

Rethinking Rationality Theory's epistemological status. Normative vs Positive approaches

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1. Introduction

In 1944 John von Neumann and Oskar Morgenstern proposed an axiomatic theory of choice under risk – known as Expected Utility Theory (EUT) – which implied the Bernoullian expected utility hypothesis. Their proposal attracted many economists because it solved the apparently unmanageable problem of how to measure utility and, under well specified conditions, allowed predictions about the choices that an individual would make among alternative lotteries. In the past utility theory had been successfully applied to economics in situations in which certainty prevailed (like consumption), but it was considered useless under conditions of risk and uncertainty. Neumann – Morgenstern's theory, as well as other equivalent theories developed shortly after its publication, seemed to open the door for the formidable task of unifying the field of economics. Hopefully, adequately developed it could help to explain decisions involving risk like gambling and insurance, which at that time were considered beyond the scope of economic theory and originated in arbitrary behavior or the prevalence of psychological factors.

A few years after EUT was proposed a debate about its epistemic status started. Some authors, like Milton Friedman, assumed the positive-normative distinction, traditional in economics, and considered that EUT was merely an empirical (descriptive) hypothesis.¹ Other economists (among them Savage (1954), Marschak (1950), Strotz (1953) Ellsberg (1961) and Raiffa (1961)) found the axioms of EUT persuasive and plausible at the normative level. If they were right, economics might count at last with a *general* theory of *rational* decision, applicable to certain as well as risky and uncertain choices.² All of them took for granted that the descriptive and normative adequacy of EUT were not only desirable but also perfectly compatible and reachable goals. In the following sections the importance of counting with a well confirmed rational theory of decision making will be examined and its feasibility critically revised. We also point out that the normative dimension of EUT was misunderstood by leading economic figures, and the crucial distinction between paradoxes and anomalies overlooked.

¹ See Friedman and Savage (1948, 1952), where Friedman's methodological emphasis clearly prevails. Friedman's most complete account of his epistemic vision of economic theories may be found in his 1953 article.

² "These axioms have strong intuitive appeal. It would seem that every normal person would clearly accept them as precepts of behaviour" (Strotz, 1953, p. 391). "...Savage's theory is not a descriptive or predictive theory of behavior. It is a theory which purports to advise any one of its believers how he *should* behave in complicated situations, provided he can make choices in a coherent manner in relatively simple, uncomplicated situations" Raiffa (1961, p. 690).

2. Normative analysis of EUT. *Feasibility and adequacy of a norm*

Though Bernoulli had made some indications about the normative value of his Expected Utility Hypothesis, he regarded it mainly as descriptive in nature. Later decision theorists, like Neumann and Morgenstern themselves, and, even more explicitly, Savage (1954), Marschack (1950) and Strotz (1953), underscored more explicitly its normative import. According to this view, EUT is not only a descriptive theory about how people make *in fact* decisions, but also a prescriptive set of rules about how decisions *should* be rationally made.

“Consider any person not deemed insane who holds contradictory preferences.... Imagine that we explain to this person the nature of the contradiction, pointing out clearly how his preferences violate our axioms. Will he in consequence of understanding the nature of the contradiction decide that his preferences are ill-founded and proceed to change them, or will he persist in his original preferences even though it is entirely clear to him exactly what precepts his preferences violate. If for nearly every person holding contradictory preferences an understanding of the character of the contradiction induces him to straighten out his preferences, then the Neumann-Morgenstern axioms may properly be regarded as precepts of rational choice. My own feeling is that it would be a strange man indeed who would persist in violating these precepts once he understood clearly in what way he was violating them” (Strotz, 1953, p. 393).

Indeed, both properties – the *normative* as well as the *descriptive* character of EUT – were considered not only compatible, but also intimately related. The hopes of Savage, Marschack and Strotz regarding its empirical adequacy were probably based on what they believed was its normative value: regularly “normal” individuals can learn from their mistakes. As Strotz (1953, p. 393) said, “my own feeling is that it would be a strange man indeed who would persist in violating these precepts once he understood clearly in what way he was violating them”. As long as Expected Utility Theory injects rationality upon people’s behavior, eventual deviations from the correct choices could be gradually eliminated. Though EUT might be initially inadequate from a descriptive point of view, its influence upon agents’ decisions would result in a progressive adjustment between its prescriptions and the observed choices of individuals. Its normative adequacy guarantees its self validation at the empirical level.

Is this happy dream of a EUT sustainable on empirical and normative grounds likely to come true? To be satisfactory any prescription should comply with two main requisites: a) *feasibility* (individuals must be able to follow the instructions received from the theory); b) *adequacy* (its instructions must be “reasonable” or unassailable after rational analysis). Both conditions are necessary for the normative adequacy of any axiom of EUT. But they are independent: granted the feasibility of EUT’s axioms their adequacy must still be proven. Below, we examine first the intuitive plausibility of EUT’s axioms and later the legitimacy of its consequences.

2.1. Normative plausibility of axioms and assumptions of EUT

How good is EUT as a theory of *rational* choice?³ The theory has not only axioms (explicit postulates) but it also involves assumptions. It is not totally clear what the logical status of an assumption is but if the axioms and assumptions of EUT have been identified, as some authors claim, in some cases its normative assessment may be carried out *directly* by employing the two criteria described above. A useful and comprehensive description of the content of EUT is provided by Starmer (2000, 2 and 3), and his classification might be sketched as follows:

Axioms

- 1) Completeness
- 2) Transitivity
- 3) Continuity
- 4) Independence

Assumptions

- 1) Procedure Invariance
- 2) Description Invariance
- 3) Stochastic Dominance

It is beyond our purpose to undertake a thorough analysis of every postulate of EUT, but a brief indication of how to use these outlined criteria would be helpful. Transitivity, for instance, seems *feasible*, in the sense that no obstacle prevents individuals to understand the rule and follow it consciously. It is also easy to defend its *adequacy* resorting to the money pump argument. In the case of assumptions, stochastic dominance seems also unassailable: as soon as individuals are aware that option A dominates option B they choose the first alternative. If every action and assumption of EUT could be defended in this way its legitimacy would be beyond question.

There are cases, however, where those criteria cannot be applied. Continuity and Procedure Invariance, for instance, cannot be easily understood as rules or precepts for rational action and are best defended as technical conditions that should be fulfilled for EUT's mathematical tractability. Unfortunately, a sharp demarcation among precepts of rational conduct and technical requirements cannot be formulated in logically acceptable terms: both are indistinctly treated as necessary conditions for the attribution of a utility function to individual agents. Logic alone is unable to show the difference between the two types of sentences.

2.2. Baumol and Ellbergs' objections to EUT and SEUT

There is another way, this time *indirect*, of assessing the normative adequacy of EUT, which puts its *consequences* at the center of the analysis. In his 1951 article Baumol objected to the current efforts for pushing forward the scope of the expected utility hypothesis arguing that in so doing excessive restrictions on preferences were incorporated.

“The point is simply that the assumptions of the system, in Samuelson's happy phrase, ‘put a straitjacket on the person's preferences’. Once he has made up his mind on a few things, the rest is decided by him for the rules. From his choices among some limited sets of items we know how he will react to the rest, and there is

³ This question would have been interpreted as rhetoric for most working economists after the Neumann-Morgenstern revolution. In allusion to SEUT, Ellsberg (1961, p. 650) pointed out that “In general, as one ponders these postulates and tests them introspectively in a variety of hypothetical situations, they do indeed appear plausible. That is to say that they do seem to have wide validity as normative criteria (for me, as well as for Savage)”.

1/3 (otherwise he would have bet on II). But then he should have bet for III, not IV in the second stage, because he should have believed that the probability for the chosen ball to be black or yellow could not be greater than the probability that it was red or yellow. Ellsberg concludes that in cases like these choices do not reveal probabilities, as Savage pretended. In his words:

“for any values of the pay-offs, it is impossible to find probability numbers in terms of which these choices could be described – even roughly or approximately – as maximizing the mathematical expectation of utility” (p. 655).

Until now, supposing that the “experiment” has been correctly devised, Ellsberg has found out an *anomaly* of SEUT. This finding has little meaning in itself, because the deviation may be caused by irrational behavior on the part of individuals. But suppose the agents are shown their inconsistency and are then allowed to reconsider their choices in light of the new information. Taking as granted the *normative* adequacy of SEUT, they should modify their pattern of choices in a way consistent with Savage’s axioms. However, many decision-makers *do not rectify* their previous decisions even after knowing they were mistaken according to the theory.

“The important finding is that, after rethinking all their ‘offending’ decisions in the light of the axioms, a number of people who are not only sophisticated but reasonable decide that they wish to persist in their choices. This includes people who previously felt a ‘first order commitment’ to the axioms, many of them surprised and some dismayed to find that they wished, in these situations, to violate the Sure-Thing Principle. Since this group included L.J.Savage, when last tested by me (I have been reluctant to try him again), it seems to deserve respectful consideration” (Ellsberg, 1961, p. 656).

The important point in Baumol and Ellsberg’s “experiments” is that the apparently reasonableness of the “biased” choices transforms the anomalies into *paradoxes*. The so-called paradoxes of EUT or SEUT are relevant for its *normative* evaluation because in these cases (unlike what happens with the presence of anomalies) the elections made by the individuals do not appear to be irrational. The presence of a paradox does not mean that the subjects cannot or do not wish to adjust their choices to the axioms of the theory; it rather indicates that even if the individuals had the super-human capacities needed for behaving as the theory instructs them, some of the results reached in so doing would be unacceptable anyway. The problem with paradoxes is not that as a *matter of fact* the subjects “deviate” themselves from the standards. The problem arises because the “biased” observed behavior may be considered at least as rational as the one prescribed by the theory. What is at stake then is the *adequacy* of EUT and SEUT.

The presence of paradoxes is important in another sense: they indicate that two different notions of rationality interfere with each other: the one defined by the theory, which disqualifies as irrational any deviation of the standards posed by itself, and another – intuitive– notion of rationality which allows a defense of some choices considered biased from the standpoint of EUT.⁴ So, anytime a paradox arises it carries with it two main implications:

⁴ Ellsberg (1961, p. 656) probably had a similar idea in mind. Confronted with paradoxes he considered that “the choices themselves do not appear to be careless or random. They are persistent, reportedly

a) an intuitive theory of rationality inconsistent with the traditional one; b) in this particular case such a theory performs at least as good as (and sometimes better than) the more formal theory incorporated in EUT.

Let's look more closely at the relation between these two concepts of rationality. Initially, given any definition X, one could decide whether a particular entity belongs or not to its domain. As long as EUT is empowered for defining "rationality", any singular choice should be assessed by its standards. Any time a definition is used it is posed beyond discussion: its application assumes its validity. How could a definition be put in question? One way is by first identifying – in an intuitive manner – a number of admittedly rational choices and look then whether they belong to the domain of the definition. This procedure is the opposite to the one mentioned before, where the definition constituted, so to speak, its own domain. In this second case, instead, the domain of rationality is set first (at least partially) and later a search for a definition that conforms to it begins.

Usually, however, an intermediate way is followed which allows for a feed-back process between the definition and its intended domain. The sequence could be as follows. First a vague and intuitive idea of rationality is advanced and with its help some behaviors are identified as rational. Second, a more formal definition of rationality (for instance, SEUT) is designed with the purpose of increasing the accuracy and precision of the former notion. This procedure is called *elucidation*. A good elucidation should preserve in its domain most of the behaviors pertaining to the domain of the vague notion, but not necessarily all of them, because the new concept corrects the older one in some respects. The domain of the elucidating concept is not necessarily co-extensive with the one of the elucidated notion. Besides, it is expected from a good elucidation that in cases in which maladjustments with the older notion are found, further inspection will reveal its superiority.

One can say that these two notions of rationality share their "power". The elucidation restricts the scope of the primitive notion, but at the same time the latter retains its power imposing restrictions and limits on the formal notion. Which one is more powerful is a relative point. Once a formal definition has been imposed people apply it without judging its merits, so that it works as a benchmark for rationality. But when the construction of the formal notion has taken place the benchmark is provided by the vague and intuitive notion. Some of this can be observed in Plato's dialogues where Socrates asks his occasional interlocutor to advance some definition (for instance, of courage or goodness) showing later that it is ill conceived because it either leaves some important phenomena outside its intended domain or includes some others which shouldn't pertain to it. In all these cases the intuitive domain works as a guide for further modifications of the more formal definition.

A normative assessment of EUT cannot be founded merely on bare empirical evidence. To assess the merits of any rational theory of decision making another normative notion for choice-making is required as a benchmark of rationality. Taken by itself the empirical evidence is irrelevant for normative purposes and should be interpreted normatively for playing such a role. These considerations throw light on the important differences among

deliberate, and they seem to predominate empirically; many of the people who take them are eminently reasonable, and they insist that they *want* to behave this way, even though they may be generally respectful of the Savage axioms. These are strong indications, in other words, not merely of the existence of reliable patterns of blind behavior but of the operation of definite normative criteria, different from and conflicting with the familiar ones, to which these people try to conform".

anomalies and paradoxes. Besides, their impact on the normative value of EUT is unequal. Anomalies could gradually disappear as long as people learn to take decisions correctly, but this fact will leave the value of paradoxes untouched. The traditional post Neumann-Morgenstern's project aimed at the construction of a theory of *rational* decision making will be more endangered by the presence of paradoxes than that of anomalies.

3. EUT as a merely descriptive theory

In two papers written shortly after the Neumann-Morgenstern contribution, Friedman and Savage took as given the positive-normative distinction between sentences or theories disregarding altogether the *normative* dimension of the axioms of EUT. They distinguished also between EUT and what they called Expected Utility Hypothesis (EUH), and focused on the positive problem of the descriptive adequacy of EUH.⁵ In so doing they defended a view of the epistemic nature of Expected Utility Hypothesis which greatly differs from the one that was examined before. There are two different interpretations of the positive approach towards EUH: the first one assumes that it describes the rule that the agents as a matter of fact follow when taking decisions in non-certain settings; according to the second interpretation it describes the objective results of the individual choices (at the individual and aggregate levels). Friedman and Savage endorse this last position:

“The hypothesis does not assert that individuals explicitly or consciously calculate and compare expected utilities. Indeed it is not at all clear what such an assertion would mean or how it could be tested. The hypothesis asserts rather that, in making a particular class of decisions, individuals behave as if they calculated and compared expected utility and as if they knew the odds” (F-S, 1948, p. 298).

Leaving out of consideration the normative dimension of Neumann-Morgenstern's theory had a price for Friedman and Savage: they fell short of fully appreciating the crucial point in the criticism of Baumol referred above. Their reply was indeed disappointing. They got rid of Baumol's position by pointing out that to imagine a possible counterexample for the theory means it is not void of empirical content and blamed Baumol for seeking an unfalsifiable theory of decision making under risk⁶. It is true that Baumol had confusing ideas about the methodological importance of having empirical content, but one thing is to clarify this issue and another thing is to misrepresent the meaning of Baumol's counterexample.

The imaginary objection of Baumol is not a simple (conceivable) anomaly that might arise within EUT's frame. In Popperian terms, taken in great consideration at that time, its counterexample is not a potential falsifier of EUT but rather a *normative* falsifier: it shows that EUT may have consequences inconsistent with patterns of choice which seem to be nonetheless perfectly reasonable. This finding put into question the standard notion of

⁵ EUT is logically equivalent to EUH, but this last expression makes explicit a concrete operation for calculating the expected utilities of lotteries. As usual EUH may be represented as $\sum p_i u(x_i)$.

⁶ Friedman and Savage (1952, pp. 465-466) make a distinction between conceivable behavior that would contradict the hypothesis and behavior that has been observed and contradicts the hypothesis. Referring to the first type of “evidence” they say that “this feature is clearly a virtue of a scientific hypothesis, not a defect – it is a valid objection only to claim that the hypothesis must be true (i.e., is a tautology). The possibility of specifying behavior that would contradict the hypothesis means that the hypothesis is not empty”.

rational choice under risk. Friedman and Savage failed to notice the difference between a simple conceivable *anomaly* (which is a virtue of any theory, at least in the minimal sense that is not completely void) and a *paradox* (which, even if just conceivable and not yet realized, contests the axioms of EUT as precepts of rational action). They didn't see the difference, we guess, because they intentionally pushed the normative approach of theories beyond their consideration⁷.

Interestingly, their neglect *weakened* the content of EUT. A whole set of paradoxical choices which are decisive for the assessment of the theory as long as normative adequacy is required, are dismissed as beside the point if a merely descriptive point of view about theories is subscribed. Friedman and Savage could say then that Baumol's objection only shows that "the expected utility hypothesis is *potentially* fruitful" and, consequently, his critic "is largely irrelevant".⁸

4. EUT as a *descriptive* theory of *rational* choice

During the second half of the twentieth century as well as in recent years a good number of experiments designed to test EUT and SEUT have shown the recurrent presence of anomalies in peoples' behavior. These findings, especially those obtained by Kahneman and Tversky, have put into question the golden dream of a *rational* theory of decision making satisfactory from a descriptive point of view. Whatever the philosophical or epistemological position one can endorse regarding the axioms or assumptions of the standard theory, anomalies are undesired and must be eliminated. Four strategies for dismissing as irrelevant the anomalous findings have been suggested.

a) It is claimed that the so-called anomalies have been obtained in ill-designed circumstances characterized by insufficient incentives. Supposedly they will tend to disappear as soon as adequate incentives are restored. However, this trend has not been observed in many important cases. In a well-known experiment, Lichtenstein and Slovic (1971) found preference reversals using small amounts of money as compensation. When Grether and Plot (1979) replicated their experiment using this time substantive monetary incentives the phenomenon not only did not disappear but was accentuated. Though it is true that in some cases the use of incentives helps to reduce the biases, the general thesis that they always work in this way (and, especially, the idea that proper incentives will in the end completely eliminate the anomalous phenomena) is yet an unconfirmed hypothesis.

⁷ "Although the word 'paradox' is frequently used in the history of EUT, it should be clear from this and the previous examples that it does not refer to deeply engrained conceptual difficulties, such as Russel's paradox in set theory, or the EPR paradox in physics, but rather just to problems or anomalies for the theory that is currently taken for granted" (Mongin, 1998, p. 172). Mongin is correct in that "paradox" means something different in logic and in economics, but he is wrong in saying that the paradoxes of EUT are just anomalies. The Friedman-Savage's neglect is still alive.

⁸ Friedman and Savage, 1952, p. 466. It is worth noticing that even if the descriptive approach of Friedman-Savage is accepted, and paradoxes are treated as mere anomalies, they are still relevant because if confirmed they show that a good number of people do *not* behave as *if* they were trying to maximize their expected utility. The practice of reducing paradoxes to anomalies does not suffice for putting the Friedman-Savage point of view beyond criticism: anomalies (which defy the descriptive credentials of the theory) are far more dangerous in their approach than paradoxes (which questions its normative value).

“It has frequently been claimed . That the observed failures of rational models are attributable to the cost of thinking and will thus be eliminated by proper incentives. Experimental findings provide little support for this view. Studies reported in the economic and psychological literature have shown that errors that are prevalent in responses to hypothetical questions persist even in the presence of significant monetary payoffs. In particular, elementary blunders of probabilistic reasoning (Grether, 1980; Tversky and Kahneman, 1983), major inconsistencies of choice (Grether and Plott, 1979; Slovic and Lichtentain, 1983), and violations of stochastic dominance in nontransparent problems ... are hardly reduced by incentives” (Tversky and Kahneman, 1986, p. S274).

b) Even if we concede that anomalies can not be eliminated, it may be pointed out that individual deviations are *random* and, consequently, they will cancel each other at the aggregate level. However, as Thaler (1986) said, this argument fails because the biases are not only *common* but also *systematic*, a fact that allowed their classification in a variety of persistent “effects” whose occurrences can be predicted beforehand.

c) A frequent standard argument against the relevance of the “effects” is that agents can learn. Being aware that errors are costly they may learn and correct their choices in such a way that biases will be gradually eliminated. This was the kernel of Baumol’s argument. Is it true that agents can learn from their mistakes and correct them? A moderate answer is “not always”. Tversky (1974) and Tversky and Kahneman (1974) show that under uncertainty individuals make systematic mistakes in the formation of their probability judgments. The important point is that subjects keep behaving in a wrong way after being informed of the errors committed and instructed about how to cope with them. Einhorn and Hogarth (1978) argued in a similar way. It seems that learning isn’t an easy task at all, and only takes place when relatively exceptional conditions are fulfilled.

“The problem with many economic models of learning is that they seem to apply to a very static environment. In fact, such models seem to be directly applicable only to the situation in which Bill Murray finds himself in the movie *Groundhog Hog Day*. In the movie, Bill Murray is a TV weatherman sent to report on weather the groundhog sees his shadow on Feb. 2. Murray’s character ends up reliving the same day over and over again. Although he is a slow learner, the opportunity to rerun the same day repeatedly, and to learn from the consequences of his actions each time, creates a controlled experiment in which he is able to learn many things eventually, from how to prevent accidents to how to play the piano. Alas, life is not like *Ground Hog Day*. In life, each day is different, and the most important of life’s decisions, such as choosing a career or spouse, offer only a few chances for learning!” (Thaler, 1986, p.p. 135-136).⁹

d) Finally, it has been held (Friedman, 1953) that market mechanisms (arbitrage and competition) work in the right direction, eliminating the influence (and in the long run the very presence in the market) of those agents who behave in a sub-optimal way.

⁹ Tversky and Kahneman (1986, pp. S274.S275) offer a more accurate classification of the many circumstances in which learning could not take place.

“The argument proceeds something like this. Suppose there were some less-than-fully-rational agents. I like to call them ‘quasi-rational’, meaning trying hard but subject to systematic error. Once these quasi-rational started interacting with rational types, the rationals would quickly take all their money, after which the quasi’s would either learn or would be rendered economically irrelevant” (Thaler, 2000, p. 136).

The usual defense of the transitivity principle (money pump argument) and the probability axioms and Bayes’s Theorem (Dutch Book Arguments) is based on this idea. However, “the claim that the market can be trusted to correct the effect of individual irrationalities cannot be made without supporting evidence, and the burden of specifying a plausible correcting mechanism should rest on those who make this claim” (Tversky and Kahneman, 1986, p. S275).

While the supporting evidence is still lacking, there are some indications that the opposite may be true. Russell and Thaler (1985) show that this is the case in the goods markets¹⁰ and De Long et. al. (1999) make a similar point regarding the stock market. A tradition that goes back to Keynes explains the stability of the financial market by the unavoidable diversity of its players.

The many mechanisms just described, designed to explain away the anomalies failed to achieve their goal: the “effects” have been reproduced in different settings and their existence has been finally accepted. Theorists of decision making started then to elaborate alternative theories which allow the presence of the observed deviations. If there is a mismatch between theory and reality, and the facts cannot be modified the adjustment has to be achieved by changing the theory itself. However, EUT or SEUT were attractive mainly because of their *normative plausibility*... Could the alternative theories proposed for “explaining away” the phenomena retain this property?

5. The end of the Golden Dream

According to Kahneman and Tversky, EUT has two essential assumptions -*dominance* and *invariance*-, whose legitimacy from a normative standpoint seems at first sight unassailable. “Dominance is both simpler and more compelling than cancellation and transitivity, and serves as the cornerstone of the normative theory of choice”, and invariance “captures the normative intuition that variations of form that do not affect the actual outcomes should not affect the choice” (Kahneman y Tversky, 1986, p. S253).

The problem arises because many choice situations show “framing effects”, in which people’s choices depend on how the options are described. This fact violates invariance, and

¹⁰ “In this paper we start to explore the implications of irrationality for economics. ... We then consider what happens if rational and less than fully rational agents ... interact in the competitive markets. We show that knee-jerk reaction of some economists that competition will render irrationality irrelevant is apt only in very special cases, probably rarely observed in the real world” (Russell and Thaler, 1985, p. 1071). According to the authors the usual justification of maximizing models claiming that “markets guarantee that only rational behavior can survive” never applies, “except (perhaps) in some highly efficient financial markets” (id., p. 1080).

consequently dominance, because as Tversky (1999, p. 187) stated “any failure of description invariance can induce a violation of dominance”.

It is important to grasp the nature of the difficulty pointed out. It is not just that agents as a matter of fact violate each of the assumptions. The trouble is that decision makers cannot, *in principle*, for *psychological* reasons, comply with the suppositions. According to Tversky and Kahneman (1986, pp. S256-S257) humans do not have mechanisms for securing invariance. As Kahneman (2003, p. 1459) explains:

“The basic principle of framing is the passive acceptance of the formulation given. Because of this passivity, people fail to construct a canonical representation for all extensionally equivalent descriptions of a state of affairs (...). they do not spontaneously transform the representations of puzzles or decisions problems. Obviously, no one is able to recognize ‘137 x 24’ and ‘3288’ as ‘the same’ number without going through some elaborate computations. Invariance cannot be achieved by a finite mind”.¹¹

If they are right, the biases originated in framing effects are unavoidable given the cognitive and computational limitations of the human mind. The usual mainstream arguments according to which agents can be rational given satisfactory incentives and time enough for collecting information and learning are shattered. This virtuous mainstream process is a psychological impossibility. In our terminology, the assumption of invariance is *not feasible* for human beings. So, *no* adequate theory of *rational* decision can be satisfactory for *descriptive* purposes. One has to choose.

“Because framing effects and the associated failures of invariance are ubiquitous, no adequate descriptive theory can ignore these phenomena. On the other hand, because invariance (or extensionality) is normatively indispensable, no adequate prescriptive theory should permit its violation. Consequently, the dream of constructing a theory that is acceptable both descriptively and normatively appears unrealizable” (Tversky and Kahneman, 1986, p. S272).

The usual vision that economists have of their own discipline has to be changed or, at least, qualified. It would not be permissible to say any more than:

“the concept of rationality is used in economic analysis in three different ways: as a descriptive hypothesis about behaviour, as a normative concept, and as an aspiration, i.e., a way of organizing behaviour which is desirable and to which individuals and societies should be educated... [...] in fact the three points of view are in perpetual interaction” (Arrow, 1999, p. XIII).

6. How bad is the impossibility of the Golden Dream for economics?

Is it really fatal for an adequate *rational* decision theory the fact that it will be descriptively inaccurate? The dream of a fully satisfactory theory, reported at the beginning of this paper,

¹¹ From a very different perspective, Radner (2000) considers this point as a formidable setback for any theory of rational decision.

suggests that the answer should be YES. However, the ingenuity of Raiffa (1961) has brought about a counter-argument which is not only valuable in itself, but helps to throw light on the crucial distinction (frequently overlooked) between anomalies and paradoxes. In his view it is *not* desirable that a plausible normative theory of decision making happens to be also satisfactory from a descriptive standpoint, because if this goal were reached it would turn the theory completely irrelevant.¹² Raiffa's argument turns an evil into a virtue: the unfavorable empirical evidence regarding the pretension of truth of EUT or SEUT is converted into favorable evidence regarding their intended normative relevance. Besides, the Kahneman and Tversky argument about the supposed inability on the part of the agents for learning is dismissed. In fact, if people had the capacity for incorporating the rational decision rules of standard theory into his "intuitive system" and in so doing were able to decide rationally spontaneously and without effort, the theory would become descriptively right but its normative relevance would come to a halt. Against common sense Raiffa argues that anomalies are important because they testify about the relevance and necessity of the theory¹³. Crediting his ingenuity we will register this thesis with his name:

Raiffa's Law: The normative relevance of a theory of choice is directly related to its empirical failure (more precisely, to the amount and quality of the anomalies that overflow real choices)

In our view, however, Raiffa's law is not so strong as it seems. He misinterprets Ellsberg's point the same way Friedman and Savage misunderstand Baumol's objection. In both cases "anomaly" is erroneously identified with "paradox". Ellsberg is not only saying that it is possible to behave against Savage's axioms (which is trivially true), nor does he say that the individuals do commit in practice these violations. He claims that by performing "hypothetical experiments", choice situations may be identified in which it would be *reasonable* to choose in a manner that conflicts with the directions given by the standard theory. Ellsberg constructs a paradox, not just an anomaly. The important point is that Raiffa's law is irrelevant in the face of paradoxes. It may be true that the anomalies of the theory "reveal" its necessity as normative theory, but paradoxes put in question its credentials for doing the job.

The presence of a paradox involves the existence of an alternative intuitive notion of rationality which, at least in the particular situation considered, performs better than the standard one. If it is right that the presence of anomalies in decision making under risk or uncertainty shows that *some* normative theory is needed (not necessarily EUT or SEUT), it is also true that the presence of paradoxes involves the lack of credentials of the current theory for advising how to choose. To Raiffa's law we may oppose Ellsberg's law, in merit of having noticed the power of paradoxes when the target is the normative status of a theory of choice.

Ellsberg's law: The normative relevance of any theory of decision making is inversely related to the number and importance of the paradoxes that could be imagined.

To be aware of the differences between anomalies and paradoxes matters!

¹² "If most people behaved in a manner roughly consistent with Savage's theory then the theory would gain stature as a descriptive theory, but would lose a good deal of its normative importance. We do not have to teach people what comes naturally" (Raiffa, 1961, p. 690-691).

¹³ If one gives credit to his idea, should it not be pushed a little further and claim that anomalies are welcome and their persistence convenient?

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