Decision under Uncertainty – State of the Science*

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Abstract

Expected utility maximization is the dominant theory for decision making under uncertainty. Over the past decades evidence has been accumulated, indicating that the theory is often violated, and sometimes even questioned as a normative standard. Alternative theories have been proposed, for choices with known and unknown probabilities. These theories, in turn, have also been challenged by more recent evidence. The present survey attempts to provide an overview of the field, highlighting some questions that economists should pose when modeling choice under uncertainty.

1 Introduction

People have been making decisions under uncertainty since days of yore. There is evidence that insurance contracts existed four millennia ago (see Sheynin,

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1977, who cites Reicher, 1947). The Bible tells the story of Jacob who splits his flocks before a meeting with his brother Esau. Jacob has rather compelling reasons to fear the encounter, and his explicit reasoning reads very much like risk aversion.[1] Moreover, animals also make decisions under uncertainty, and they often seem to exhibit risk aversion (see Kacelnik and Bateson, 1996, and references therein). Yet, formal, explicit theory of decision making was developed only in the 16th-17th centuries, in tandem with the emergence of probability.[2] The theory flourished in the 20th century, and in the 1940s-1960s (subjective) expected utility theory [(S)EUT] became the dominant approach to deal with decision making by economic agents, for positive or normative purposes, with known or unknown probabilities.

Critiques of the theory soon emerged, but they did not have a major impact on economic research for a while. In the following decades two developments occurred: on the one hand, economics has made tremendous progress on a variety of questions, mostly using SEUT; on the other hand, psychological evidence was accumulating, casting doubts on the theory's validity. The resulting tension gave rise to alternative theories of decision making under uncertainty, which were developed over the past four decades. The present paper attempts to survey these developments. Its scope is limited to decision under uncertainty, and, within it, to a bare minimum that can fit into a reasonable-length piece and be read also by non-specialists. In delineating this scope, many difficult, subjective, and probably debatable choices were made. A more satisfactory review would have to be considerably longer. Further, similar reviews can and should be written about other subfields of decision science, including, but not limited to, decision making over time, other-dependent preferences, the ingredients of utility, and consumer choice under certainty.

The above notwithstanding, some of the discussions that follow may also apply to these subfields. Section 2 offers the general framework of questions to be asked. It starts by an attempt to map the type of goals decision theory

[1] "...And said, If Esau come to the one company, and smite it, then the other company which is left shall escape." King James version, Genesis chapter 32, 6-8.

[2] Cardano (1564) is considered a pioneer in the field of probability theory, about a century before the burst of activity in the mid-17th century.

sets (2.1). It then deals with the questions about the theory, or, to be precise, what could be our responses to conflicts between theory and evidence (2.2). This discussion calls for some questions about evidence, which we should also pose (2.3). The following section (3) deals with decision under uncertainty, and it is divided along the traditional lines of risk (3.1) and ambiguity (3.2), where the former refers to situations in which objective probabilities are given, whereas the latter – to situations in which they are not. Each subsection is divided into short descriptions of (i) the classical theory, (ii) the main problems encountered, and (iii) some of the new models that have been offered. Section 4 contains some methodological comments, and, like Section 2 may be relevant for other domains of decision theory. Section 5 concludes.

2 Framework

2.1 Questions of decision theory

Decision theory has several academic markets in which it attempts to sell its merchandise. These can be differentiated according to two basic questions: (i) Are decisions, per se, the subject matter of scientific inquiry? Or is it the case that scientists are interested in a broader phenomenon, whose analysis requires a micro-level modeling of decisions? (ii) Is the scientific endeavor positive or normative in nature? While both questions can be viewed as a matter of degree rather than of kind, it might be useful to keep in mind a simplistic 2x2 matrix for the intended goal:

	Positive	Normative
The decision per se	I	II
The decision as part of a larger phenomenon	III	IV

Practically any field in the social sciences touches upon decision making. This is true also of parts of other fields that are not typically thought of as social science, such as philosophy and cognitive science. Moreover, almost any field can find itself interested in any of the four categories in the matrix. Yet, disciplines do vary in the type of questions they pose about decisions. Psychology typically asks questions about the way people tend to make decisions (as in (I)), and, conversely, most studies in this category will be classified as psychological. By contrast, fields as diverse as health, management, and philosophy may pose normative questions about decisions (belonging to category (II)). In a management application the question might be, which is the most profitable way to make decisions in a business setup, whereas in philosophy – which model guarantees an ethical decision. Consequently, these fields would have rather different notions of “a good decision”. Yet, they belong in the same category in the sense that they are interested in procedures for “good” decision making, and they do not attempt to capture a given reality; rather, they are most useful when they suggest how to *change* the reality of decision making.

In categories (III) and (IV) we find a variety of studies in economics, finance, political science, and other fields that seek to describe, explain, or predict economic, political, or social phenomena, or to make policy recommendations about such phenomena. In the former case, of a positive theory, the questions about decisions would tend to be positive as well. For example, in making economic predictions, one would like to rely on good predictions of decision making by agents in the economy. Often, a normative theory would also rely on positive decision theory; for example, making recommendations about taxation, economists would adopt a normative approach to the government's economic problem, but would typically assume that they should take households' decision making as given.

As mentioned above, the distinctions drawn here are clearly fuzzy. For example, a government's decision to adopt a “nudge” policy (Thaler and Sunstein, 2008), or a central bank's decision regarding the interest rate can be viewed as belonging to (II) or to (IV). Similarly, the distinction between (I) and (III) suggests an entire gamut. However, asking what decision theory has accomplished, and which goals it should set for the future, I find it useful to draw the distinctions above.

Further ramifications may be of interest. First, while the focus of this paper is decision *theory*, one may think, more broadly, of decision *science*, emphasizing data and observations as well as theories. Many such observations result from experimental tests of theories, and will be an inherent part of the present discussion. Hence, this distinction will not play a major role in the sequel. Second, one may distinguish between empirical and theoretical studies in categories (III) and (IV) above. For example, both micro-based theoretical analysis and structural econometric estimation need to use decision-theoretic modules as plug-ins in their models. It is worthwhile to keep in mind that, judging the contributions of decision theory, we might wish to draw the empirical/theoretical distinction as well.

2.1.1 Normative theories

The discussion above uses the terms "positive" and "normative" as is common in economic parlance. In bold strokes, the former refers to the "is" and the latter – to the "ought".[3] While the terms are widely used, their exact definition might be worth a short digression.

A theory can be interpreted as a way to explain/describe/predict reality, as is the case in the natural sciences, but it can also be interpreted as a call for changing reality, which, in the social sciences, is also a possibility. The question about the goal of a theory is crucial to its assessment: whereas a positive theory should be tested for its fit with reality, or the precision of its predictions, a normative theory should not. In fact, a normative theory that matches reality would be useless, as it would not offer ways of changing the current state of affairs.[4]

The degree to which a theory is successful at describing reality or predicting future observations is clearly a complex, multi-faceted notion. Theories might provide good descriptions in some domains and poor ones in others; there is

[3] The distinction goes back to Hume (1739) at the latest. See Book III (Of Morals, pp.469-470 of the online Wikipedia version).

[4] One may further draw distinctions between explanation, description, and prediction, and thus between descriptive and positive theories, as well as between normative and prescriptive ones. These distinctions are less crucial for our purposes here.

considerable freedom in selecting the scope of phenomena discussed, as well as the statistical measures employed. And yet, we seem to have a vague understanding of the meaning of "a successful theory", and at least some agreement on the type of observations that would make a theory more vs. less successful as a positive one.

But what does it mean for a theory to be a "good normative theory"? A theory that calls for changes in reality should describe a different reality from the one we know – but which different reality? What is the standard against which we can test such a normative theory?

One possibility is to judge normative theories against one's values. Indeed, a normative call to change the tax code so as to reduce inequality involves value judgments. But I find the value-based definition too narrow in some sense, and too wide in another. It is too narrow because normative theories need not resort to values as we usually think of them. For example, the theory that people make binary choices in a transitive manner can be interpreted as a normative theory. As such, it would read as, "Those people who do not make choices in a transitive way should do so!" This appears to be a rather compelling statement. Students and laypeople who see it for the first time tend to agree that transitivity is a desirable property. And yet, it does not seem to be a "value" in the sense of moral values. On the other hand, I think that normative theories should not explicitly and self-professedly reflect the researcher's personal values. While perfect scientific objectivity is unattainable, I still believe that it is a desirable goal, and that our academic education and research does not bestow upon us the moral authority needed for preaching our values to others.

I therefore use a slightly different definition of a "normative theory": it is the social scientist's attempt to capture (in a positive sense) the preferences of others in her society. Thus, when transitivity is presented as a normative theory, I would like to read it as saying, "I claim that many people in our society would like to make decisions in a transitive way". Similarly, an argument for progressive taxation should be read as a claim about the preferences of most people in the economy regarding income redistribution. Social scientists are not especially qualified to serve as moral leaders, but they may have a chance of helping people and societies figure out their preferences over the state of affairs they are to experience.

The exercise of defining terms that are in common usage can itself be positive or normative: we may describe the way a term is used in a given community, and we may also suggest that this usage be changed, or more sharply delineated. In the case at hand, I believe that this definition of "normative" is more positive than normative: based on very casual observation, it seems to me that most economists do not believe that they have privileged access to moral truths, and when they use the term "normative" they refer to something along the lines of the definition above. However, to the extent that this definition is normative, that is, to the extent that it suggests that we use the term ("normative") differently than we sometimes do, I believe that it is normative in the sense suggested. That is, my conjecture is that most economists would agree that this is a more fruitful use of the term "normative" than definitions that allow economists to preach their values.

Be that as it may, this is the way I will use the term in the following. That is, a normative theory is a second-order positive theory: it does not attempt to describe reality as it is, but it attempts to describe people's preferences over that reality.

2.2 Questions about Decision Theory

We live in interesting times. We witness a major clash between two bodies of literature. On the one hand, we have the remarkable apparatus of rational choice models, consisting of decision theory, game theory, microeconomic theory, operations research – all of which having roots in previous centuries, and achieving considerable maturity in the mid-20th century. I refer to rational choice "models" rather than "theory", because the general structures under discussion fall short of specific, refutable theories. These models involve abstract concepts, such as acts and strategies, decision makers and players, states of the world and time periods, each of which may have multiple interpretations in a given situation. This freedom of interpretation adds considerable flexibility to these models. It makes them harder to refute by concrete evidence, and thus less “scientific” in the Popperian sense. At the same time, this freedom also makes them powerful tools of analysis, as they become general conceptual frameworks (as referred to in Gilboa and Schmeidler, 2001) that can help organize our thinking on a wide variety of economic phenomena. (See also Gilboa, Postlewaite, Samuelson, and Schmeidler, 2018.)

On the other hand, there is an impressive body of literature, mostly in psychology, showing that the classical assumptions of rational choice fail descriptively. The most prominent contributors to this literature are Daniel Kahneman and Amos Tversky, who, starting in the late 60s, embarked upon a project that was viewed by some as showing that no assumption of rationality holds. Kahneman and Tversky were not the first to question assumptions about reasoning and decision making. Psychologists have shown difficulties in reasoning in general, and about probabilities in particular, starting in the late 1940s (at the latest). Moreover, the most famous counterexamples to expected utility theory were suggested by economists: Allais (1953) and Ellsberg (1961). And yet, there was something rather shocking in the systematic approach of Kahneman and Tversky. The latter used to say, “Show me the axiom and I'll design the experiment that refutes it.” Many of their disciples and followers have carried the torch since, and it seems that, indeed, any theory can be refuted in a rather compelling experiment.

How should we deal with the gap between the two strands in the literature? The normal progress of science, most widely associated with Popper's (1934) view, suggests that in light of conflicts between theory and evidence, the former should be adapted and refined to fit the latter. This, in bold strokes, is the direction taken by behavioral economics: incorporating insights from psychological studies into economic theory, in an attempt to make the theories' predictions more accurate. However, while the natural sciences can only hope to get theory closer to reality, the social sciences might consider the option of bringing reality closer to theory. For example, Kahneman and Tversky (1973) showed that people may be poor reasoners about conditional probabilities, confounding $P(A|B)$ with $P(B|A)$ (for events A, B). We could revise economic theory and allow it to reflect this “Base Rate Fallacy”. Indeed, such faulty reasoning can be useful for marketing purposes.[5] We could also conclude that, as social scientists, our role is to teach probability reasoning, preferably in highschools, so that fewer people would be prone to err. The COVID-19 pandemic provided a relevant example in which the Base Rate Fallacy could lead people to the conclusion that vaccinations are more dangerous than they actually were, and health care professionals found themselves explaining Bayes's rule to the general public (see Washington State Department of Health, 2023).

I do not suggest that it is possible to completely eradicate phenomena such as the Base Rate Fallacy, or that economics as a science should ignore such phenomena because people can be convinced that they are mistakes. I only wish to point out that the social sciences have more than one way of dealing with gaps between theory and evidence. It stands to reason that in many cases we might want to pursue both directions: preaching a theory as a normative one, while also extending it so that it can deal with some violations and become a better positive theory.

Finally, there is a third option of dealing with evidence that refutes theory, which is simply to ignore it. It seems that this was a rather common response when Kahneman and Tversky started their project: many economists felt that the experiments were somewhat contrived or that the phenomena discussed were not so relevant to economics. In some extreme cases it seemed that economists were prone to dismiss evidence based on a strong prior belief, saying “this cannot be”. Evidently, such a response does not appear very serious or intellectually honest. And yet, I think one has to admit that sometimes this response was not completely off. Clearly, ignoring data based on one's hunches is hardly a prescription for successful science. Scientists are not supposed to be narrow-minded and opinionated. And yet, sometimes even narrow-minded and opinionated people may happen to be right.

I do not attempt, here or anywhere else, to provide an authoritative clas-

[5] I was told of a state lottery marketed with the slogan, “100% of the lottery winners bought a ticket”.

sification of violations of classical theories according to the ways we should choose to deal with them. Whether we should ignore evidence, incorporate it into our theories, or try to preach it out of existence depends on many factors, including personal tastes and empirical questions to which I do not have answers. I will only try to raise the questions, and provide some examples where I hope that some partial consensus may emerge. In the following I suggest further sharpening the questions we should ask about any particular violation of a theory.

2.3 Questions about Evidence

When faced with experimental or empirical findings that are in conflict with a classical theory, there are three questions we might ask:

(i) How robust are the findings?

(ii) How relevant are they to economic behavior?

(iii) How rational are they?[6]

The following provides a few words of elaboration on each of these.

2.3.1 Robustness

Economic research often involves work within a certain methodology, coupled with interpretation of the results obtained therein. For example, a theoretical paper would construct a model and prove some theorems about it, but the mathematical model is, almost by definition, not what the paper is about. Rather, the paper should be saying something about economics, and an act of interpretation is required to translate a mathematical result into a statement about the world. Similarly, an experiment would be run to test some conjectures, but the results of the experiment are of little interest if they do not generalize to "real-life" economic settings.[7]

It follows that there are two types of robustness questions one may pose:

[6] The three criteria invite a catchy title such as "The Three R's". This is pure coincidence.

[7] In principle, a similar distinction can be drawn in empirical and even historical work. But in these fields the work-within-the-method already says something about real economic phenomena, and would be considered "economics" even without interpretation.

within the method and beyond it. In the case of a theoretical paper, one would like to know that the model is coherent and the proofs – correct. Beyond these, one may ask how the results would change if some assumptions were tweaked. That is, how robust is the interpretation offered to the original results? Along similar lines, in experimental work one would like to ask whether the results do not involve any mistakes and whether they were analyzed using appropriate statistical procedures – but also, how dependent are they on details of the experimental design?

Both types of robustness questions are relevant to experiments that challenge classical theory. Questions within the method became very pertinent given the ongoing "replication crisis". Nosek et al. (2022) provide a survey of the crisis in psychology, distinguishing among replicability, robustness,[8] and reproducibility. Similarly, Bohannon (2016) and Camerer et al. (2016) discuss similar problems in experimental economics. While these questions are evidently of paramount importance, at least in an ideal world they can be relegated to experts in the field. The general audience of economists might expect different fields to perform their own quality-assurance, as it were. Indeed, it does not seem very efficient to expect economists of all stripes to check the reproducibility of experimental results, nor the correctness of mathematical proofs. By contrast, it seems that the robustness of interpretation should be judged by all economists who may consume the results for their own work.

In the case of experimental work, such questions about robustness of interpretation are tightly related to external validity, and more specifically, to ecological validity. Consider, for example, the "Ultimatum Game" by Guth, Schmittberger, and Schwarze (1982). Presenting it, one often feels that there is no need to report the results: a description of the experiment design is sufficient for the listeners to have a reasonable guess about what happened in the lab.[9] People can use *introspection* to imagine what the data are. Not

[8] Nosek et al (2022) use the term "robustness" in a much narrower sense, referring to different statistical analyses of a given dataset.

[9] This is based on casual observation in classrooms. With this game, as well as with many experiments of Kahneman and Tversky, I often describe the experiment and let students guess what the typical results are, and which assumptions of economics are being challenged

surprisingly, these results are, for the most part, reproducible and replicable. In fact, we can consider the description of the game as a mind experiment and analyze our intuitive predictions, obviating the questions of replicability. By contrast, the ecological validity of the experiment is often challenged. It is natural to ask, what would the results be, were the monetary payoffs multiplied by 1,000,000? Would Player II be willing to give up $1,000,000 just because Player I kept for herself 99 times that amount? Or, what would happen if players could only act after some time lag? For example, Grimm and Mengel (2011) found that forcing a delay of 10 minutes in Player II's response increased the acceptance rate of low offers in a rather dramatic way. These questions are about ecological validity of the experiment; they basically ask, to what extent can we generalize the experimental setting to the phenomena of interest?

Questions of ecological validity have been raised about experiments in psychology and in decision science for many years. For example, Wason (1968) showed that people were performing rather poorly in manipulating simple logical rules, confounding "A implies B" with "B implies A" (rather than equating a conditional statement with its contrapositive). Cox and Griggs (1982) showed that the effect was greatly reduced if, rather than abstract terms, people were asked about concepts they knew from their daily life. Gigerenzer and Hoffrage (1995) showed that people's probabilistic reasoning could be improved if data were given as relative frequencies rather than as probability numbers. (See also Gigerenzer, 1991, 1997, 2005.) By and large, the argument is that people tend to make more reasoning mistakes in abstract and unfamiliar problems than in concrete situations that are more similar to their experiences in daily life. In none of these examples should such a claim be taken as a reason to dismiss the phenomenon or as an excuse to ignore psychological findings. But they do call for caution in consuming experimental results.

Ecological validity obviously depends on the application one has in mind.

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by the study. Typically, students do very well on these guessing tasks. Admittedly, the very fact that they are asked about an experiment gives them a strong signal that there are interesting findings to be discussed. Yet, this is an informative test: not all experimental results seem to be equally intuitive to students.

Heuristics and biases that lead to irrational decisions could be expected to have greater effects on minor decisions that are made in a matter of seconds than on weighty ones that are typically made after reflection and consultation with others.[10] Thus, a given experimental result may appear very relevant to consumer choice, but less so to mergers and acquisitions. Along similar lines, emotions are likely to influence choices that involve self esteem and social rank more than choices that revolve around material payoff. Hence, an experiment may be very relevant to analyzing the behavior of employees when negotiating salaries, but perhaps less so to understanding strategies of firms when purchasing raw materials. The experimental community can be expected to test reproducibility and replicability, but economists who use experimental results should keep in mind that they should pose questions of ecological validity.

2.3.2 Relevance

Another question that should be asked by economists is whether experimental findings in psychology are relevant to economics. In principle, this question deals again with ecological validity, especially if we remind ourselves that ecological validity depends on the ecology one has in mind. Yet, crossing the admittedly-fuzzy boundary between disciplines might warrant a separate term. Some findings may be rather robust as psychological phenomena but less relevant to economics. For example, Tversky and Kahneman (1973) report the finding that people tend to estimate the value of $8 \times 7 \times 6 \times 5 \times 4 \times 3 \times 2 \times 1$ as larger than $1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8$. There does not seem to be any reason to doubt the robustness of this finding. Tversky and Kahneman provide a rather cogent explanation of the psychological mechanism that is likely to be behind it, and the example certainly passes the "gedanken experiment" test: most of us have probably never thought of this question, but when we do, we tend to find the experiment's results convincing. At the same time, the economic implications of this phenomenon seem limited. One can surely conjure up some settings in which such biases would affect economic behavior, and can perhaps be applied in marketing. It seems that this is true of any

[10] See, for instance, Charness, Karni, and Levin (2010).

psychological phenomenon. And yet, it also appears that this type of mistake does not affect economic predictions or policy recommendations to the same degree that do, say, failures of dynamic consistency. (See Strotz, 1956, Laibson, 1997, O'Donoghue and Rabin, 1999.)

Along similar lines, Thaler (1985) discusses the notion of "mental accounting". One of its meanings has to do with the way good and bad news aggregate: participants are told about two imaginary characters, Mr. A and Mr. B, who received different news, sharing the financial bottom line. The participants are asked, "Who is more upset?" or "Who is happier?", while these questions do not relate to any economic decision. These emotional reactions can surely have some impact on purchase decisions. Indeed, Thaler (1985) uses his analysis to explain some practices in marketing (such as offering consumers rebates rather than decreasing the price to begin with). In this sense, the "integration" or "segregation" of transactions is probably more relevant to economic behavior than the perception of $8 \times 7 \times 6 \times 5 \times 4 \times 3 \times 2 \times 1$ as larger than $1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8$. Yet, economists would be justified in asking, "Can you also show me that this makes a difference for the phenomena *I'm* interested in?"

The distinction between robustness and relevance is at least as fuzzy as the boundary between psychology and economics. In particular, questions about monetary incentives offered in experiments are often raised by economists, but may also be viewed as part of the psychological robustness test. For example, the phenomenon of "probability matching" refers to the finding that, when people have to guess the value of i.i.d binary random variables, they tend to choose the more frequent value with probability that matches its relative frequency, rather than with probability 1 (Estes, 1964, 1976). However, it seems that this phenomenon isn't robust to learning with meaningful incentives (Shanks, Tunney, and McCarthy, 2002 – see also Montag, 2021). Clearly, this casts a doubt about its relevance to economics. At the same time, the suspicion that participants misunderstood the task also questions the robustness of the phenomenon from a psychological viewpoint.

Thus, questions of robustness and relevance may be hard to separate cleanly.

Yet, I think it is useful to bear in mind that some violations of classical theory might be robust as psychological findings, but less relevant to economics, while others might have significant impact on the way we think of economics as well.

2.3.3 Rationality

Suppose that we are faced with a violation of classical theory that is both robust and relevant. What should we do about it? The discussion in Section 2.2 suggests two possible ways: integrating the mode of behavior into economic models, or trying to change people's behavior so that the normative theory becomes a better positive one. Which should we choose?

The term "rationality" is used here in a way that is tailored for this problem: a mode of behavior is rational for a given decision maker (in a given setup) if, when confronted with analysis of her decisions, she does not feel that she should have done otherwise. Thus, if most decision makers find a pattern of behavior rational, it would be useless to preach our theories as normative ones, and we should better improve them as positive theories. If, by contrast, most decision makers feel that they should (and could) have known better, the option of changing reality to fit the normative theory is a viable one. Clearly, changing reality this way doesn't happen overnight. But when we think of the impact of decision theory in the long run, the possibility of changing behavior via education should not be dismissed.

Some apologies are due when suggesting a definition of a widely used term such as "rationality". One should first acknowledge that the definition differs from commonly used ones. Weber (1921) distinguished among four types of rationality and rationalization. Simon (1976, 1986) contrasted substantive and procedural rationality. Habermas (1981) introduced the concept of communicative rationality. By contrast, economics, or at least economic theory in the past 100 years or so, has focused on rationality as consistency of choice, making it a matter of form rather than content. Similarly, rationality is sometimes viewed as a match between means and goals, without dictating the latter. In economic and decision theory, rationality is defined by axioms on behavior, which are typically shown to imply (or be equivalent to) a certain mathematical description (such as utility maximization). This notion of rationality is silent on the content of the mathematical objects (say, the choice of the utility function). For many economists, deviations from these modes of behavior are by definition irrational. Given these well-established yet rather different definitions of "rationality", why should we suggest yet another one?

First, I think that the standard economic definition isn't accurate from a positive viewpoint. In discussions among decision theorists, it is not uncommon to hear statements such as "I do not find it irrational to..." when referring to some violations of classical theory. Thus, based on my personal impression, defining rationality by the classical axioms on behavior isn't precisely what researchers mean when they use the term. Second, taking a normative viewpoint, I find the standard definition unhelpful: according to this definition, people who fail to play Chess optimally, or to solve large instances of NP-Hard problems are "irrational". This definition does not distinguish computational limitations from, say, susceptibility to framing effects (Tversky and Kahneman, 1981). But these behaviors differ greatly in terms of their robustness to analysis: most people seem embarrassed when they are prone to make frame-dependent choices, and they can be taught how to immune themselves, as it were, against such mistakes.[11] In fact, pondering choices with friends or colleagues might be sufficient to greatly reduce framing effects. By contrast, when facing an NP-Hard problem, such as a complex scheduling task, or when pondering a complex Chess position, there is no simple way to find the "right" answer. Neither friends nor teachers can show the path to an optimal solution. We may still dub the decision makers irrational, and we can add some other slurs and insults if it makes us feel good about ourselves – but this will not change the way decisions are made. Thus, I find it more useful for our scientific discourse to use the term "irrational" in a more pragmatic way: irrationality is reserved to decision modes that can be changed. It is thus the test we need to apply when asking ourselves whether we should bring theory

[11] One has to be slightly more precise on the definitions here. I usually *define* framing effects as the behavior modes that disappear as a result of formal modeling, and thus the statement made above is true by definition.

closer to reality or vice versa.

Finally, there is another reason to be concerned with the way we use the term "rationality", and it has to do with rhetoric: when a mode of behavior is dubbed "irrational", our tendency would be to avoid it whenever we make decisions or consult others about theirs. An economist who is asked by a government agency to provide policy recommendations would tend to choose among the modes of behavior that the profession recognizes as "rational". The seemingly-theoretical discussion of the appropriate definition of rationality might therefore have rather practical implications. Note, however, that the definition of rationality suggested here does not purport to determine what is and what is not rational. Rather, it is even more subjective than the standard definition in economics: not only tastes, beliefs, or goals are subjective – even the notion of consistency that choice should satisfy is up to the decision maker to choose.[12]

3 Decision under Uncertainty

3.1 Risk

3.1.1 Classical theory

There are decision problems in which probabilities of all relevant events are "given" or "known", in the sense that they are explicitly stated in the description of the problem, or can be reliably estimated from statistical data and probability calculus. These problems are commonly referred to as situations of "risk". Other decision problems under uncertainty are referred to as situations of "uncertainty" or "ambiguity". While the terms are often used in slightly different ways, we will refer here to "uncertainty" as the larger class, encompassing "risk" and "ambiguity", and the latter will refer to all situations in which probabilities are not given in the description of the problem, and cannot be readily estimated.

[12] For further discussion of this definition and its variants, see Gilboa (1991, 2009, 2015) and Gilboa, Maccheroni, Marinacci, David Schmeidler (2010).

Two classes of examples of risky situations include games of chance and insurance problems, and, indeed, both were prominent in the development of probability theory (see Hacking, 1975, Sheynin, 1977). In games of chance probabilities are presumably given by some underlying symmetry among primitive events – such as equal probabilities for all sides of a die, or all cards in a deck – and these can serve as a basis for mathematical calculations of the probabilities of more complex events. In insurance problems, probabilities are assumed to be approximated by observed empirical frequencies.

The notion of expectation was developed in the mid-17th century, in correspondences between Blaise Pascal, Pierre de Fermat, and Christiaan Huygens. The main motivation for the concept was normative, asking what is the just way to split an amount of a gamble. The next important step in the story was the introduction of the "St. Petersburg Paradox" by Nicolaus Bernoulli in 1713. It is an example of a game of chance in which a person's profit has infinite expectation, while most people are willing to pay only bounded amounts to play it.

The paradox was tremendously important in two ways. First, it was one of the first examples in which a mathematical theory was supposed to describe people's behavior, that is, to serve as a *positive* theory. Like many examples that would follow, the paradox was not run as a lab experiment. Rather, it was a mind-experiment. In this sense, it passed the informal robustness test mentioned above: one need not actually run an experiment to see the point of the example; introspection suffices.[13]

Second, the paradox paved the way to the theory of expected utility, suggested by Daniel Bernoulli in 1738 (and by Gabriel Cramer – see Seidl, 2013). Given the context of its emergence, it is fair to suggest an interpretation of the theory as mostly positive, although it also has normative elements.[14] But

[13] Bernoulli's example, like those of Allais (1953) and Ellsberg (1961), is often referred to as a "paradox" rather than a "counterexample". This is probably related to the fact that these examples exhibited conflicts between presumably-well-understood theories and introspection, rather than experimental findings.

[14] Many of Bernoulli's phrases have a normative tone, discussing what a person will be "ill-advised" to do, and even referring to irrationality. Yet, he also uses descriptive claims such as "Considering the nature of man, it seems to me that the foregoing hypothesis is apt

Bernoulli also provided an explanation for the logic of the theory (based on the decreasing marginal utility of money) and this explanation has a normative flavor. Indeed, from 1738 to this day, expected utility maximization is a prominent theory of choice under risk for positive and normative purposes alike.

There is, apparently, little to say about expected utility theory or about decision theory in general during the following 200 years. This period witnessed considerable progress in statistics and probability theory, as well as in philosophy and psychology, but decision under risk has not received much attention. It was only in the 1940s, when von Neumann and Morgenstern (vNM) published *Theory of Games and Economic Behavior* (1944, 1947), that expected utility theory (EUT) deepened its axiomatic roots and flourished into economic analysis. The axiomatic foundations clearly established EUT as a normative standard, but they also made it the theory of choice for positive economics. (See Moscati, 2016, 2018.)

3.1.2 Problems

It did not take long for problems to arise. Following the axiomatic derivation of EUT by vNM in the second edition of the book (1947), Friedman and Savage (1948) showed the implications of the theory to risk averse and risk loving behavior. That paper already pointed out the problem posed by an individual who simultaneously insures her property and buys lottery tickets. It suggested an inverse-S-shaped utility function in an attempt to explain the phenomenon, but this explanation seemed dubious.[15] Around that time, Preston and Baratta (1948) published results of experiments that suggested that people's behavior can't be explained by a formula that is linear in probabilities. They suggested looking at "psychological probability" as a function of "mathematical probability", and argued that the function is above the diagonal for

to be valid for many people to whom this sort of comparison can be applied". Furthermore, Bernoulli refers to the existing practice of insurance and notes that it is explained by the theory.

[15] Specifically, it implies that a person who becomes very rich would be in the convex part of her function, buying lottery tickets but not insuring her property.

very low probabilities, but then crosses it and remains below the diagonal for most of the $[0, 1]$ range.

Allais (1953) famously attacked vNM's Independence Axiom, presenting an example in which many people tend to violate it. Mongin (2019) pointed out that Allais did not view the paradox merely as a failure of EUT as a positive theory, but that he thought of it as a normative critique as well. Edwards (1954) suggested generalizing EUT (for descriptive purposes) by allowing "probabilities effective for behavior which differ in some way from the objective probabilities, as well as on utilities different from the objective values of the objects involved." (p. 394)

The notion of a utility function also received critical attention. In particular, several lines of research suggested that there might be some special points on the utility scale. In perception theory, Helson (1947, 1964) suggested "adaptation level theory", which held that a person's brain adapts to a level of a stimulus and notices mostly changes relative to this level. The theory was not about decision making, but it raised questions about well-being and, indirectly, also about economic activity (Brickman and Campbell, 1971, Brickman, Coates, and Janoff-Bulman, 1978). Specifically, it suggested that one might not be able to measure utility from material consumption in a way that is independent of past experiences.

Another, unrelated, project was Simon's (1947, 1956, 1957) theory of satisficing. He challenged the view of economic agents as rationally optimizing, and suggested that they are *boundedly rational*, and only "satisfice": as long as performance is satisfactory, the same choice is made, but when the outcome is below a certain standard, people start thinking about alternatives. Thus, Simon explicitly challenged the notion of utility maximization.

Helson and Simon were interested in very different questions: the former studied perception at the sensory level, whereas the latter – decision making, mostly by organizations. But in both theories a special point was marked on a scale: an adaptation level in the case of adaptation level theory (interpreted by Brickman and colleagues as a level of well-being), and an aspiration level in the case of satisficing. Both lines of research cast a doubt on economists' ability to summarize all that matters for behavior in a single function that is defined on material goods, independently of past consumption.

Markowitz (1952) pointed out problems in Friedman and Savage's (1948) EUT explanation of the coexistence of gambling and insurance: the shape of the utility function they proposed predicted behavior patterns that seemed to be in conflict with casual observations. In particular, Markowitz argued that we do not observe middle-income people eager to take fair bets (bets with zero expected value), while we do observe poor people buying lottery tickets and rich people insuring their property. Markowitz argued that people behave differently when the wealth involved is above as compared to below their "customary" wealth and suggested that "except in cases of recent windfall gains and losses, customary wealth equals present wealth." He also reported casual questionnaires showing that people may be risk seeking in the domain of losses.

Despite these early studies, criticizing both the notion of utility and the linearity of the expectation formula (in probabilities), EUT has been the main workhorse of economics, and had practically no competition until the late 1970s. Helson's theory was not about decision making to begin with. Simon's satisficing was all about decisions, but it was probably too remote from EUT to allow for some integration of the two. Markowitz's notion of "customary wealth", as well as the work on psychological probabilities, including Allais's paradox, were largely ignored by economics. The dramatic change occurred with the introduction of *Prospect Theory* (PT) by Kahneman and Tversky (1979). This paper was part of a large and systematic project the authors conducted, challenging almost any assumption of rationality in economics and decision theory. Indeed, the term "Prospect Theory" sometimes refers to the project at large. But in its narrower interpretation it suggested a generalization of EUT under risk that could deal with two main problems: (i) People's behavior in situations of risk does not seem to be captured by models that are linear in probabilities; in particular, small probabilities seem to "matter more". (ii) Amounts of money, or levels of wealth are not perceived as mere bottom-line magnitudes; rather, a person has a *reference point* in mind, distinguishing between gains and losses; importantly, behavior under risk may be different in the gains as compared to the losses domain. Kahneman and Tversky argued that individuals who are risk averse in the domain of gains may be risk seeking in the domain of losses. A possible intuitive explanation of this phenomenon may be that individuals dislike losses, and thus, rather than choosing the certainty of the expected loss, may risk larger losses in the hope of not incurring loss at all.

3.1.3 New models

Prospect Theory thus suggested adding a reference point to the description of an economic problem, and applying a "value" function to the gain or the loss in question. For a typical individual, the value function could be concave in the domain of gains and convex in the domain of losses (*the reflection effect*). Further, Kahneman and Tversky argued that "losses loom larger than gains", so that the value function would be steeper for losses than it is for gains (exhibiting *loss aversion*).

Kahneman and Tversky (1979) also reported experimental violations of EUT both in the domain of gains and of losses, as in Allais (1953). To capture these in a descriptive model, PT also suggested that probabilities be transformed into "decision weights" in a non-linear way (corresponding to Preston and Baratta's "psychological probability" or Edward's "subjective probability"). The specific formula suggested differed if outcomes were all in the domain of gains or all in the domain of losses, as compared to the mixed case.

One may view EUT as a generalization of expected value maximization which allows for a non-linear (utility) function on monetary outcomes. Thus, a lottery that yields an outcome x_i with probability p_i is not evaluated by $\sum p_i x_i$, but by $\sum p_i u(x_i)$. It seems natural to suggest that, if behavior is non-linear in probabilities, another function be applied to probabilities, so that a lottery is evaluated by $\sum f(p_i) u(x_i)$ – where $f : [0,1] \to [0,1]$ is a non-decreasing function and $f(p_i)$ can be viewed as "psychological probabilities" or "decision weights". However, this definition raises a number of related problems. First, when f is not linear, one has to deal with the case of x_i's that happen to be identical. If, for example, $x_1 = x_2 = \$10$, $x_3 = \$0$ and $p_1 = p_2 = 0.10$, $p_3 = 0.8$, do we evaluate the lottery by

$$f(0.2)\,u(\$10) + f(0.8)\,u(\$0)$$

or by

$$f(0.1)\,u(\$10) + f(0.1)\,u(\$10) + f(0.8)\,u(\$0)$$

?

It makes sense to choose the former, which would indeed result from Kahneman and Tversky's (1979) "editing phase". But if $x_1 = \$10$ and $x_2 = \$(10+\varepsilon)$ one cannot lump the two together, and will have to use the formula

$$f(0.1)\,u(\$10) + f(0.1)\,u(\$10+\varepsilon) + f(0.8)\,u(\$0)$$

This would render the evaluation function discontinuous (relative to ε at zero). Further, one will get violations of first-order stochastic dominance for a small enough ε (positive or negative, depending on whether $2f(0.1)$ is smaller or greater than $f(0.2)$). The problem will not occur only if $f(p+q) = f(p) + f(q)$ for all $p, q > 0$ (with $p + q \leq 1$), which reduces to the case of a linear f, bringing us back to EUT.

A way out of these difficulties is to apply the function f not to the probability of obtaining a particular outcome, but to cumulative probabilities – the probability of obtaining a certain outcome *or more*. This is the idea behind Quiggin (1982), and Yaari (1987) and it is known as *Rank-Dependent (expected) Utility* (RDU), because the probability of an outcome is replaced by some non-linear function thereof, but the latter depends on the rank of the outcome in the lottery. For example, the same probability of 0.1 in the example above would be replaced by $f(0.1)$ if it is associated with the best outcome (say, $10+\varepsilon$ for $\varepsilon > 0$), but it will be replaced by a potentially different number, $[f(0.2) - f(0.1)]$ if the probability is associated with the second-best outcome (and the best one has probability 0.1). This idea was adopted by Tversky and Kahneman (1992), who suggested *Cumulative Prospect Theory* (CPT). As opposed to RDU, CPT also retains the gain/loss asymmetry of the original PT.

PT was based on the notion of a reference point, but it did not suggest an explicit model of its determination. Like the notion of an aspiration level in Simon's satisficing theory, and the applications of Helson's adaptation level to economics, there appears to be an agreement that one's reference point would be affected by one's past experiences, social comparisons, and explicit statements that may generate expectations. There are a few theoretical and experimental studies that address the question (see Kőszegi and Rabin, 2006, and van Osch, van den Hout, and Stiggelbout, 2006) but it does not seem that there is a consensus over a concrete formula for reference point formation.[16]

Many other models for decision under risk have been suggested, most of which with axiomatic derivations. (Among them are Chew, 1983, Dekel, 1986, and Gul, 1991. See Starmer, 2000, for an early survey.) It is probably fair to say that CPT is the leading candidate to generalize EUT as a positive theory of choice under risk. And it is equally fair to say that its status is controversial. In particular, Ert and Erev (2013) questioned the robustness of loss aversion findings. Similarly, Bernheim and Sprenger (2020) argued that there is no evidence for probability "distortions" being rank-dependent, and that it is possible that the original PT explains choice data better than does CPT. This paper was criticized by Wakker (2023), and Bernheim and Sprenger (2023) responded (see also Bernheim, Royer, and Sprenger, 2022).

It seems that there is no consensus regarding the degree to which PT or CPT accurately describe choice under risk. Moreover, no other theory is agreed upon as successful. However, there seems to be a rather wide consensus that the two main ingredients of PT are persistent, robust effects: (i) When people are confronted with stated probabilities they do not always follow EUT's predictions; specifically, small stated probabilities seem to have a larger effect on behavior than the linear formula suggests.[17] (ii) There is a meaningful concept

[16] Helson's original model, limited to perception theory, involved an explicit mathematical formula based on past stimuli. But its application to the measurement of well-being (Brickman and Campbell, 1971, Brickman, Coates, and Janoff-Bulman, 1978) involves more varied and less clearly measurable data.

[17] It is important to emphasize that this consensus is about probabilities that are explicitly stated, and typically presented numerically (as opposed to, say, graphically). Hertwig, Barron, Weber, and Erev (2004) studied choice where probabilities are learnt from experi-

of a reference point, which can affect behavior; the same amounts of money might induce different decisions under risk, depending on whether they are perceived as gains or as losses.

How rational are these effects? Of course, this is a subjective matter and the question is, in principle, an empirical one. As far as I'm concerned, for each of these I can imagine examples in which I am prone to the effect without feeling terribly embarrassed about it. Let us start with small probabilities. I'm willing to confess that, as far as my decision making under risk is concerned, $10^{-9} = 10^{-12}$. I cannot imagine a situation in which I am faced with these stated probabilities and make different decisions. Whether this small probability is attached to a catastrophic or to a highly desirable outcome, whether I will decide to take the risk or not, it seems to me that I will make the same choice whether the probability number is 10^{-9} or 10^{-12}. Of course, this would involve violations of vNM's Independence Axiom (if I behave differently with, say, probabilities of 0.9 and 0.0009). How can I justify such silliness? Well, the best rationalization I have come up with is that a rational agent should always ask herself how reliable the probability number she has been shown is. If we allow for some healthy doubt about the probability estimation procedure, or the reliability of the person reporting it, such noises might make 10^{-12} much closer to 10^{-9} than the $1 : 1,000$ ratio suggests.[18]

Evaluating wealth, or consumption bundles, relative to a reference point need not be irrational either. It seems that the same level of consumption generates a different experience if one expects, or has become adapted to a certain standard. Spending a night at a four-star hotel does not feel the same if one is used to five- vs. three-star hotels. Beyond the aspect of adaptation, which changes the experience of consumption, people might care about drop-

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ence, and they found the opposite effect: an event that occurs rather infrequently may be completely ignored by decision makers.

[18] In a simple model, one might attach probability $(1 - \varepsilon)$ to the stated probability, and probability $\varepsilon > 0$ to some guess, say, a uniform distribution. A more sophisticated model might let ε depend on the estimate, and maybe find 10^{-12} more suspicious than 10^{-9}. Also, we might take into account the fact that for small stated probabilities there is a larger range on the right than on the left in a way that is reminiscent of Gayer (2009) explanation for the overweighting of small probabilities.

ping below an expected level of consumption due to a social aspect: reduced consumption may imply loss of status and social rank. Being the social creatures that we are, one can hardly dismiss such effects as mere irrationalities.

To conclude, EUT suffers from two main criticisms, and PT/CPT were trying to deal with them. No specific mathematical formula is widely accepted as the "best" model for decision under risk. Yet, the two effects – non linearity in probabilities and the overweighting of small stated probabilities, and a reference point that affects economic decisions – seem to be rather robust, and can even be rationalized.

3.2 Ambiguity

3.2.1 Classical theory

Given the history of decision making under uncertainty and of reasoning about it, it is perhaps surprising that probability theory was only developed in the mid-17th century. A common explanation, at least as far as Europe is concerned, is that probability only emerged when people attempted to deal with uncertainty themselves, rather than by appealing to a deity.[19] Interestingly, some of the major developments of the field were nevertheless related to theological issues.

Whereas Pascal was originally interested in probability for games of chance and gambling, he was also a pioneer in using the theory for problems in which objective probabilities did not exist, and might be meaningless. In his famous "wager", Pascal discusses the choice of becoming a believer. In this short text, he introduces five different ideas of decision theory: the decision matrix, dominant strategies, expected utility, subjective probabilities, and multiple probabilities (again, see Hacking, 1975). The main argument is about "betting at all odds" that God exists, because the infinite payoff of the afterlife (of a believer) overwhelms any finite payoff that awaits her on earth (if she chooses to relish the life of a non-believer). The argument invites the reader to quantify the belief that God exists by a probability number, which evidently can't have

[19] See Hacking (1975) and Connor (2006).

a frequency interpretation. Pascal therefore suggests that the machinery of probability theory, developed by him and others for games of chance, will be used to sort out our intuition about a decision problem under uncertainty. Note that the argument is normative in nature.

In the mid-18th century Thomas Bayes (1763) addressed the question of faith again.[20] According to McGrayne (2011), Bayes's argument was very much like the "watchmaker argument" in philosophy, or the "intelligent design" argument: the world we observe is rather amazing, and it seems more likely that it was created by the Almighty than that it was generated by a sequence of coincidences. In other words, the conditional probability of the world (W) given God (G) is $P(W|G) = 1$ and the probability of the world given that God does not exist ($\neg G$) is very small. But in order to figure out $P(G|W)$ from these conditionals, Bayes needed a prior probability. He used $p(G) = 0.5$. Whether we find this prior convincing may be worth debating. However, in the process of the argument Bayes invented conditional probability, and the commitment – to which we refer as "Bayesian" – to quantify any uncertainty by subjective probabilities.[21]

The debate, whether any uncertainty can be quantified by subjective probability, or whether this is the only rational way to deal with uncertainty, starts more or less with the emergence of probability. An important argument against the universal appeal of the Bayesian approach dates back to Peirce (1878) at the latest: the amount of information one bases one's judgment on does not play a role in the resulting prior probability. A 50%-50% probability estimate that is based on a shrug of one's shoulders is the same 50%-50% estimate that is based on relative frequencies. Despite these occasional philosophical

[20] As opposed to Pascal, however, who was trying to convince his readers that they should try to become believers, Bayes played the more common game of trying to prove that God exists. See Connor (2006) who credits Pascal with dealing with a more modern question, allowing the human reader to take centerstage.

[21] It is worth noting that this reasoning seems to be a good model of the way miracles are supposed to convince us of the existence of God. Note that, in order to explain how miracles work, we need to ascribe to the observers of a miracle some prior beliefs. If, for instance, they were choosing a maximum-likelihood theory, the miracle would not have to involve very extraordinary events.

discussions, the Bayesian approach as well as decision theory received scarce attention in the 19th century. The debate was revived in the beginning of the 20th century, with Knight (1921) and Keynes (1921, 1937) arguing against the universal appeal of the Bayesian approach, and Ramsey (1931) and de Finetti (1931, 1937) arguing for it. The argument of the latter was inspired by logical positivism, and suggested that the meaning of "subjective probability" be related to one's willingness to bet. Rational decision making in the context of betting implied that one behaves *as if* one had a probability over the unknown states. By contrast, Knight and Keynes appealed to intuition, trying to suggest that, when one "simply doesn't know", the Bayesian model is too strict to capture this state of ignorance.

And then Savage came on stage. Based on ideas of de Finetti and vNM, Savage (1954) offered an awe-inspiring theorem showing that very few, seemingly indisputable axioms on coherence of choice can only be satisfied if the decision maker behaves in accordance with maximization of expected utility, for some utility function with respect to some probability measure (Subjective Expected Utility Theory, SEUT). Part of the rhetorical strength (and mathematical difficulty) of Savage's theorem lies in that his model assumes no numbers or linear spaces as primitives. Both mathematical entities – utility and probability – are derived from preferences over functions that map an abstract set of states to an abstract set of consequences. Moreover, the axioms imposed seem extremely elegant and compelling, and the fact that they do not involve any algebraic (let alone topological) structure enhances their intuitive appeal. Anscombe and Aumann (1963) provided a conceptually-similar result, in a model that assumed objective probabilities as given, and derived subjective probabilities from choice under uncertainty. Their axioms seem to be only a minor addition to those of vNM's for choice under risk, applied to more complex objects, namely, functions from states of the world to lotteries. These axiomatic results convinced the communities of economists and other social scientists that the only rational way of making decisions under ambiguity is to behave as if one knew the probabilities of the states of the world, that is, to behave in a Bayesian way.

3.2.2 Problems

Ellsberg (1961) suggested two mind experiments that challenged SEUT. Consider the "two-urn" experiment: imagine that you are faced with two urns, with 100 balls in each. In one there is a known proportion: 50 balls are black and 50 are red. In the other, it is known that each ball is black or red, but nothing else is known about the color distribution. You can choose an urn and a color. A ball will be randomly selected from the urn you named, and if it is of the color you picked, you win a prize (and otherwise nothing). Let's assume that you are indifferent between betting on Black or on Red when sampled from the known urn, as well as between the two bets when sampled from the unknown urn. The question is, would you also be indifferent between the bets on the known vs. the unknown urn?

Ellsberg's point was that a non-negligible proportion of decision makers would not be indifferent. Indeed, a very common finding is that many are "ambiguity averse", preferring to bet on the known rather than on the unknown urn. Some are "ambiguity seeking", exhibiting the opposite pattern. Both patterns are incompatible with the Bayesian approach: if one is Bayesian, one's beliefs boil down to a "probability of red" vs. "probability of black", and these two numbers add up to 1 whether the known or the unknown urn is concerned. If, further, the two numbers are equal, they are 0.5 each, in each of the urns. The 50%-50% probabilities that are based on knowledge of the composition of the urn in one case, and on sheer symmetry considerations in the other are indistinguishable. The amount of information that led to these probability assessments has no effect on a Bayesian's decision making.

A practically identical example was presented by Keynes (1921), and similar ones by Peirce (1878). But these were not directly related to decisions and surely could not refer to Savage (1954). By contrast, Ellsberg designed the experiments so as to challenge Savage's theory, and his examples are remarkable in pointing out the axiom that is being questioned here.

Ellsberg was trying to make both a positive and a normative point. However, many theorists are willing to accept ambiguity aversion as a behavioral phenomenon that may have economic implications, but not as a possibility for a mode of rational decision making. I believe that part of the reason is the elegance and symmetry of Ellsberg's examples. Considering the example above, thanks to the symmetry of information about the two colors in each of the urns, we can compare bets with known and unknown probabilities that are, in a sense, equivalent. This, however, comes at a cost: should one be convinced by Savage that one would have liked to satisfy his axioms, there is a natural way to do it in this example: to attach equal probabilities to the two outcomes, based on symmetry alone. But when there is no natural candidate for the probability, the normative appeal of Savage's axioms is questionable.[22]

For example, assume that you see an announcement of a seminar entitled "Arbodytes and Cyclophines". You have never heard these terms before, and you have no idea what they mean, or even to which domain of knowledge they belong. I now ask you what is your probability that all arbodytes are cyclophines. As a Bayesian, you have to have a precise probability number. But the fact is that you don't have the foggiest idea. You don't even know whether the two are enzymes or ancient languages, terms in group theory or in anthropology. Yet, you cannot say, "I simply do not know"; the Bayesian approach necessitates that you know the probabilities. Do you think 63% is a good estimate? or 64%? I've tried this example with some colleagues, and some suggested 50% as an estimate of the probability. But this is hardly satisfactory: if the probability that all arbodytes are cyclophines is 50%, what about the probability that all cyclophines are arbodytes? Or that the two sets are disjoint? Or logically independent? What about meta-arbodytes being a subset of pseudo-cyclophines? It seems to me that the only rational answer is, "I have no idea whatsoever" – a statement that is inexpressible in the Bayesian language.

Arbodytes, cyclophines, and other made-up terms are hardly at the center of any economic question. They are only presented as an example in which one may not have objective probabilities for some events, nor any "natural

[22] Even in the presence of symmetry, one may question the justification of this procedure, often referred to as Laplace's "Principle of Indifference" (or "Principle of Insufficient Reason"). The main point, however, is that in many real-life problems such symmetry does not exist. (See Gilboa, 2009.)

prior" for them. The point is that many meaningful economic questions involve such events. Wars and financial crises may or may not occur. There is a lot of evidence one may rely on in assessing their probabilities, but coming up with a single probability measure over the state space is typically a challenge. Such uncertain situations are almost never identical to past occurrences, and, importantly, they are not causally independent of these past occurrences. The very fact that a financial crisis resulted in a Depression may be the reason that the next one will not. Thus, relative frequencies of past events hardly suggest a natural prior for future ones. Uncertainty may be hard to quantify also at the individual level. In making career choices, one needs to take into account future economic, technological, and geopolitical trends that are hard to predict. Even fields that are closer to the natural or life sciences leave much uncertainty unquantified. For example, the health risks of known vaccines can be accurately estimated. But when a new vaccine is introduced, data are typically lacking. The eruption of new pandemics and the success of R&D efforts may also present challenges to probability estimation. Moreover, even in the case of climate change, where probability estimates are based on physical sciences, they can considerably vary. Heal and Millner (2014) argue that the density of the increase in average temperature cannot be estimated with sufficient accuracy, even if one takes human behavior as given.

To conclude, my personal view is that it is wrong to assume that the only rational way to make decisions is to adopt a prior and follow SEUT. Despite the enticing elegance and conceptual beauty of SEUT, there are problems in which it forces us to make rather arbitrary choices of probabilities. If there is an infinite horizon of learning periods ahead of us, the choice of such a prior may be almost immaterial: as long as the prior is sufficiently open-minded, the underlying process would be learnt. But there are too many problems, ranging from wars to climate change, where we simply don't have the time to learn the underlying process (to the extent that it can be meaningfully defined) – whereas decisions need to be made, at the individual and societal levels. In these cases, I believe that it may be more rational to admit that we do not know the probabilities than to pretend that we do.

3.2.3 New models

Schmeidler (1989) was a pioneer in presenting a general-purpose, axiomatically-based model for decision making under uncertainty that allowed for non-Bayesian beliefs. His intuition was that, should "subjective probability" reflect a person's willingness to bet, it may well behave in a non-additive way: it is possible that the "probability" of the union of two disjoint events differs from the sum of "probabilities" of the two events. Such set functions were referred to by Choquet (1953-4) as *capacities*, and he defined an integral with respect to them. Schmeidler axiomatized *Choquet Expected Utility (CEU)*, generalizing the model of Anscombe and Aumann (1963), replacing probability by capacity, and standard integration – by Choquet integration.[23]

It turns out that, if the capacity in Schmeidler's model happens to be a function of a regular, additive probability (that is, if there exists an additive probability p and an increasing function f such that $v(A) = f(p(A))$ for every event A), Choquet integration is equivalent to RDU relative to the given probability (p). The similarity between RDU and Choquet expected utility theory is highlighted in Wakker (2010). It is important to note, however, that, from the point of view of CEU, this intersection is an extremely special case. It corresponds to *probabilistic sophistication* as defined in Machina and Schmeidler (1992): a mode of behavior that allows us to attribute to the decision maker a regular probability measure, even if she uses this measure in a way that differs from standard expected utility theory.[24] This class of models cannot explain findings as in Ellsberg's experiments. In the two-urn example discussed above, should there be a probability measure that summarizes the decision maker's attitude to the various choices, it would have to assign to a draw from the unknown urn the same 50%-50% distribution it would assign to a draw from the known urn. Considering a real-life example, an RDU decision

[23] Shackle (1955) and Dempster (1967) also suggested representing uncertainty by non-additive set functions. (See also Shafer, 1976.) Schmeidler's contribution was to relate the representation of uncertainty to a theory of decision making, and support it with an axiomatic derivation.

[24] Machina and Schmeidler (1992) axiomatize this type of behavior in a Savage-like model, where the resulting mode of behavior need not be maximization of Choquet expected utility.

maker will have a well-defined probability for the eruption of war, but will make choices (as if) to maximize some non-EU function using this probability. A CEU decision maker, by contrast, will be able to behave as if she doesn't know what the probability is.[25]

Schmeidler's CEU was a path-breaking model that opened the way to many other models of decision making under ambiguity. For example, Gilboa and Schmeidler (1989) axiomatized the maxmin-EU (MEU) model, which allows for a set of probabilities,[26] such that each alternative is evaluated by its worst-case expected utility (ranging over all probabilities in the set).[27] Sarin and Wakker (1998) and Epstein and Schneider (2003) provided an axiomatic basis for a dynamically consistent version of these preferences for a family of ("rectangular") sets of probabilities. Hansen and Sargent (2001) generalized the model by introducing a relative-entropy cost function, and defined the resulting model as *multiplier preferences* (see also Hansen, 2007, and Hansen and Sargent, 2008). Maccheroni, Marinacci, and Rustichini (2006a, 2006b) axiomatized a (yet) more general family of preferences, dubbed "variational preferences" and provided also an axiomatic basis for its recursive version. Strzalecki (2011) axiomatized multiplier preferences. Chateauneuf and Faro (2009) axiomatized another family of preferences, based on "confidence functions", where the cost is multiplicative rather than additive. Gul and Pesendorfer (2015) axiomatized a model in which the decision rule is not based on the worst-case expected utility but on Hurwicz's α-criterion (Hurwicz, 1951) while Chateauneuf, Eichberger, and Grant (2007) offered a combination of this criterion and expected utility. Hurwicz also suggested applying this criterion to the minimal and maximal expected utility values over a given set of priors (see also Luce and Raiffa, 1957), allowing for the special case of Bayesian deci-

[25]Tversky and Kahneman's (1992) CPT, mentioned in the previous section, was stated in a state-space model with no given probabilities, and can thus be viewed as combining Schmeidler's model with prospect theory.

[26]Sets of probabilities naturally appear in non-Bayesian statistics. Indeed, a confidence interval (set) is a set of distributions. See also Hurwicz (1951), Smith (1961), Huber and Strassen (1973), Chateauneuf (1991), and Walley (1991).

[27]See also Delbaen, Eber, and Heath (1999) definition and axiomatization of "coherent measures of risk".

sion making (if the set of priors is a singleton). Ghirardato, Maccheroni, and Marinacci (2004), and Frick, Iijima, and Le Yaouanq (2022) axiomatized this "α-MEU rule".

Models that are based on a minimum operation may seem rather extreme, at the intuitive level. Considering MEU as an example, one may ask, if there is a set of probabilities, each generating an expected utility value for each act, why should the decision maker be as pessimistic as to consider the minimal such value? Does it make sense to behave as if, after one has made one's choice, Nature pounces and selects the worst possible probability for that choice? Isn't it somewhat paranoiac, believing that Nature is out to get us, as it were?

When this criticism was mentioned to me in the early 1990s, my immediate, textbook response was to point at the axioms: after all, this is to a large extent what the axioms are for. If the behavioral implications of the rule were reasonable, one should not overinterpret a particular mathematical representation thereof. Moreover, the MEU theorem only states that there *exists* a set of probabilities with respect to which the decision maker follows the MEU rule, and it is completely silent on the cognitive interpretation of this representation. Importantly, the set of probabilities with respect to which one follows the rule need not coincide with objectively given probabilities. Indeed, Gajdos, Hayashi, Tallon, and Vergnaud (2008) develop a model in which the objectively given probabilities are formally represented, and the decision maker follows MEU with respect to a subset thereof. Interestingly, the related model of Choquet-EU rarely received such criticisms, even in applications where the two models coincided. The reason is, I suspect, that Choquet integrals were (and probably still are) not sufficiently familiar to invite a cognitive interpretation of the representation.

Over the years, however, I found my aforementioned response inadequate: the MEU model became more popular, and it seemed that the intuitive appeal of the representation had some role in that. In particular, in applications it is natural to assume that the set of objectively-given probabilities, or a confidence set obtained by classical statistics estimation is simply the set that the decision maker uses for the MEU rule. If this is the case, there is surely room to consider other models, which would consider the set of probabilities that are presumably "known" to be possible, but use them in a less extreme way (such as the α-MEU rule mentioned above).

Beyond this conceptual issue, models that are based on a minimum operation may generate evaluation functions that are not differentiable. As a result, partly due to considerations of intuitive appeal, and partly to analytical tractability, there has been an interest in models that involve a set of probabilities, as well as some (second-order) beliefs about these. Clearly, beliefs over beliefs induce a Bayesian prior. However, in these models a nonlinear function is introduced, so that the resulting behavior is not equivalent to SEUT with respect to this prior. The most widely used model of this type is the "smooth preference" model of Klibanoff, Marinacci, and Mukerji (2005, 2009). See also a related model by Nau (2001, 2006), and an axiomatization by Seo (2009). These models are reminiscent of Segal (1987, 1990), who dealt with compound lotteries, where decision makers may fail to behave in accordance with Bayesian calculus.

Other generalizations of the SEUT model allowed preferences to be incomplete, based on the intuition that, if one does not know what the probability is, one does not know what is a better choice to make. Conversely, preferences for one act over another would emerge only if the expected utility of the first is higher than of the second for all the probabilities one considers possible. Bewley (2002) axiomatized such preferences. Ok, Ortoleva, and Riella (2012) dealt with preferences that are incomplete due to multiplicity of probabilities or of utilities, and Galaabaatar and Karni (2013) – of both.

For brevity's sake, the formulas of the various models are not presented here. Similarly, I do not survey the various applications of these models. The reader is referred to Gilboa (2004) for an anthology of theoretical and applied papers as well as to Gilboa and Marinacci (2013) for further details and references. Ilut and Schneider (2022) offers a recent and extensive survey of applications, mostly in macroeconomics and finance.

In more applied work, situations in which there are no "given" probabilities are also known as "deep uncertainty" or "severe uncertainty". Researchers in these fields seem to view non-Bayesian approaches as acceptable standards for rational decision making in such situations. See Bloemen, Popper, and Walker (2019) and Andreoletti, Chiffi, and Taebi (2022).

As opposed to the special (though controversial) status of PT/CPT in the literature on decision under risk, the literature on decision under ambiguity does not seem to have any single model as the main candidate for replacing or generalizing SEUT. In fact, I believe that each of the alternative models above has been suggested neither as an accurate description of human behavior, nor as *the* normative standard for decision making. Partly, this may be due to the fact that there are many types of unknown probabilities, as compared to one type of known ones. Be that as it may, it seems that the literature is rather pluralistic, and that it is widely accepted that various models have their advantages and limitations, for positive and normative purposes alike.

4 Methodological Comments

4.1 Axiomatizations and Experiments

An axiomatization of a decision rule is a mathematical result showing that a certain set of assumptions – the axioms – imply the rule. The axioms are expected to be simple and intuitive, so that, even if they are far from being indisputable, they are more convincing than the decision rule they necessitate. Especially when one has a positive theory in mind, the axioms are supposed to be stated in terms of observables, in line with the teachings of logical positivism: theoretical concepts should be defined by observable data. For example, the revealed preference paradigm (Samuelson, 1938) holds that utility maximization means no more than certain regularities of choice.

What is considered observable may be debatable. The claim that certain types of choices are observable is an empirical claim about the work of economists, and about availability of databases. For example, in consumer choice one may assume that the economist can observe choices between any two bundles, or only choices of single bundles picked out of budget sets. Alternatively, one may argue that observable data contain more information, such as the probability of choice for each element out of some subsets of alternatives, etc.

Axiomatizations are often characterization theorems, showing not only that a decision rule is implied by axioms, but also the other way around. Also, these results are expected to be accompanied by uniqueness results, specifying to which degree the mathematical representation of observable data is uniquely identified by these data. Famously, the notion of "ordinal utility" is an example of a uniqueness exercise: it is commonly argued that the utility function in consumer choice is unique only up to monotone transformations.[28]

Axiomatizations are powerful rhetorical tools. Their main contribution to decision theory, and perhaps to social science in general, is in convincing people of a certain decision rule. In the case of a normative theory, the audience are decision makers, whom one attempts to convince to use the rule in question. For example, an axiomatic derivation of EUT can be used to convince a decision maker that she would wish to be an expected utility maximizer, even if she doesn't happen to be one to begin with. In the case of social choice theory or coalitional game theory, the audience is a group of people who need to make a joint decision, and might be convinced that they would like to make it in a given way. Thus, axiomatizations are a basic tool of normative social science.

But why would a social scientist be interested in axiomatizations if her goals are to develop a positive theory? Mathematical properties of a decision rule are nice to know, but they do not affect the degree to which it fits the data. Indeed, most sciences that use mathematical models do not bother to axiomatize them. Why is decision theory different?

I think that the main reason is the following.[29] The question, "Which decision model should we use for our work?" is often asked when the theory cannot be directly tested in the contexts it is designed for. Consider, first,

[28] Personally, I view this as a myth. Ordinality only holds in the standard model in which indifferences are perfectly transitive. This model is highly unrealistic – and this is an empirical claim about the reality of "observable data". See Gilboa (2009) for details and references.

[29] See Gilboa, Postlewaite, Samuelson, and Schmeidler (2019) elaborating on this argument and suggesting some other scenarios in which axiomatizations might be useful.

decision under risk (with given, stated probabilities). In the early days of the theory (1948-1951, see Moscati, 2016, 2018), the vNM axioms were used to convince economists that EUT was the model of choice, and not "just an example". But, as the decades unfolded, the debate about the appropriate theory has become an empirical one. As described above, the validity of CPT as a model of choice under risk is currently debated by studying experimental evidence more than by pondering axioms – which is the "right" way to do science, according to many. When the theory of choice under risk is applied to large amounts of money, however, which cannot be used in experiments, one may need to resort to musing about axioms. In the case of decision under ambiguity the gap between experiments and reality is even wider. An investor in the stock market might be viewed as betting on real-life events, such as the performance of the economy, the price of commodities, etc. Some of these can be used in experiments, and they surely have higher ecological validity than bets on urns and balls. But we typically do not know what state space an investor has in mind, which uncertainties she is aware of, which causal relationships she believes in and so forth. Further, in many cases, such as climate change, we simply cannot observe the state of the world at present, and therefore we cannot condition payoffs in an experiment on such a state. Yet, we are asked to provide a decision model that would be used as a plug-in into a model of financial behavior, well beyond the reach of experiments. In these cases, the type of regularities a model exhibits – namely, the behavioral axioms it satisfies and, ideally, is implied by – may be of great help in judging its reasonableness.

Axiomatic and experimental studies thus interact and complement each other. Axiomatic results suggest models that can be tested by experiments. The latter may challenge the theory and point out problematic axioms. Experiments test theories where possible, while axiomatizations help us generalize them beyond the scope of the lab. However, while both axiomatic and experimental studies are very important, each might be misleading.

A danger posed by axiomatizations is the beauty of abstraction. This can be seen even in the relatively simple case of choice under certainty: it is very elegant to think of consumer choice as a binary relation between bundles, and, within that model, completeness and transitivity are rather compelling axioms. Yet, actual consumer choice is far removed from the neat, abstract model, and, correspondingly, the elegant axioms may not be as convincing in reality as they seem to be in the model. Similarly, Savage's model of choice under ambiguity is extremely elegant, and it is nice to think of any decision problem as a choice among functions from states to outcomes. Within that model, Savage's axioms are eminently reasonable. But in many real-life choice situations, the state space isn't given, and needs to be theoretically constructed. When one constructs the state space, the axioms become much less compelling, and in many important cases also theoretically unobservable. (See Gilboa, Postlewaite, and Schmeidler, 2009, 2012, and Gilboa, Minardi, Samuelson, and Schmeidler, 2020.)

Experiments, on the other hand, also involve risks. As mentioned above, we should be asking questions about experimental findings: both about their replicability and robustness, and about their relevance and ecological validity. In a sense, the former is a more manageable danger: when experiments fail to replicate, one can hope that this will be found out, sooner or later. It is probably true that the field does not sufficiently encourage reproduction studies, but coming up with conclusions that differ qualitatively from those of classical experiments is likely to pay off academically. By contrast, ecological validity is often harder to deal with. There seems to be a lamppost effect, due to which much attention is devoted to setups that can be experimentally tested, in a way that might invite overgeneralizations to domains that are not easily amenable to testing. Harping on my favorite chord, experiments on balls drawn out of urns make one believe that any deviation from Savage's axiom is a matter of irrationality, not to say stupidity. Thinking about wars or personal careers could give a rather different impression. As David Schmeidler used to say, "Real life is not about balls and urns".

I believe that decision theory crucially needs both axiomatizations and experiments, and discarding any of them could lead it astray. At the same time, we need to remind ourselves that the abstraction of axioms and the concreteness of experiments might be misleading.

4.2 The development of the field

Considering the development of decision theory over the past decades, one may identify two interesting, and perhaps predictable phenomena: exhaustion of demand and fragmentation of supply. "Exhaustion of demand" refers to the seemingly obvious fact that economists, as well as other social scientists who wish to use decision theory in their work, can't keep changing their basic models every so often. EUT was used to construct a remarkable body of theoretical and empirical research in economics; and after so many decades, economists are open to ask, "And what would happen to our analysis if the decision model module we use is replaced by another, presumably a more realistic one?" But there seems to be some limit to the appetite for new models, whether for decision under risk, under ambiguity, or for other purposes. One reason might be the field's limited resources, not to say laziness: there is so much research energy out there. Another has to do with academic payoff: using a new decision model in one's work, one faces two basic possibilities: if the results are similar to those obtained with known models, the study will have little impact. If, however, the results are very different from those obtained with several other models, the audience might be rather skeptical.[30] Thus, economics is typically interested in changing some basic assumptions and trying other models, but at some point the demand for new theories is exhausted.

"Fragmentation of supply" refers to the fact that not all decisions need to be modeled by the same tools. There is something beautiful and enticing in the fact that Savage's model can be applied to one's betting and investment behavior, to the choice of a person's career or to a state's foreign policy. Moreover, the notions of utility and probability, alongside the theory of subjective

[30] Further, as mentioned in Gilboa, Postlewaite, Samuelson, and Schmeidler (2022), even in the absence of previous analysis, the audience might follow a Gricean implicature and assume that the new decision model is used because the results could not be obtained with known ones.

expected utility maximization, seem to say something interesting about each of these problems. We should consider ourselves lucky to have such a unified framework for all decisions. But when we attempt to refine the theory, whether for positive or normative purposes, we should not necessarily expect the same model to fit all applications. Thus, we should be willing to accept a state of affairs in which decision theory for, say, portfolio selection might be rather different from decision theory for political conflicts. Indeed, there are major advances in subfields of decision theory. For example, the "revealed preferences" approach as in Afriat (1967) has been generalized and extended – see Chambers and Echenique (2016). Similarly, stochastic choice theory, pioneered by Luce (1959) has seen a revival in recent years – see Strzalecki (forthcoming). In both cases the advances are not supposed to be generalizations or potential alternatives to, say, Savage (1954); rather, they are focused on more specific problems, in these cases, paying more attention to the nature of actually-available data.

Taking the two phenomena together, it seems that the market for decision theory is active, but changing: there are so many answers that can be given to a single question, but there are more questions to ask.[31]

4.3 Theories and effects

The discussion of theories of decision under risk and under ambiguity, as well as many other examples mentioned above, may suggest the general conclusions that there are many robust effects, but fewer robust theories. For example, the effect of reference points seems to be robust, whereas no precise theory of choice under risk seems to withstand experimental challenges. The effect of unknown probabilities, or unquantifiable uncertainty might be persistent, but no theory of ambiguity non-neutrality has been championed as the "correct"

[31] To a large extent, the two phenomena have also characterized the development of game theory. It started as an amazingly general structure that could say something meaningful and insightful about practically all situations of interaction. In a certain period there was a quest for "*the* solution concept", mostly in refinements of Nash equilibrium. It seems that, over the years, economics lost its appetite for new general solution concepts. At the same time, there is a lot of progress in game theory when specified to subdomains.

one. To consider a more extreme example, framing effects are hard to dismiss, but there is hardly any formal theory of framing effects that can be tested and produce robust predictions. (See, however, Rubinstein and Salant, 2008)

There is a sense in which this observation is on the verge of being tautological. If we expect theories to say what will happen, while effects only say "something is going on", there is little doubt that effects are more robust than theories. We can even think of effects as counter-examples to theories. Revisiting the examples above, the effect of reference points can be viewed as a counterexample to the assumption that only the total amount of wealth matters to economic decisions. Framing effects are counterexamples to the (implicit) assumption that the representation of a decision problem is immaterial. If by a "theory" we mean a mathematical equation, we can think of it as reducing dimensionality. An effect can be thought of as adding a dimension, saying that another variable might be at play. Viewed thus, one should be surprised to find social science theories that are robust.

There are applications of decision theory for which one may make do with effects, without insisting on specific mathematical theories that model them. (These would tend to be mostly in category (I) of Table 1) But there are other applications, in which these effects need to be captured by mathematical formulas in order to be used in larger models (as in categories (III, IV)). It seems that a useful guideline would be not to take the theories too seriously, but also not to dismiss the effects too lightly.

4.4 Economics' Preferences

It appears that economic modeling, whether for theoretical or empirical purposes, tends to favor models that are almost-minimal generalizations of classical ones. This rather trivial observation involves two claims: (i) first, the field prefers to use models that generalize classical ones, rather than models that offer completely new approaches; and (ii) second, among the possible generalizations, there is a preference for models that are not *too* general.

Both tendencies are natural, and probably healthy. Generalizations of existing models allow science to make progress without losing insights and findings already obtained. In our context, a theory of decision under uncertainty that does not generalize SEUT could do wonders in explaining some phenomena, but would require going back to the fundamentals and trying to figure out how decades of theoretical and empirical research fit in with the new results. For example, Simon's (1955) "satisficing" behavior is an intuitive and powerful model, which can explain quite naturally a variety of phenomena related to decision making by organizations and individuals. But, should it be adopted as the unifying model of decision making, one would have to devote years of research to understand general equilibrium under uncertainty, information economics, and related subfields – and only then to compare its overall success with that of the dominant theory. Alternatively, one could try to make progress with several competing paradigms. This, however, would require some meta-theory that would say which paradigm should be used when. It is therefore not very surprising that Simon's theory had but a limited impact on economic modeling as compared to EUT.[32]

On the other hand, generalizations of standard theory are expected to be relatively parsimonious, and not to allow for too wide a range of behaviors. When a model captures a specific phenomenon in a succinct way – figuratively, we may think of it as adding a single parameter – economists tend to be interested in it more than if it is "too general" or "can explain everything". Moreover, when a minor tweak of a classical model can explain a phenomenon that is beyond the scope of existing theory, one justifiably feels that the new generalization *explains* the phenomenon.

Thus, the universal preferences for unifying and parsimonious theories suggest that a new decision theory has a relatively high chance of being adopted if it offers but a minor generalization of a classical one. However, this preference for evolution, rather than revolution of economics may come at a cost. In the context of decision under uncertainty, it has been argued that generalizations of classical theories retained the "as if" paradigm and haven't done much in

[32] This is probably the case with any theory that does not readily generalize SEUT, such as Case-Based Decision Theory (Gilboa and Schmeidler, 1995, 2001). The latter is stated in a different language than SEUT. One can embed SEUT in this new language if one insists on doing so, but such an exercise is not a direct, natural generalization.

terms of describing the process of reasoning or the procedures for decision making. (See Simon, 1976, 1986, Rubinstein, 1988, 2003.) Though this may be a very risky proposition for junior scholars, it is possible that the field as a whole could benefit from greater openness to different approaches.[33]

5 Conclusion

The focus of this survey has been decision under uncertainty. There are many other fields in and beyond decision theory that merit similar attention. I think that the questions raised at the outset are generally relevant and, at least in some cases, my personal conclusions extend to those fields as well.

I should apologize again for the subjectivity in selecting topics and theories to be included in this survey. Even if one includes only material that one believes is of interest to the profession at large, this judgment of others' interests remains subjective.

Independently of the choices made in this survey, I believe that there is room for many new decision theories. For positive applications, we should not expect a single theory to fit all. Despite the desire to have a unified theory of the world, we should be willing to accept a state of affairs in which the field is more fragmented, and new theories are more specialized than classical ones. For normative applications the ideal variety of theories might be even larger: in such applications the decision maker should feel comfortable with the decision they make, and for that purpose – also with the model that leads to this decision. As there are many decision makers out there, it would be good to have many decision models to offer them.

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[33] See, for example, Rubinstein (1988), and Brandstaetter, Gigerenzer, and Hertwig (2006). See, however, Moscati (2023) regarding the interpretation of such models.

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