The History of Prospect Theory for Risk and Its Future for Ambiguity: Interactions between Economists and Psychologists

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September, 2025

ABSTRACT. This paper describes the history of prospect theory, focusing on risk, and provides suggestions for future research, there focusing on ambiguity. In particular, this paper shows how the state of the art in these fields could only come about through inputs from both psychologists and economists and from their interactions.

1 Introduction

In this book on prospect theory, good to start with its history, and what can be learnt from it for the present and future. At this time of writing, ambiguity, the handling of unknown probabilities, is a central topic in decision theory. However, several suboptimal developments that occurred in the history of risk theory are now reoccurring for ambiguity theory. This paper describes those developments, aiming to warn readers and avoid repetitions of such suboptimalities as much as possible, where I am more optimistic than the speaker in Figure 1. The main cause of the suboptimalities is lack of communication between empirically and theoretically oriented researchers.

"Those who don't study history are doomed to repeat it. Yet those who *do* study history are doomed to stand by helplessly while everyone else repeats it."

FIGURE 1

This paper focusses on descriptive decision theory, and will not consider normative aspects. I will discuss historical developments of risk theory along two lines.

LINE 1. How in general model risk attitude? To what extent is it through sensitivity towards outcomes (utility), and to what extent though sensitivity towards probabilities (probability weigthting)?

LINE 2. What is the prevailing empirical phenomenon for risk attitudes? In particular, is it universal risk aversion (with noise) or are the violations of risk aversion too prevalent and systematic to ignore?

Specialized readers will already know the answers to the above questions. It is nevertheless informative to learn about the historical developkments that led to our current views. At some stages, insights needed for progression in the field could only come from psychologists, but at other stages insights from economists were needed.

Moscati wrote many works on the general history of risk theory, e.g. Moscati (2023).[1] This paper focuses on prospect theory.

2 Expected utility as normative and descriptive model in economics

Outcomes are real numbers, designating money. *Prospect* are probability distributions over outcomes that take only finitely many values. $P = (p_1 : x_1, \dots, p_n : x_n)$ denotes the prospect assigning probability p_j to x_j for all j. It is implicit that the probabilities are nonnegative and sum to 1. Outcomes α are identified with degenerate prospects $(1 : \alpha)$. By $\succcurlyeq$ we denote a preference relation over prospects. We throughout assume that $\succcurlyeq$ satisfies *completeness* ($P \succcurlyeq Q$ or $Q \succcurlyeq P$ for all prospects) and transitivity. *Risk aversion* means that prospects are preferred less than their expected values, and *risk seeking* that they are preferred more.

Under *expected utility* (*EU*), introduced by Bernoulli (1738), there exists a *utility function* $U : \mathbb{R} \to \mathbb{R}$, continuous and strictly increasing, such that preferences maximize

$$(p_1 : x_1, \dots, p_n : x_n) \mapsto p_1 U(x_1) + \cdots + p_n U(x_n), \tag{1}$$

the *expected utility* of the prospect. Marshall (1890) was the first to essentially prove a classical theorem:

[1] Blaug (1962, 1997) also discussed the history of risk theory (and of many other topics) but missed relevant nuances.

THEOREM 1. Under EU: risk aversion $\Leftrightarrow$ U is concave. □

This result illustrates well how U serves as the subjective index of risk attitude under EU. U has been used to provide indexes of risk aversion, such as $-U''(\alpha)/U'(\alpha)$, or $\alpha(-U''(\alpha)/U'(\alpha))$ on $\mathbb{R}^{++}$, and many results in insurance and other applications have been explained in terms of properties of U.

Friedman & Savage (1948) tried to explain the co-existence of gambling and insurance through utility curvature. Their study became famous because it was the first attempt to seriously use utility curvature to explain refined empirical findings. However, their explanation turned out to be empirically unsatisfactory (Moscati 2016 p. 227).

When the psychologists Preston & Baratta (1948), in the first experiment on risky choices, used probability weighting rather than utility curvature to explain their findings, economists commonly qualified their analysis as faulty and due to lack of understanding. Economists considered it proven mathematically that risk attitude should be modeled through utility curvature in EU for normative purposes. In those days, economists, well aware of empirical deficiencies of normative theories, nevertheless believed that normative theories were best also for descriptive purposes. Irrational behavior was assumed to be too irregular and chaotic to be modeled in any systematic manner. Thus, in an early and brilliant survey of decision under risk, Arrow (1951a p. 406)[2] wrote

> In view of the general tradition of economics, which tends to regard rational behavior as a first approximation to actual, I feel justified in lumping the two classes of theory together.

McQuillin & Sugden (2012) later similarly wrote, in their opening sentence:

> For at least the last three quarters of a century, both descriptive and normative economics have been based on assumptions about individual rationality.

Newton (1687) preceded them with essentially the same idea:

> I can calculate the motion of heavenly bodies, but not the madness of people.

With these ideas, economists were not open to the probability weighting used by Preston & Baratta (1948). Time was not yet ready for prospect theory.

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[2] In the same year Arrow's (1951b) Ph.D. thesis appeared, later Nobel-awarded.

3 Psychologists always liked probability weighting

We continuing on Line 1 of this paper, on a good general way to model risk attitude. The following point can best be understood by novices, and less so by experts. Can the reader remember when for the first time hearing that risk aversion is equivalent to concavity of utility? What did you feel then? I think it was surprise. How can risk attitude, which seems to refer primarily to how one feels about probabilities, be captured by how much happiness one feels from receiving money?? These seem to be entirely different concepts, located in different compartments of our brains (hearts?). Economists have never been bothered by such sentiments. Their main business is to express everything in terms of money, and to go by revealed preference instead of feelings. Thus, if a revealed preference analysis proves mathematically that risk aversion is captured by how one feels about money, it is business as usual, and no alarm bells go off, where feelings are to be ignored anyhow.

For psychologists, the story is entirely different. Lopes (1987) expressed the sentiments, felt since the 1950s, well: “Risk attitude is more than the psychopysics of money.” Psychophysics is the subfield of psychology that investigates how physical stimuli generate feelings, e.g., how a drop of cold water on our skin generates a perception of coldness. Utility then describes how much happiness an extra euro generates in our heart. From this perspective, it makes more sense to model risk attitude through a model that involves feelings about probabilities, e.g., through probability weighting. A formula immediately suggesting itself is:

$$(p_1\colon x_1, \ldots, p_n\colon x_n) \mapsto w(p_1)U(x_1) + \cdots + w(p_n)U(x_n). \tag{2}$$

Here w is the (*probability*) *weighting function*, which is normalized $\big(w(0) = 0$ and $w(1) = 1\big)$ and strictly increasing. Continuing on Preston & Baratta (1948), modeling risk attitude through probability weighting remained popular in psychology in the decades that followed. Ward Edwards wrote many papers on it, with Edwards (1954) an impressive survey of the economic literature for psychologists. It contained many modern ideas later found back in prospect theory and elsewhere. Amos Tversky was a student of Edwards for a short while and could benefit from Edward’s ideas. Unfortunately, their personal relationship turned bad, maybe because the young brilliant Tversky had not yet learned how to interact optimally with nonbrilliant people.

Psychologists' view, that risk attitude is best captured through a model operating on probabilities, is based on intuitive arguments about psychological processes, and not on revealed preference or formal arguments. Economists have never been very open to such arguments, with the ordinal view explicitly discarding them. I believe that the psychological view was indispensable at this stage for the progress of decision theory. It provided an essential step forward, a step that could only come from psychologists and not from economists. Is must be noted, though, that the theory of probability weighting in Eq. 2, known as *separable probability weighting*, never became very big, for reasons explained later.

4 Is risk aversion universal?

We now turn to Line 2 in this paper, about which risk attitudes are found empirically. Economists commonly assume universal risk aversion, where deviations are taken as nonsystematic noise. Their arguments, all within EU:

(1) Diminishing marginal utility is plausible.
(2) Concave utility is needed for the existence of equilibria.

Ad (1), for your first euro you buy the most useful thing, and for your second euro only the second-most useful thing.

Until the 1980s, economics was an arm-chair discipline, where economists speculated on empirical phenomena but, at least at the micro-level, did not systematically consider data. Systematic violations of risk aversion, as in the gambling industry, were acknowledged but they were considered too minor and unimportant to change theory. Thus, Arrow (1971, p. 90) wrote, about the co-existence of gambling and insurance:

> I will not dwell on this point extensively, emulating rather the preacher, who, expounding a subtle theological point to his congregation, frankly stated:
> "Brethren, here there is a great difficulty; let
> us face it firmly,
> and pass on."

Psychologists will not as easily accept empirical anomalies. However, one shouldn't throw old shoes away before having new ones, and one shouldn't throw away a good theory for one or few anomalies.

5 Economists' inputs into probability weighting

We return to Line 1, on the general modeling of risk attitude. At the end of the 1970s, there was a renewed interest in probability weighting in economics. One reason was that Allais' (1953) ideas, originally written in French, became available in English in Allais (1979), thanks to the endless efforts of Ole Hagen[3]. Those ideas were the focus of early "Foundations of Uncertainty and Risk" (FUR) conferences.[4] There a young, energetic, diplomatic, and brilliant Mark Machina conquered the podium, leading to the classic Machina (1982) and many other papers, and propagating nonexpected utility models. Exceptional was that Machina did not only criticize EU for things to be abandoned and to deviate from, but was constructive in showing that surprisingly much can be retained. However, his concrete tool, local utility functions, have been found to be intractable.

Handa's use of probability weighting functions was another reason for their renewed interest in economics. Whereas papers on separable probability weighting (Eq. 2) and minor varations[5] had as yet appeared in psychological journals, Handa's paper appeared in a leading economic journal. Now, many economists read about probability weighting, be it in Handa's way. I praised psychologists before for valuable intuitions, but theoretical sophistication comes primarily from economists. The moment a classical probability weighting model appeared in an economic journal, dozens of readers noticed that it has a serious theoretical problem: it violates stochastic dominance. This makes the model unacceptable for normative purposes.

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[3] Hagen had been denied tenure because his research interest, nonexpected utility, was not considered to be promising. Then he became more determined than ever to push our field. His patience in encouraging Allais to rewrite his work in English was endless.

[4] This conference was another crucial cornerstone for the birth of behavioral economics. Hagen was soon joined in his efforts by Bertrand Munier here.

[5] Such as normalizing Eq. 2 by dividing by the total weight $w(p_1) + \cdots + w(p_n)$ which, contrary to what has often been believed, does not avoid violations of stochastic dominance.

Also, more subtly, for descriptive purposes. Empirically, violations of stochastic dominance have been found. However, they do not justify any violation of stochastic dominance whatsoever in any descriptive model whatsoever. The empirical violations found are of a very special nature and only descriptive models that capture those special ones are of interest. The great majority of violations of stochastic dominance are empirically unrealistic and remain unacceptable for descriptive models. Although Starmer (1999) reported choices where the violations of Kahneman & Tversky's (1979) (KT79 henceforth) prospect theory are plausible, I continue to believe that also those violations are not plausible in general. Wakker (2023b §2.2) illustrated how unacceptable the violations of stochastic dominace by the separable probability weighting of Eq. 2 and by the probability weighting in KT79 are.

When the Journal of Political Economy published Handa's paper, they received ten submitted comments, each pointing out that Handa's model violates stochastic dominance. The journal only published the comment by the most prominent theoretician, Fishburn (1978). Amongst the comments not published was one by John Quiggin, then an unknown Australian masters' student. KT79, also in a prominent economic journal, also used probability weighting but they were aware of the problematic violations of stochastic dominance.[6] They tried to reduce that problem but could not really solve it.

It is unbelievable that a theoretical problem could go unnoticed in the psychological literature for over 30 years, to be immediately discovered upon first appearance in a prominent economic journal. If this paper were to describe a match between economics and psychology, this would be a point for the economists. They would now lead 1–0.

6 Quiggin's rank dependence

Continuing on Line 1, how to generally model risk attitudes, we have arrived at the 1980s. Psychologists, right so, had pushed the idea that risk attitudes should be modeled through probability weighting. Economists, due to impulses from Allais, Machina, and others, had also gotten interested in relaxing the linear processing of

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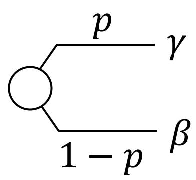
[6] They had first been pointed out to Kahneman by a student, Chew Soo Hong, taking Kahneman's course.

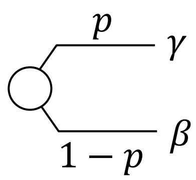
probability in EU. KT79 had just published their pathbreaking new paper. It showed for the first time that, contrary to common belief (§2), it was possible to model irrational behavior in systematic manners using models deviating from rationality, exact and quantitative as needed in economic analyses. Thus, at the early 1980s, whereas it had been widely understood that we need models that treat probabilities nonlinearly, no-one had yet been able to write a mathematically sensible formula for doing so.

Quiggin, the aforementioned unknown Australian master student, was the only one to not only see the mistake by Handa (1977), but also a solution. His basic insight is that not probabilities of separate outcomes, but "cumultive probabilities," or "goodnews probabilities', are the analogs of outcomes. A cumulative probability is the probability of receiving anything better than some outcome. Thus, was Quiggin's insight, one should not transform probabilities of fixed outcomes, but probabilities of receiving anything better than some outcome. Only this way one obtains a natural duality between probabilities and outcomes.[7] Yaari (1987) put this duality central and Wakker (2010, Ch. 5) explained it in detail. Now the decision weight of an outcome is the difference between the weights of two cumulative events, the event of receving the outcome or anything better and the event of only receiving anything better.



$$\rightarrow\ w(p)U(\gamma) + \cancel{w(1-p)}U(\beta)$$

FIG 2a. Traditional, separable, probability weighting



$$\rightarrow\ w(p)U(\gamma) + (1 - w(p))U(\beta)$$

FIG 2b. Quiggin's rank-dependent probability weighting

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[7] Similarly, an amount of $10 dos not only refer to the 10th dollar, but also to the nine dollars below.

For a two-outcome prospect with $\gamma \succ \beta$, Figure 2 displays how the traditional weighting is replaced by Quiggin's rank dependence. Quiggin (1982) gave the extension to general prospects but, for simplicity, I will focus on two outcomes. Some readers may feel that replacing $w(1-p)$ by $1-w(p)$, as in Figure 2, is not the biggest innovation they ever saw. However, they are then wrong. It is a big innovation, and it is an essential cornerstone in the initiation of behavioral economics.

Behavioral economics deviates from classical economics by reckoning with irrationalities. Psychology and marketing had done so long before. What sets behavioral economics apart is that it develops exact quantitative models, as needed in economics. Economists for instance want to know exactly how much demand will increase, and hence supply should increase, after a 2% drop in price. KT79 provided a breakthrough because they did not only understand irrationalities as did their psychological colleagues, but they could also develop formal quantitative models to capture those irrationalities and make exact predictions, thus speaking the language of economists. KT79 can be taken as the birth of behavioral economics, truly connecting psychology with economics. Unfortunately, KT79 could not do a complete job.

KT79 could not provide a completely sound theory of probability weighting and, thus, behavioral economics could not fully take off yet. Only Quiggin made it possible to complete that job. His idea in Fig. 2b, and its extension to general prospects, provided the first real behavioral decision model, providing sound mathematical modeling of irrational behavior. This contribution is indeed an essential cornerstone for behavioral economics. The intuition of probability weighting had to come from psychologists, but its theoretical modeling had to come from economists.

Quiggin's (1982) model first went unnoticed, and he left academia. But time was ready for the idea of rank dependence, and it was independently rediscovered by Allais (1988), by Yaari (1987), and, for the subtler context of uncertainty, by Schmeider (1989), whose first version dates from 1982, with a brilliant modification by his Ph.D. student Gilboa (1987). Remarkably, Weymark (1981 Theorem 3) also provided a rank-dependent model for welfare, but he was not aware of the importance of this model among the many other models in his paper. Other independent discoveries of rank-dependent forms include Höhle (1982), Lopes (1984), and Yager (1988). Remarkably, Anger (1977; Theorem 3; displayed equality in proof]) already axiomatized the Choquet integral in mathematics, preceding and generalizing Schmeidler (1986). When Yaari's and Schmeidler's models were discussed in the mid

1980s, Chew Soo Hong[8] pointed out the precedence of Quiggin. In those pre-internet days, researchers nevertheless succeeded in locating Quiggin in Australia, outside academia, and he received his deserved credit still, after a delay.

Kahneman and Tversky were happy to learn about Quiggin's idea and incorporated it into prospect theory in Tversky & Kahneman (1992), TK92 henceforth. They added their empirical realism, discussed later, to Quiggin's mathematical idea. Whereas the behavioral approach was born in 1979, it reached maturity in 1992.

7 Empirical realism in rank dependence

We now return to Line 2, which risk attitudes are found empirically. As explained before, economists commonly assumed universal risk aversion—and most still do today—with deviations taken as noise. Under EU, it means that utility is concave. In the 1980s, rank-dependent utility was invented by theoretical economists and first studied by them, adhering to universal risk aversion. They, thus, only studied risk-averse probability weighting, which amounts to convex probability weighting.

Empirically, there is so much systematic risk seeking, for low-probability gains for instance, and even more for losses, that it is warranted to model it, and to give up the assumption of universal risk aversion. This empirical realism, present in KT79, was maintained in TK92, who introduced it into rank dependence. It leads to inverse S-shaped probability weighting, interpreted as insensitivity: people do not understand probabilities, and their gradations, well, and insuficiently discriminate.

Another modification in rank dependence is warranted to achieve empirical realism: reference/sign dependence. Attitudes towards losses are very different than towards gains, with attitudes partly reflected. For empirical realism, we have to bring in reference/sign dependence. Doing so is not routine mathematically, but requires

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[8] Chew (1983) also introduced weighted utility, the second published axiomatized nonEU theory, but the first one well-known. I think that it is less suited than rank-dependent utility because its component deviating from EU operates on outcomes rather than on probabilities, and I will not discuss it further. The other main rival of rank dependence today is Gul's (1991) disappointment aversion theory. Its main problem is that it uses an implicit formula, making it intractable. Further, it satisfies betweenness, as does Chew's weighted utility, but betweenness is empirically problematic (Starmer 2000, §5.1.1).

deep mathematical insights and creativity. TK92 provided those, but their depth here has rarely been recognized. I can recognize it, first-hand even. At the end of the 1980s it was clear that a sign-dependent generalization of rank dependence was called for. I thus once, for myself, wrote such a generalization on a piece of paper, which in fact was exactly the golden formula that TK92 would later introduce. However, when seeing that the decision weights did not add to 1, I erroneously concluded that this formula must violate stochastic dominance. I did not realize that I was having gold in my hand, crumbled the piece of paper and threw it in the trashcan. TK92 saw deeper mathematically and did realize that this was the right formula. Surprisingly, that it does not violate stochastic dominance.

One of the greatest contributions in decision theory is Schmeidler's (1989) invention of Choquet expected utility. For it, he independently invented the Choquet integral, around 1980. Only latter, around 1984, Dellacherie pointed out to Schmeidler that this integral had been known before in mathematics, through Choquet (1954). Similarly, it later became understood that the functional invented by TK92 had been known before in mathematics, through Sipos (1979 §3). The functional was also invented, independently, by Starmer & Sugden (1989) who, remarkably, published this idea only in an Appendix of a conference contribution.[9] From their paper I first learned about the soundness of the functional, and that the formula on my crumbled piece of paper had not been so bad after all. Luce & Fishburn (1991) also, independently, developed this new functional. As time had been ready for rank dependence with several independent discoveries in the early and mid 1980s, it had been ready for its sign-dependent generalization at the end of the 1980s.

It is unfortunate that empirical risk seeking had been ignored in the economic literature for over 30 years in the 1980s (continuing up to today …) . If this paper were to describe a match between economics and psychology, this would be a point for the psychologists. They would now level the score: 1−1.

[9] Some of the papers cited here assumed the same weighting function for gains and for losses, and some did not. It is at will in all these papers.

8 Synthesis for risk

The behavioral approach became mature by TK92. It was the first theory that combined empirical realism, allowing for irrationalities, with mathematical soundness, giving the quantitative predictions that economics needs. I share Tversky's (personal communication) preference that the new theory of 1992 now be called prospect theory (PT). The original theory of 1979 can be called original prospect theory (OPT).[10] The theory has since remained the leading empirical theory for decision under risk and has been used in numerous applications. When authors propose new models for decision under risk, they will typically compare their model with prospect theory, arguing for better performance, while ignoring the other models.[11] In sports terms, prospect theory is the guy to beat. An exception is Erev et al. (2017), whose exemplary prediction competitions give fair chances to all theories. These competitions, as do many critics of prospect theory, are limited in only consider data fitting and not conceptual plausibility.

Behavioral models have now been developed in many fields other than risk, on intertemporal choice, welfare, game theory, and so on. Many economic theories are being reconsidered today using the new behavioral theories.

9 The future of ambiguity: how history repeats itself

Aydogan & Gutierrez (2026) discuss the future of prospect theory for ambiguity in detail. This section presents some brief comments.

Keynes (1921) emphasized that in many situations of uncertainty we do not know probabilities, especially in economics. Knight (1921) did so too, but the quantity and quality of his writing on ambiguity are lower than of Keynes'. I regret that the field

[10] I think that OPT is of no more interest, using an unsound formula that cannot be extended to prospects with many outcomes in any good manner. Thus, Wakker (2023b §2.2) showed that the only possible extension to many outcomes (Wakker 2023a) is unsatisfactory. We better use the most efficient (nontechnical) term PT for the only theory of interest, instead of the currently popular but technical term cumulative prospect theory.

[11] See, for instance, Bernheim & Sprenger (2020), Birnbaum (2008), Bordalo, Gennaioli, & Shleifer (2012), Erev et al. (2023), Gigerenzer & Goldstein (1996), Hertwig et al. (2004), Loomes & Pogrebna (2014), Lopes & Oden (1999), Oprea (2024), Stewart, Chater, & Brown (2006), and Vieider (2024).

commonly uses his name, rather than Keynes, to refer to ambiguity. Primarily Savage (1954) convinced many, including me, that under ambiguity we should assign our best guesses of probabilities to events, and after proceed as under risk, for normative purposes. Allais (1953) showed that we need more general theories for empirical purposes, and according to some also for normative purposes. Contrary to common thinking, Allais' common consequence paradox (more fundamental than his common ratio paradox), signaling the certainty effect, is as relevant for uncertainty and ambiguity as it is for risk (Wakker 2010 p. 134). Ellsberg (1961) added further arguments against expected utility that, however, only pertain to uncertainty and ambiguity and not to risk. Ellsberg's paradox is more fundamental than Allais' in the sense that it reveals problems for the very concept of probability.

The most popular ambiguity theory today is the smooth model (Klibanoff, Marinacci, & Mukerji 2005). Here, ambiguity attitudes are captured by a function φ operating on (the utility images of) outcomes. If φ is concave on the utility image of the outcome interval considered, then ambiguity aversion is implied, and if φ is convex there then ambiguity seeking is implied. However, I think that ambiguity primarily concerns the events considered and not the outcomes, and that theories using functions operating on events will work better. I presented the corresponding argument for risk, where it was put forward by psychologists including Lola Lopes, in preceding sections. Epstein (1999 p. 594)[12] and Machina (2009 p. 390)[13] stated this argument for ambiguity.

Admittedly, deviations from EU under ambiguity will depend on the outcomes considered, similarly as risk attitudes will. As everthing depends on everything. Our models have to be pragmatic and tractable though, and we can only incorporate the main dependencies. Allowing dependence on both events and outcomes is too general, predicting not much more than transitivity. Hence it is better to choose one. Dependency of risk attitudes on probabilities, and of ambiguity attitudes on events, is more important than their dependency on outcomes. Let us capture those primary

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[12] "The intuition is that uncertainty or ambiguity stems from events and that aversion to uncertainty reflects attitudes towards changes in those events."

[13] "If there is a general lesson to be learned from Ellsberg's examples and the examples here, it is that the phenomenon of ambiguity aversion is intrinsically one of nonseparable preferences across mutually exclusive events"

dependencies. For pragmatic reasons, let us ignore secondary dependencies where possible. An immediate and big drawback of outcome-based, but not event-based, models for ambiguity, as for risk, is that they cannot capture the empirically prevailing four-fold pattern.[14] Here risk and ambiguity aversion depend on the likelihood of events. Utility-based models such as Chew's (1983) weighted utility and Cerreia-Vioglio, Dillenberger, & Ortoleva's (2015) cautious utility for risk, and Klibanoff, Marinacci, & Mukerji's (2005) smooth model for ambiguity, cannot accommodate such event-dependent switches and, therefore, they cannot work well empirically. The popularity of the smooth model derives from its tractability, requiring only standard EU calculations and providing a smoothness that is convenient in optimizations.

The disconnect between theoretical and empirical works on risk, leading to an overconcentration on risk aversion in risk theories, is repeating itself for ambiguity at present. Most theoretical and applied studies today merely focus on ambiguity aversion, even though empirical studies find much ambiguity seeking, as much as risk seeking (Trautmann & van de Kuilen 2015). Again, besides the motivational component of aversion/seeking, a cognitive/perceptual component of insensitivity plays an important role, but it is ignored in most theoretical and applied papers. A similar ignorance of insensitivity is still common in many studies on risk attitudes at this moment of writing, unfortunately. For instance, many papers today follow up on Holt & Laury (2002), a paper that put back the clock by 20 years (cf. Farquhar 1984), use the outcome-oriented EU to model risk attitude, then can only measure the motivational aversion/seeking component and not insensitivity, and lack a theory to provide satisfactory quantitative estimations. See the keyword "Prospect theory not cited" in Wakker (2025) for such papers.

My second-most preferred ambiguity model today is the α-maxmin model. Hurwicz (1951 Remark 4) first proposed it for decision making, and Good (1950) did

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[14] For risk, the fourfold pattern gives risk aversion for moderate- and high-probability-gains, risk seeking for low-probability-gains, and these phenomena reflected for losses: risk seeking for moderate- and high-probability-losses and risk aversion for low-probability-losses. Similarly, for ambiguity, the fourfold pattern, documented by Trautmann & van de Kuilen (2015), gives ambiguity aversion for gains of moderate and high likelihood, ambiguity seeking for unlikely gains, ambiguity seeking for losses of moderate and high likelihood, and ambiguity aversion for unlikely losses.

so before for statistical inference, with a valuable discussion in Luce & Raiffa (1957 §13.5). Ghirardato, Maccheroni, & Marinacci (2004) provided a sophisticated analysis; see also Eichberger et al. (2011). The α-maxmin model is event-oriented and can accommodate insensitivity, often interpreted as ambiguity perception and related to the size of the set of priors.

The most important innovation of TK92 has often been overlooked: they extended prospect theory to ambiguity. That is, they did not only incorporate Quiggin's rank dependence to model risk attitudes, but they also incorporated Gilboa's (1987) and Schmeidler's (1989) rank dependence (Choquet expected utility) to model ambiguity attitudes. These models allow for any nonadditive measure on events. However, as has often been pointed out, nonadditive measures are too general for all but the simplest state spaces.[15] Therefore, Abdellaoui et al. (2011) added an assumption of local probabilistic sophistication, leading to what is called source theory (Baillon et al. 2025). This theory essentially is nothing but PT for risk extended to ambiguity. PT's tools of utility, probability weighting, and sign dependence are then used, with two generalizations. First, for events for which no objective probabilities are available, additive subjective probabilities are used, called a-neutral. Second, probability weighting functions can depend on sources of uncertainty. For Ellsberg's two-color urns, probability weighting can be more pessimistic for the unknown urn than for the known urn, capturing ambiguity aversion. An important feature is that source theory does not need the Anscombe-Aumann (1963) framework, avoiding the complications of multistage optimization and the restriction to EU for risk.

[15] See Basu & Echenique (2020), Eichberger, Grant, & Kelsey (2012 p. 241), Grabisch & Labreuche (2008 §2.7 and §7), Ivanov (2011 p. 367), and Tversky & Kahneman 1992 p. 311).. This problem holds even more for multiple prior models (Basu & Echenique 2020), and yet more for the smooth ambiguity model (Epstein 2010 §4). In applications, people then commonly add strong parametric assumptions, often ad hoc, such as some second-order distribution in the smooth model, but then their results are more driven by those parametric assumptions than by the ambiguity model used.

10 Conclusion

In the development of decision theory for risk, there were suboptimalities due to lack of communication between empirical and theoretical specialists. Theoretical papers ignored the empirically found risk seeking and empirical papers used unsound theories. Similar suboptimalites occur today for ambiguity. I hope that the readers won't mind that I end this history of prospect theory with my most-preferred ambiguity theory. That is, unsurprisingly, the theory I work on: source theory. It aims to give a future to prospect theory for ambiguity.

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