald Gould (1957) to comment on this point he replied: "Some critics ... have urged on me the view that it is the *kind* of uncertainty which envelopes the business-man's decisions, the confusion and fog, as Professor Carter has called it, of conflicting indications, the difficulty of interpreting his raw material, that I ought to lay stress on, and not the uniqueness of the experiment he makes. However, I think both are true" (Shackle 1957: 661). To counter this consideration Gould (1957: 663) was led to stress the novelty introduced by Savage: "I should like as a sort of postscript to make a remark about the von Neumann-Morgenstern theory of utility ...that is

only applicable to situations where numerical probability can be used. ... The theory says nothing about *a priori* behaviour, nor is meant to. This point is explained very well by Savage in his *Foundations of Statistics*."

The reference to Carter in Shackle's quotation is important. Charles F. Carter was one of the early advocates of Shackle's theory. He contributed two papers to the *Economic Journal* in 1950 and 1953 which provided, first, an assessment of Shackle's theory, and then a generalisation of it intended to strengthen it. He supported Shackle even in the years after 1961, the time that we take as the point of no return for the interest of decision theorists in Shackle's analysis. In 1972 Carter edited a volume in honour of Shackle including Arrow and Hurwicz's paper referred to in the previous section. Despite all this, in reviewing Shackle's 1955 volume collecting the essays of the early 1950s devoted to the restatement of his theory Carter pointed out a clear uneasiness about Shackle's inability to comprehend the subjectivist developments. After recalling that Shackle's opinion of the importance of uncertainty in the economic system was accepted by his critics, Carter (1956: 700) stressed that Shackle's theory of expectations was still regarded as an interesting possibility, "but not [as] one of which much use need be made." Carter then commented that, though this decrease of interest was disappointing, it was not without reason: "This is not a field of study in which a new theory can have strong claims to immediate acceptance; it is not even certain how it can be tested, not what range of alternative theories can fit the facts ... therefore there is a need for people to get together to find the highest common factor of agreement. ... But Professor Shackle ... still gives the impression of campaigning for his own theory, the whole theory, and nothing but the theory."

Shackle's attitude towards alternative theories of uncertainty is exemplified by Carter's (1956: 701) reference to Shackle's failure to distinguish between the frequency and the subjective probability approach: "in his justified attacks on the follies of those who apply frequency-ratio probability to unique situations, he gives little attention to the tradition of another interpretation of subjective probability. He prefers to stay alone, and in so doing makes it difficult for the subject to advance by agreed improvement and amendment." This statement is typical of a number of comments conveying the concern of authors sympathetic with Shackle's approach, but at odds with both his extremely negative attitude towards the application of the subjective approach and his inability to circumscribe the field of application of his own approach.

²⁸ Letter from M. J. Bowman to G. L. S. Shackle, November 8, 1955 (Add. MS 7669, 9/1/148).

The second important episode regarding the diffusion and discussion of Shackle's theory in the late 1950s is the monographic issue of *Metroeconomica*, an Italian-based international journal started in 1949 with the intent to publish technical contributions to economics. Shackle contributed a series of article to the first issues of *Metroeconomica*, among which particularly relevant were those intended to restate the main aspects of his theory (Shackle 1952 and 1953). The 1959 issue was intended to celebrate the 10th year of the publishing of *Expectations and Economics*. Apart from Arrow's article examined earlier, there were two papers of major interest for our reconstruction by Jurg Niehans and Richard Weckstein. Both Niehans and Weckstein put attention on the alternative notions of probability and asked Shackle to take the subjectivist development into account. Niehans (1959: 77-78) recalled that "it is Shackle's *basic thesis* that probabilities cannot be used to describe the uncertainty of expectations of an individual confronted with a decision" and that "Shackle seems to suggest that this thesis is self-evident and not in need of any proof." But, Niehans argued, "today, ..., in a period of lively discussion of 'subjective' probabilities, Shackle would hardly feel so confident ... I am sure that on the contrary he would be the first to agree that his thesis requires to be proved." Then Niehans summarised Savage's procedure to elicit probabilities from choices and clarified: "this probability order is independent of any frequency connotations." Niehans was also clear that Shackle's approach had great relevance despite his inability to consider the subjectivist approach, because there existed situations in which the set of axioms underlying Savage's approach – and the application of Bernoulli's principle of mathematical expectations – did not apply. It is in this case, Niehans (1959: 87 f.) argued, that "a case could be make for a theory which, say, 1) uses probabilities to eliminate 'impossible' states ... 2) does not weigh 'possible outcomes' with probabilities, 3) lets decisions be guided by minimax regret as applied to the matrix of the 'possible' outcomes."²⁹

Weckstein examined frequency probability, logical probability and subjective probability in turn, in order to determinate how apt Shackle's criticism was, with specific regard to the supposed inability of probability measures to deal with unique decisions. As Niehans, Weckstein (1959: 116) strongly argued in favour of the probability approach. He stressed that the probability of a single event is correctly defined in the subjective interpretation of probability: "this concept avoids the difficulties of the frequency notion by

²⁹ The minimax regret was introduced in Niehans' paper as one of the possible criteria intended to cover situations in which the validity of the "principle of mathematical expectation" could be questioned. Other criteria included "minimax risk" (that is Wald's criterion) and the theory of Shackle (Niehans 1959: 87).

requiring no statistical evidence at its basis. Indeed there are *no* evidence requirements for the statement of subjective probability at all.”³⁰ Furthermore, Weckstein contended that it could be adequate to use a probability measure even to account for the residual hypothesis. His argument needs to be quoted at length, because it offered a link to the incoming developments which paralleled Edwards’ remarks: “consider the argument that the probability number assigned to the occurrence of an outcome must be changed whenever the number of outcomes considered change, and that this adjustment of probability numbers has no analogy in the psychology of a decision-maker. This happens to probability statements because of the formal requirement that the sum of the probabilities of all of the possible outcomes equals one ... This is a formal convention which may have no precise analogy in the psychology of decision comparable to Shackle’s potential surprise function which is expressed in psychologically suggestive terms, but it is a convention which is widely understood.”

Shackle commented both on Niehans and Weckstein in his 1961 volume. The relevance of the points made by them both is testified by the fact that Shackle dedicated to each paper a chapter of the book, including a summary of the critic’s argument and his reply. Notwithstanding Niehans and Weckstein’s insistence, in neither chapter Shackle recognised to Savage’s interpretation of probably a status different from the frequency interpretation. Moreover he did not discuss the possibility of alternative criteria of selection of the best action. Shackle’s attitude to trivialize the discussion of alternative explanations is evident here and comes as *a posteriori* justification of Carter’s 1956 comments reported above. However one specific comment to Weckstein deserves to be reported. Shackle (1961: 107-108) quoted Weckstein’s statement on the requirement that probabilities add up to 1 and then commented: “When Professor Weckstein speaks of the ‘*formal* requirement’ that probabilities should sum to unity, I feel that his words convey the suggestion that there might be some escape from this requirement without abandoning the essence of the notion of probability. Surely this is not so? Once we abandon the *distributional* character of probability we are in an essentially different scheme of thought, the conception of a non-distributional uncertainty variable of which potential surprise is an example.” The issue of non-additive probability is apparent but Shackle was not able to develop it.

³⁰ Weckstein (1959: 110) also argued that even frequency probability could claim to be able to deal with singular events on grounds that: “an actual individual event has more than one property on the basis of which it may be classified, and therefore its probability of occurrence will be different depending upon the class with respect to which it is classified.”

6. The searching for a convincing representation of uncertainty: Shackle marginalised.

The symposium “Decision under uncertainty,” published in 1961 in the *Quarterly Journal of Economics*, can be considered as a watershed in the development of modern decision theory. Here, on the basis of observed violations, Ellsberg and Fellner questioned Savage’s subjective probability, while Raiffa argued in its favour. Ellsberg (1961) criticised Savage on the grounds that vagueness about probabilities could lead individuals to violate the axioms of consistent behaviour upon which Bayesian decision theory was based. Savage’s sure thing principle, in particular, was shown to be violated by individuals acting in what Ellsberg called “ambiguous” environments. Ellsberg suggested that, even from a normative point of view, it could be more appropriate to apply a decision rule combining Savage’s rule, to be applied in non-ambiguous situation, with Wald’s rule, to be applied in situation of maximum ambiguity. Ellsberg’s (1961, p. 646) suggestion is worth quoting at length: “I propose to indicate a class of choice-situations in which many otherwise reasonable people neither wish nor tend to conform to the Savage postulates, nor to the other axiom sets that have been devised. ... We might hope that it is precisely in such circumstances that certain proposals for alternative decision rules and non-probabilistic descriptions of uncertainty (e.g. by Knight, Shackle, Hurwicz and Hodges and Lehmann) might prove fruitful. I believe, in fact, that this is the case.”

As Ellsberg (1961: 661-664) observed from the beginning, the rationale under such a decision rule was that in certain situation the use of mathematical expectations seems difficult to understand and new criteria are worth analysing.³¹ Raiffa reaffirmed his faith in Savage’s axioms as normative guides. In referring to experimental sessions he conducted using adaptations of Ellsberg’s example, Raiffa stated: “But if certain uncertainties in the problem were in a cloudy or fuzzy form, then very often there was a shifting of gears and no effort at all was made to think deliberately and reflectively about the problem. ... In reporting this I am just confirming the experience of Ellsberg and Fellner (and also of Shackle, Knight, etc.). However, I draw a different message from this experience. There is a need to teach people how to cope with uncertainty in a purposive and reflective manner, and to break down the taboo that probabilities should only be assigned if one has clear cut

³¹ In his doctoral thesis, Ellsberg (2001, pp. 16-17) added: “when ambiguity is extreme, by any of his indices: relevant information sparse, or obviously unreliable and contradictory; wide differences in the expressed expectations of different individuals; low confidence in available estimates,” the “somber reflections” expressed by Shackle “seem too ominously relevant to the very circumstances upon which this study focuses to be dismissed.”

relative frequency data at hand.” Raiffa reported that experimental subjects who were initially inclined to violate the axioms behaved consistently with the axioms when made aware of randomisation over possibilities. The mainstream endorsed Raiffa’s rejection of Ellsberg, whose results concerning urn problems entered the textbooks as paradoxes.

Fellner made the following point. He argued that the observable decision weights that an individual attaches to prospective events (namely, “the weights attached to an event if the individual bets *on* the event”) may differ from the true probability, or degree of belief, which she would attach to an event if she regards some of her degrees of belief as “shaky judgements.” The directly observable decision weights, Fellner maintained, can in many cases be derived from subjective probabilities by means of a process of distortion, which he called “slanting of probabilities.” The extent of this distortion depends on both the perceived instability of certain types of probability judgements and the magnitude of the prize at stake. Fellner termed his view a semi-probabilistic one, and discussed at length the subtle question of whether the phenomenon of non-additivity was compatible with the notion of rationality. He maintained that when events are unique, that is, when they do not belong to any standard process with objective characteristics, “we should not regard it as irrational if a person develops a reaction to uncertainty rather than is guided exclusively by mathematical expectations; and reactions to uncertainty, expressing themselves in slanting tendencies, are likely to be stronger for a good many non-standard processes than for the standard processes” (Fellner 1961, p. 685). Fellner’s intuition provided the framework for the works of Schmeidler (1989) and Gilboa and Schmeidler (1994) to develop a consistent axiomatic system through a non-additive probability measure that rejects Savage’s solution for the representation of ambiguity and uncertainty.

After Edwards’s early reports on experimental trials (Edwards 1954 and 1958), other violations of the Ellsberg’s type were reported in consistent experimental settings and were definitely brought to the fore by the summary results of Kahneman and Tversky (1979). On the basis of the experimental evidence, Kahneman and Tversky put forward prospect theory as a model for decision under risk; their goal was to permit descriptive deviations from Savage’s rationality and to achieve theoretical tractability. They attempted to apply distorted decision weights as a basis for probabilistic reasoning, using a non linear transformation of the probability scale that overweighs small probabilities and underweighs high probabilities. Afterwards they applied the transformation to the cumulative distribution of the probability scale, and the theory proved to be applicable to uncertain prospects as well (Tversky and

Kahneman 1992). It then emerged that prospect theory is nothing but a special case of non-additive probability (Tversky and Wakker 1995).

The discussion originated by Ellsberg's 1961 paper is a crucial episode in our story. With the benefit of hindsight, the symposium constitutes a point of no return as regards Shackle's contribution to the debate on decision theory. In the same 1961 Shackle published the final and most complete version of his theory, explicitly intended as *the* non-probabilistic alternative to the probabilistic approach. As seen in the previous section, the themes of the QJE symposium were hinted at both in Niehans and in Weckstein's *Metroeconomica* papers. Shackle discussed these papers at length in the 1961 volume, but we were not able to find any evidence that he acknowledged the possible relevance of new theories of uncertainty. Shackle continued for long to defend his own theory, even on analytical grounds, but the people he talked to and discussed with were no longer well-known decision theorists, either supporters or critics of Savage's approach. Although he was still quoted by both Ellsberg and Raiffa in 1961, and referred to by Fellner (1965), Shackle's inability to follow the debate over violations of Savage's framework made him extraneous to subsequent analytical progress.

7. Shackle's peculiar place in the development of a theory of decision making

As we have detailed, Shackle's special contribution to economics has been to try to discern ways in which the usual treatment of uncertainty diverged for the experience of individuals taking decisions in actual context. His aim was to provide a new way of thinking, arguably more relevant but no less rigorous in logical and mathematical terms. In his book *Expectations in Economics* Shackle held that in economics there was the prevalent practice of ignoring true uncertainty, that is, of expressing it by a numerical probability whose logical origin was obscure, to say the least (Ford 1993).

Among the interpreters of Keynes, Shackle was known for laying the greatest emphasis on questions relating to the unpredictability of future as the foundation for individual decision making. In his famous critical remarks on the Keynesian schools, Coddington (1982: 480) emphasised that those who attributed great significance to Keynes' work on probability theory and on uncertainty in economic decision making "have singled out this uncertainty theme not only for its special importance, but also for his potential for analytical subversion." Coddington made reference to the post-Keynesians like Davidson

and the radical subjectivists like Shackle and Lachmann. The all-embracing subjectivism of Shackle, in particular, was said to be “consistent but analytically nihilistic.”

The way in which Shackle first established and later defended his non-probabilistic approach to decision making gives substance to this view.³² But a closer look of the relationships between Shackle and the contemporary subjectivist developments may also justify the contention that Shackle’s enterprise partook of utopia in the sense of pursuing what, in the theoretical context of the 1950s, was an unattainable aim. Indeed, the theme of “analytical subversion” is central to Shackle’s endeavour. His main objective was to replace the orthodox paradigm for the explanation of individual behaviour under uncertainty. But Shackle intended his argument not simply destructive of the mainstream. To be sure Shackle did not aim to provide an additional normative theory alternative to the probability approach. He aimed to replace the expected utility model by a new model motivated on descriptive grounds. But he insisted that his theory could, in principle, be axiomatised and, as a result, constitute a viable formalised alternative to the mainstream.³³ Potential surprise was Shackle’s replacement for probability, designed to make room, on formal, analytically consistent ground for what he called a residual hypothesis. The role of the residual hypothesis was to make sure that a proper theory of decision-making did consider, unlike the situations assumed to hold under probability calculus, that “the universe of discourse is not know” (Ford 1993: 694).

Shackle insisted that, in order to clarify the influence of potential uniqueness of many important decisions, a better appreciation of the epistemic states of the individual was needed. For Shackle, a descriptively relevant theory of decision making required explicit recognition of personal mental activities, like imagination and the accommodation of potentially unknown events. In order to do so he marked the difference of his theory from the use of probability and insisted that an alternative non-probabilistic set up could be provided.

Shackle was the only one among the dissenting decision theorists, in the early 1950s, and the dissenting Keynesians, later, who after discussing the limits of probability calculus consistently pursued the aim of providing a non-probabilistic decision theory. For instance, the argument that, in order to apply probability theory, one must assume replicability of an

³² Shackle himself accepted that, from the point of view of “the builders of precise predictive models,” he could be considered a nihilistic. From this viewpoint, he contended, also Keynes could have been depicted as nihilist (Shackle, 1984: 391).

experiment under the same conditions was restated by the Keynesians authors arguing against the rational expectations revolution.³⁴ Moreover, the Austrians have emphasised the strict relation between Lachmann and Shackle in regarding the future as undetermined, and choice as the result of a process of imagination creating a definitive cut between past and future (Vaughn 1994). But no follower of Shackle have been able to propose a formalised alternative, and this is why he still remains *the* outstanding dissenting Keynesian to refer to.

The history of modern decision theory, from the 1950s up until the late 1980s and the development of the non-additive approach of Gilboa and Schmeidler, has provided alternative theoretical models designed to explain choice in situations of less than complete knowledge. But for long these theories have been based upon the fundamental assumption of a single, additive probability function. Shackle's aim to pursue a non-probabilistic approach by abandoning this fundamental assumption proved to be utopian in the face of the consolidating subjectivist developments. But it might be vindicated by the current developments.

8. Concluding remarks

Our reconstruction of the period of maximum influence of Shackle's ideas, which ranges from 1949 to 1961, have tried to show that commentators sympathetic towards Shackle's search for a new foundation of decision theory highlighted a number of points of major interests. But Shackle did not try to apply it, on the grounds that they were based on probability theory. He continued to pursue his own utopian project of providing a non-probabilistic account of decision under uncertainty. Shackle's denial to concede to Savage's representation the status of a theory of decision apt to deal with singular acts made him alien both to the development of the new mainstream and, what is more relevant, to the discussion on the alternatives to subjective expected utility.

This is mostly unfortunate because as we reported in the previous sections there is ample evidence that most of his insights are at the basis of current developments of decision theory. Had Shackle been able to completely understand the comments and appropriately

³³ Indeed, Shackle's inability to comprehend the difference between von Neumann and Morgenstern and Savage's subjective approach may find origin in the fact that, from a descriptive point of view, the two theories are alike.

³⁴ See Davidson (1983), where Shackle's theory is quoted in support of the applicability of a Keynesian theory of probability.

develop the suggestions he received, his theory might have turned into a more appealing alternative to Savage's theory than it actually was.

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