The SIPTA Newsletter

Society for Imprecise Probability:
Theories and Applications

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Message from the editor

This issue of the SIPTA newsletter brings you a very special treat: an interview with Prof. Isaac Levi, a true pioneer on many themes of interest to the society. Prof. Levi's answers were so substantial that his interview has been divided in two parts. This issue brings the first part of the interview, where Prof. Levi looks at the genesis of his ideas. The second part of the interview, to appear on the next issue, comments on the reception of these ideas and on open challenges.

This issue also reports on the tutorials, invited talks and papers presented at the Fifth *International Symposium on Imprecise Probability: Theories and Applications (ISIPTA)*. Another happy event to report is the appearance of the society at the Wikipedia, thanks to an initiative by our executive editor, Alessandro Antonucci. And there is also fresh information on the *Workshop on Interval/Probabilistic Uncertainty and Non-Classical Logics*, to happen in March 2008.

Finally, the Software Section describes the essential features of the *Statool* package, a system that manipulates partially specified probability distributions.

As always, if you know of any event or publication that should be of interest to members of the society, please let me know (send a message to fgcozman@usp.br).

Cheers!

Fabio G. Cozman

History section: An Interview with Isaac Levi Part I

This email interview with Prof. Levi is centered on his contributions to imprecise and indeterminate probabilities. We thank Prof. Levi for his willingness to contribute to the newsletter.

Thanks also to Daniel Kikuti for help in formatting the material.

Fabio G. Cozman

You have been discussing indeterminate probabilities and related issues for many years. Could you describe the origin of these ideas — the environment that led to them, and the main influences in your work?

I am fortunate to have been a student of Ernest Nagel at Columbia in the 1950's. Although my doctoral dissertation had little bearing on probability, induction and statistics (it was on the epistemology of Moritz Schlick), Nagel offered courses on probability and induction where he instilled a respectful skepticism regarding the prospects of Bayesianism as an approach either to the foundations of statistics or to scientific methodology. Nagel students learned to sniff out places where some variant of insufficient reason was used to introduce prior probabilities; to appreciate the importance of applications of the calculus of probability where probability is understood to be statistical probability and to explore the extent to which efforts to couch scientific methods in probabilistic terms might succeed.

At least three other Nagel students made contributions to the development of ideas relevant to the formation of my concept of indeterminate probabilities: Patrick Suppes, Henry Kyburg and Frederic Schick.

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Suppes received his doctorate before I began graduate school so he had little direct influence on my intellectual environment during my graduate school days at Columbia. In the late 1960's, I was invited to review a volume edited by Hintikka and Suppes (*Aspects of Inductive Logic*, Amsterdam: North Holland, 1966) that appeared in *The British Journal for the Philosophy of Science* 19 (1968), 73-81. My attention was drawn to second of three interesting papers by Suppes that explored the properties of the statistical syllogism. This paper and the first one confronted the disparities between models of probabilistic learning based on Bayesian updating (conditionalization via Bayes theorem) and experimental data concerning probabilistic learning. Suppes' challenge to the empirical adequacy of Bayesian updating and the threat imposed to the normative adequacy of such updating provoked me to think about how prescriptions about rational belief, probability judgment and value judgment relate to the performances of decision makers. This eventually led me to contrast propositional attitudes as *commitments* with propositional attitudes as *performances* and to my formulating my accounts of rational full belief, probability judgment, value judgment and choice as constraints on commitments.[1]

Both Kyburg and Schick attended graduate school at Columbia during same time that I did.

Schick and I became friends in the 1950's while at graduate school and remained so subsequently. Schick undertook in his dissertation [*Explication and Induction*, Ph.D. dissertation, Columbia University, 1958] to improve the applicability of Carnap's measures of confirmation to hypotheses formulated in natural languages by replacing the notion of the relative logical width of a predicate by a range of such widths and, in that way, utilize Carnaps procedures to define "spans of confirmation" that were essentially sets of carnapian confirmation functions. Such sets are "intervalist" in the sense that they are the largest sets of such confirmation functions that satisfy the interval - valued constraints derived from the interval - valued specification of relative width. In this he resembled authors like I.J.Good (1962), B.O. Koopman (1940), C.A.B. Smith (1961), P.Suppes (1974), A.P. Dempster (1967) and K.Weichselberger (2001). Even though none of these authors seemed to go beyond sets of probabilities that are the largest sets enveloped by upper and lower probabilities, taking these authors seriously led me to ask whether differences between sets that share the same intervalist envelope might not make a difference to applications of sets of probabilities as representations of uncertainty in rational decision making and in statistical inference.

I recall seeing Kyburg around the Philosophy Department occasionally during the 1950's but did not become acquainted with him or his work until the early 1970's after both of us had obtained our Ph.D.'s. Our contacts in 1960's marked the beginning of what became a lifelong intellectual and personal friendship. Kyburg had developed a conception of interval valued epistemic probability as part of an effort to elaborate a replacement for strict Bayesian inverse inference. Judgments of epistemic probability (corresponding roughly to what I call judgments of credal probability) were to be derived from information about frequencies via direct inference according to rules for selecting reference classes that legitimated Fisher's fiducial inferences at least in cases of estimating one dimensional statistical parameters (Kyburg, 1961.)

When I took my first teaching job at Western Reserve University (later called Case Western Reserve University) in 1957, I had not as yet focused my research on problems pertaining to probability and scientific inference. My interests changed, however, after attending a lecture to the faculty at the University offered by a Professor of Religion who argued for the historical accuracy of the Old Testament narrative because, according to him, the biblical description of the customs and practices of early Hebrews resembled in a striking manner the practices of the Bedouin in the mid twentieth century. This argument did not go down well with my colleagues, I am pleased to say. Perhaps, so the gentle critical response ran, the biblical narrative is a reliable indicator of some elements of the customs and mores in Asia Minor three millennia (or so) ago. But to conclude that the Biblical narrative is true history is too strong a conclusion to be supported by such data.

At the time, I had been reading *Games and Decisions* by Luce and Raiffa. If the argument were evaluated from the point of view of rational choice, it involved a trade-off between risk of error incurred and the information contained in the conclusion. This is a feature it shares with good inductive arguments. The trouble is that our colleague was too bold. He was prepared to take a risk of importing error into his beliefs that was not sufficiently compensated for by the information promised. More caution needed to be exercised.

I began to pursue this random thought. After a few exploratory efforts and false starts, I proposed a schema for an "epistemic utility function" to express a quest for new valuable error free information. According to the proposal, someone pursuing such a cognitive goal should add to one's initial information the potential answer among those available to the question under study that maximizes *expected epistemic utility*. According to the technical elaboration I favored, the epistemic utility is represented as a weighted average of the utility of truth and the value of the information afforded by a potential answer. The degree of caution (or boldness) of the inquirer is characterized in terms of the relative importance attached to the concern to avoid error and the interest in acquiring valuable information. Elaborating on this idea

[1] See Levi, 1980, 1.5 and elsewhere; 1991, ch.2 and especially 2.1, 2.6-2.8; 1997, chapters 1 and 2; 2004, 121-34.

eventually led to my publishing *Gambling with Truth* with Random House in 1967. (A paperback edition was issued by MIT Press in 1973.) An important modification of the proposal made in that book was published in "Information and Inference," *Synthese* 17 (1967), 369-71 (reprinted in *Decisions and Revisions*, Cambridge University Press, ch.5).

In chapter III of *Gambling with Truth*, I acknowledged the limited applicability of expected utility theory as an account of rational decision making because of the need to appeal to numerically determinate credal probability and utility judgment. The pangs of conscience I had in modeling my decision theoretic approach to inductive inference on a decision theory that I did not strictly speaking endorse was a strong part of my motivation for introducing indeterminate probabilities. In 1967, I did not have an alternative approach. I used expected utility theory in spite of its limitations because I considered the case for using the theory in contexts where assigning determinate probabilities and utilities was not a problem to be compelling. I hoped that exploring this special case could shed some preliminary light on the more general case where probabilities and utilities are no longer determinate. I have continued to think that this is true. This conviction shaped the manner in which I approached indeterminacy when I finally came to consider it systematically.

Why did I think that indeterminacy in probability and utility judgment was often rational and that we needed an account of rational probability judgment, utility judgment and choice that systematically acknowledged this? Thanks to Nagel and Sidney Hook, who taught me political philosophy when I was an undergraduate at Washington Square College of NYU in the late 1940s, I had picked up the message of Peirce's essay "The Fixation of Belief" according to which inquiry is prompted by "real and living" and not manufactured or "paper" doubts and terminates with the removal of such doubt. This message framed the problem addressed in *Gambling with Truth*. It also set the stage for my interest in indeterminate probability.

Inquiry involves efforts to change the inquirer's point of view. At the initial stage of inquiry, the inquirer is committed to a stock of full beliefs that enable him to distinguish between propositions he judges to be "serious possibilities" (i.e., consistent with his state of full belief) and propositions ruled out as impossible even though they may be logically consistent. The agent also distinguishes between the serious possibilities with respect to how probable they are. Full beliefs are absolutely certain and, for this reason, carry probability 1. But other propositions judged to be possibly false also carry probability 1. One cannot define the full beliefs in terms of probability. The initial full beliefs constitute the initial evidence and knowledge available to the inquirer. Certainly true as the inquirer ought to take them to be, they are subject to critical review and may be struck from the list to become possibly false and doubtful. And as these changes take place, so too do changes in judgments of credal or belief or personal probability.

I also thought, as had Keynes (1921), Jeffreys (1939), Carnap (1952, 1962), Kyburg (1961) and many others, that credal probability judgments are constrained by the inquirer's evidence and background information. The constraints were often thought to be determined by a misleadingly called "logical" probability function. The evidence might change over time in the course of inquiry. Hence, this allegedly logical probability could be represented by a function from potential states of evidence, knowledge or, as I now prefer *full belief* to candidate numerically determinate credal states. Jeffreys' probabilities and Carnap's confirmation functions were supposed to characterize such functions.

What seemed objectionable about these logical probabilities is their status as "logical". Functions of the type $C : K \rightarrow B$ from potential states of full belief in K to credal states or states of credal probability judgment in B could be constructed in abundance. Are principles of probability logic available that restrict our choice of C to some unique standard logical probability? Ramsey (1950), De Finetti (1972) and Savage (1954) were right to dismiss this bit of science fiction.

I do not mean to suggest that we cannot countenance principles of probability logic. I think we can. But they allow for many logically coherent "credibility functions" from potential states of full belief to numerically determinate credal states. And this means that we should allow these credibility functions to change in the course of inquiry. And if they can change, there should be some way to allow for holding several credibility functions in suspense in another credibility function — just as distinct states of full belief can be held in suspense in another state of full belief.

Peirce and Dewey urged a focus on the topic of doubt and belief management. Philosophers should turn their backs on the topic of justifying (and explaining) current beliefs and focus instead on conditions under which they are justified in acquiring new beliefs (removing doubt) or giving up old beliefs (coming to doubt). This shift in problematic raised questions of a similar nature concerning what is probable and what is valuable. I came to consider the question of indeterminate probability by worrying about what it is to come to doubt and to cease to doubt probability judgments while concerned to modify credibility functions or, as I called, them *confirmational commitments*.

Let an agent X's *doxastic point of view* at a given time consist of three elements: a *state of full belief* (or a corpus, standard for serious possibility, or body of evidence) $K_{X,t}$; a *confirmational commitment* (or credibility function) $C_{X,t}$ and a *state of credal probabil-*

ity judgment or credal state $B_{X,,t}$. I assume that the credal state is determined by the state of full belief and confirmational commitments and that the latter pair of components may be varied independently of one another. My interests since the 1970's as reported in "On Indeterminate Probabilities" and *The Enterprise of Knowledge* has been directed to identifying conditions under which inquirers should change states of full belief due to expansion and contraction and conditions under which they should change confirmational commitments. The first topic explains my interest in the work on belief revision pioneered by Alchourrón, Gärdenfors and Makinson (1985). The second motivated my study of indeterminate probability. We are concerned here with indeterminate probability.

Probability Logic[2] as I understand it imposes minimal constraints on the confirmational commitments of rational agents:

> *Confirmational Consistency:*
> If $K_{X.t}$ is consistent, $C_{X,t}(K_{X,t})$ is nonempty. Otherwise $C_{X,t}(K_{X,t})$ is empty.

> *Confirmational Coherence:*
> $C_{X,t}(K_{X,t})$ is a set of conditional probability functions $p(x|y)$ where y is consistent with K, for fixed y $p(x|y)$ is a finitely additive probability on the quotient algebra defined by equivalence given K and where $p(x \wedge y|z) = p(x|y \wedge z)p(y|z)$.

I favor introducing an additional constraint that some serious authors resist (e.g. Suppes, Seidenfeld):

> *Confirmational Convexity:*
> If $p(x|y)$ and $p'(x|y)$ belong to $C_{X,t}(K_{X,t})$, so does $\alpha p(x|y) + (1-\alpha)p'(x|y)$.

Strict Bayesians insist that confirmational commitments should satisfy *confirmational uniqueness* so that $C_{X,t}(K_{X,t})$ is a singleton. Satisfying confirmational uniqueness is one way to satisfy confirmational convexity.

In contrast to R.A. Fisher, H.E.Kyburg and A.P.Dempster, I agree with the classical strict Bayesian view that confirmational commitments should satisfy the following:

> *Confirmational Conditionalization:*
> For any potential state of full belief K, If K^+_e is the expansion of K by adding e and forming the deductive closure, $C(K^+_e)$ should be the conditionalization of $C(K)$. That is to say, if $p(x|y)$ is permissible according to $C(K)$, there is a $p_e(x|y)$ permissible according to K^+_e such that $p_e(x|y) = p(x|y \wedge e)$ and if $p_e(x|y)$ is permissible according to $C(K^+_e)$, there is a $p(x|y)$ permissible probability according to K and C such that $p_e(x|y) = p(x|y \wedge e)$.

Confirmational conditionalization is a constraint on a confirmational commitment at a given time. As long as the confirmational commitment C does not change over time, it provides a prescription for how credal states should change over time whether the change is due to *expanding* $K_{X,t}$ by adding e to form $K^+_{X,t\ e}$ or *contracting* $K^+_{X,t\ e}$ by removing e. In the former case, the change is the familiar process of Bayesian updating involving a shift from a "prior" to a "posterior". I have called it *temporal credal conditionalization*. In the latter case, I call it *inverse temporal credal conditionalization*. One can also construct prescriptions for replacing K with a theory incompatible with it as in the case of AGM revision and other types of transformation as well. I have taken the view that all legitimate changes in state of full belief should be decomposable into sequences of legitimate expansions and contractions (in some order or other). As long as the confirmational commitment remains fixed, the transformation of credal state is determined. However, I contend that confirmational commitments are subject to justifiable alterations.

If confirmational conditionalization is satisfied, then for every consistent expansion LK^+_e of some minimal or weakest state of full belief LK, $C(LK^+_e)$ is uniquely determined by $C(LK)$.[3] If we can contemplate a "veil of ignorance" or conceptually minimal state of full belief UK such that every conceptually accessible state of full belief is an expansion of UK, specifying the credal state relative to $C(UK)$ determines the value of $C(K)$ for every conceptually accessible K. In my view, we need no longer be skeptical of the existence of prior credal probability (relative to UK or LK as the case may be) as long as the confirmational commitment is allowed to take indeterminate values in those cases.[4]

Other principles have been advocated as constraints on confirmational commitments. The best known are:

> *Principles of direct inference* that control the derivation of credal probability judgment from

[2]In Levi, 1980, I called it "inductive logic" as did Carnap.

[3]Let t be a sentential expression of the state of full belief LK. Confirmational conditionalization guarantees that every permissible probability $p_K(x|y)$ in $C(K)$ is determined by a permissible probability $p_{LK}(x|y \wedge t)$ in $C(LK)$. According to the necessitarian approach favored by Carnap and others, there is a uniquely permissible $p_{LK}(x|y \wedge t)$ and this is the logical probability or confirmation function. In the indeterminate case, there will be more than one such "credibility" function. Kyburg (1961) is a necessitarian who does not endorse confirmational conditionalization so that this mode of representation cannot work for him.

[4]I should say that I am content to work with minimal states of full belief relative a given cluster of problems or subject matters rather than a conceptually absolutely minimal state of full belief. If in the course of deliberation, beginning with LK proves unsatisfactory and weakening to a more expressive framework proves desirable, nothing prevents it. By a minimal state of full belief, I mean one that is minimal given the aims of the inquirer.

information about statistical probabilities or chances. Authors like De Finetti and Savage who are skeptical of statistical probability reject such principles. Authors who acknowledge both statistical probability and credal probability (as I do — see *Enterprise of Knowledge*) regard the construction of adequate principles of this sort as important. Venn, Peirce, Fisher, Reichenbach and Kyburg have discussed such principles.[5]

Entropic Principles: that include various forms of insufficient reason that appeal to symmetries to derive priors. Laplace, Jeffreys, Carnap and E.T.Jaynes are well known apologists for such *entropic* constraints.

A *logical confirmational commitment* CIL is the weakest confirmational commitment if it exists satisfying the requirements of a complete probability logic. $CIL(K)$ is a proper subset of $C(K)$ for every C and every K.

When confirmational conditionalization is required, CIL is determined by the set of all probability functions relative to the weakest state of full belief UK (i.e., weakest given the subject under investigation) satisfying the constraints of probability logic for that state of full belief.

Many authors, Carnap among them, once dreamed that a complete probability logic would entail confirmational uniqueness so that numerically determinate probability functions could be used to represent logical probability functions. Even after Carnap abandoned this necessitarian dream, he continued, however, to speak of determinate logical probability or confirmation functions even though it is difficult to understand what this might mean.

Ramsey, de Finetti and Savage thought that a complete probability logic would be restricted to confirmational consistency, coherence uniqueness and confirmational conditionalization. On this personalist view, there could be no *logical* confirmational commitment. Although a weakest confirmational commitment satisfying all the requirements of consistency and coherence is constructible, the requirement of confirmational uniqueness forbids rational agents from adopting it.

The necessitarians maintain that rational agents should endorse CIL and never stray from it. As a consequence, temporal credal conditionalization is entailed for expansions and its inverse for contractions. Some personalists follow the same practice. The necessitarian thinks that the entailment is supported by probability logic. The personalist does not. Yet many of them mimic the practice of necessitarians. They advocate *confirmational tenacity* according to which once one is committed to a numerically determinate confirmational commitment, one is saddled with it forever even though it is not mandated by probability logic. Other personalists allow for changing one numerically determinate confirmational commitment for another but abdicate all pretensions to subjecting such changes to critical scrutiny as long as the requirements of consistency, coherence and uniqueness are met. They are saddled with a form of *confirmational anomy*.

To my way of thinking, neither necessitarianism, personalist tenacity, nor personalist anomy are attractive.

We need accounts of the conditions under which an inquirer is justified in weakening his confirmational commitment C by moving to suspense between C and C' so that the result *adds* permissible probabilities to $C(LK)$. We also need a specification of conditions under which the inquirer is justified in strengthening C by eliminating permissible probabilities from $C(LK)$.

Weakening might sometimes be justified when inquirer X is confronted with Y whose views X respects and who embraces a different confirmational commitment. In such a case X might shift from C_X to C^* that recognizes as permissible all probabilities in $C_X(UK)$ and in $C_Y(UK)$ and if confirmational convexity holds, the convexification of the union of these two sets.

Strengthening might be justified in a context where the inquirer is concerned with inductive expansion and seeks a credal state that is the strongest that begs no questions concerning the issues under study. In *Enterprise of Knowledge* (13.4-13.5) I made a proposal as to how this might be done which (as Teddy Seidenfeld pointed out to me) has the mathematical structure of so called "epsilon contaminated" models for identifying families of priors proposed by Berger and Berliner (*Annals of Statistics* 14 (1986), 461-86).

Once one recognizes the justifiability of weakening confirmational commitments in some contexts, indeterminacy in credal probability judgment should be allowed. Acknowledging indeterminacy is pressed upon us once we recognize that how evidence controls probability judgments according to confirmational commitments is subject to critical review.

According to this *revisionist* view of confirmational commitments, *confirmational inertia* is endorsed. One does not change a confirmational commitment unless there is a good reason to do so. As long occasions arise where weakening is warranted, revisionist perspective provides, in my judgment, powerful support for recognizing the rational coherence of indeterminacy in probability judgment.

I have devoted these remarks to sketching how the

[5] Borel, Kolmogorov, Cramer and Braithwaite consider rules that license judgments of practical certainty or other judgments that are not equivalent to judgments of subjective probability that are derived from information about statistical probability. These may also be considered direct inference. The relation between such direct inferences and those leading to judgments of credal probability call for closer discussion than can be given here. What seems clear is that such principles of direct inference are not principles of probability logic.

concept of indeterminate probability could be used to clarify the relation between logical probability and probability logic and how acknowledgment of indeterminacy becomes relevant to the question of modifying confirmational commitments.

The third topic that can be discussed with profit utilizing notions of indeterminacy in probability judgment is rational decision making.

In my 1974 paper and *Enterprise of Knowledge* in 1980, I defended *E*-admissibility relative to the set of available options and the set of permissible probability distributions over the set of states as a necessary but not a sufficient condition for the admissibility of an option for choice. The decision maker must also invoke secondary criteria for selecting an option from the E-admissible options. Although I originally thought that rationality might entail invoking considerations of security as secondary criteria, I am now inclined to think that any secondary criterion may be invoked — provided that it selects from among the E-admissible options and selects from among these options in accordance with some permissible ranking specified by the secondary criterion.

In the formulations I favored in *Hard Choices* and subsequent discussions, the decision maker's problem is formatted as consisting of (i) a set A of available options and the mixture set $M[A]$ of such options, a set H of states such that the state of full belief K entails that exactly one element of H is true and that each h in H is consistent with K_a^+ for each a asserting that a member of A is implemented; (ii) a set O where $K_{ai\&hj}^+$ entails that exactly one element of O to wit o_{ij} is true for each a_i and h_j. and where refining o_{ij} yields propositions that are equipreferred with o_j. $M[O]$ is the mixture set of the elements of O.

The decision maker's value structure $V(A)$ for A is the set of seriously permissible von Neumann-Morgenstern utility functions defined for elements of A.[6] An option is *V*-admissible if and only if it is optimal according to at least one element of $V(A)$.

The extended value structure for the set $EV(O)$ of potential consequences O consists of all permissible von Neumann-Morgenstern utility functions for elements of O according to the decision maker's goals and values and B is the set of permissible credal distributions over the set H.

Consider the set of probability-utility pairs $B \otimes EV(O)$. $Exp(A)$ is the set of expected utility functions for the set A defined by probability-utility pairs in $B \otimes EV(O)$.

The *Principle of Expected Utility* stipulates that $V(A) \subseteq Exp(A)$. An option is *E*-admissible if it is V-admissible and the principle of expected utility is satisfied.

I proposed E-admissibility as necessary (though not sufficient) for admissibility of an option in A for choice. The decision maker may embrace secondary goals that come into play when primary considerations such as those that determine E-admissibility fail to adjudicate an impasse.

When I initially proposed using E-admissibility, I failed to mention explicitly my intention to endorse the *Cross Product Rule* according to which $V(A) = Exp(A)$. As I just formulated it, the cross product rule entails the principle of expected utility but not conversely. I always intended to endorse the cross product rule but did not appreciate the importance of explicitly mentioning it until confronted with criticisms by Seidenfeld, Kadane and Schervish (ch.1 of Kadane, Schervish and Seidenfeld, 1999).

Seidenfeld, Kadane and Schervish did not question the principle of expected utility. Suppose one is given the probability-utility pairs $< p_1, u_1 >$ and $< p_2, u_2 >$ and the expected utilities e_1 and e_2 determined by them and seeks to identify all utility functions over A that (i) preserve the Pareto agreements between e_1 and e_2 over the set of mixtures of A and (ii) belong to $Exp(A)$ as the principle of expected utility requires. Seidenfeld, Kadane and Schervish show that, in the general case where the pair of probability functions differ and so do the pair of utility functions, $< p_1, u_1 >$ and $< p_2, u_2 >$ are the sole probability utility pairs satisfying (i) and (ii).

Clearly this result clashes with the Cross Product Rule. I continue to endorse the Cross Product Rule and reject the requirement that Pareto Unanimity be satisfied in $V(A)$ as (i) stipulates. I do agree that Pareto unanimity should hold when two probability-utility pairs share one component in common. And this together with the Cross Product Rule insures the convexity of B and $EV(O)$. (Levi, 1990, 1999.)

My disagreement with Seidenfeld, Kadane and Schervish here concerns the scope of the Pareto condition. I think that what should be considered non-controversial should be what is non controversial in probability judgments and the utility judgments of the agent's taken separately. Seidenfeld, Kadane and Schervish think that what is noncontroversial in the value structure should determine how V-admissibility or E-admissibility should be assessed.

Seidenfeld, Kadane and Schervish and I agree concerning the merits of the requirement that E-admissibility should be a necessary condition for rational choice. This is by no means, however, a widely shared view.

Peter Walley (1991) has followed A.K.Sen (1970), Hans Herzberger (1973), Daniel Ellsberg (1954, 2001) and many others in favoring a principle of *maximality*.

Option a is categorically strictly (weakly equi) preferred to option b if and only if every permissible utility in $V(A)$ ranks a better than, at least as good as, as good as b. Let $V^*[V(A)]$ be the set of utility functions that are extensions of the categorical preference relation over the mixture set $M[A]$.

An option is maximal if and only if is there is a

[6] Or the set of permissible weak orderings over the mixture set $M(A)$ satisfying von Neumann-Morgenstern conditions.

utility in the set $V^*[V(A)]$ according to which that option comes out at least as good as any other option in A.

A maximal option is, in effect, one which is not categorically dispreferred to any other available option according to the categorical preference defined by $V(A)$. This is the standard way of characterizing maximality. I have used a more devious formulation in order to see maximality as a variant on V-admissibility.

I assume that any set of utility functions defined over $V(A)$ may be used to define $V^*[V(A)]$. $V(A)$ could violate the expected utility principle. But it need not do so. Thus, $V(A)$ could satisfy the expected utility principle (and, indeed, even the cross product rule) so that $Exp(A) = V(A)$. Advocates of maximality might still resist endorsing E-admissibility.

One can adopt maximality as a necessary but not sufficient condition for admissibility just as I did for E-admissibility. Ellsberg, Gärdenfors and Sahlin, and Walley, in effect, proceed in this fashion. For these authors, the prescription to maximize minimum expectations can be seen as the application of a secondary criterion to the set of maximal options determined by a value structure $V(A)$ satisfying the expected utility principle.

Herzberger (1973) and Sen (1970) appear to favor maximality as a necessary and sufficient condition for the rational admissibility of the choice of an option.

Seidenfeld, Kadane and Schervish and I agree in endorsing E-admissibility over maximality. The main argument for this preference is that maximality is insensitive the difference between cases of three way choice where ranking A over B over C is supported by some permissible utilities and C over B over A by others. In some cases, B is closer to the worst option according to both rankings (the second worst case) and others it is not. B is not E-admissible in second worst cases but it is maximal. In other cases, all three options are both E-admissible and maximal. E-admissibility should be recommended over maximality because of its greater sensitivity in these cases.

Seidenfeld, Kadane and Schervish who have tended to favor E-admissibility have, not been so sympathetic to confirmational convexity. They have as a consequence qualified their endorsement of the analysis of the second best case. Whereas I contend that B should be forbidden in all such cases, they think sometimes it may be permitted.

I began to put my ideas together around the time that I returned to Columbia as a member of the senior faculty in 1970. As good fortune would have it, one of my first graduate students at Columbia was Teddy Seidenfeld. Seidenfeld was not only endowed with enormous philosophical intelligence but an excellent mathematical training. Seidenfeld was present during the most of the 1970's when I was working up the ideas that first led to the publication in 1974 of "On Indeterminate Probabilities," *Journal of Philosophy*, 71 (1974) 391-418 reprinted with some modifications in *The Covenant of Reason*, Cambridge University Press (1997), ch.6 and eventually to *The Enterprise of Knowledge*, MIT Press, 1980. Seidenfeld was more than present. He participated in the process that led to the development of my ideas.

My friend Howard Stein followed me to Columbia a few years later and also provided invaluable critical advice.

In 1986, I published *Hard Choices*, where I emphasized the topic of indeterminate utility and cognate issues that arise in social choice theory - explaining the relations between my approach and the ideas of A.K.Sen (1970). Since that time, I have published three books that have focused in the topic of theory change, conditionals and cognate topics. I have, however, engaged in several discussions of indeterminacy in probability and utility - largely in response to serious criticisms from Teddy Seidenfeld and his colleagues J.Kadane and M.Schervish at CMU. Although unconvinced by the amendments Seidenfeld has suggested to the approach I favor, the cogency of the critiques advanced by the Carnegie Mellon trio have led me to make amendments of my own — amendments that improve (so I think) the approach I advocate

In 1952, I.J. Good had suggested that options in a decision problem relative to a given set of available options should be Bayes solutions against "reasonable" probabilities. As I understand him, the reasonable probabilities should be those that could be elicited by invoking secondary, tertiary, etc. criteria. In 1974 and subsequently, I pointed out that in spite of the formal similarities between Good's proposal and mine, his approach is consonant with adherence to a strict Bayesian view according to which rational agents should be committed to credal states where exactly one distribution is permissible. Such agents can still be unsure as to what that distribution might be and represent their uncertainty by a set of seriously possible hypotheses as to what that uniquely permissible probability might be. I called views of this sort, accounts of *imprecise* probabilities. Agents who embrace numerically determinate probabilities may have only an imprecise view of what they are.

I do not deny the phenomenon of imprecision so conceived. Nor do I think that it matters much whether indeterminacy in the sense I favor is called "imprecision" or not. What does matter is that indeterminate (in my sense) probability not be confused with imprecise (in my sense) probability.

Among the differences between the two concepts is the fact that decisions restricted to Bayes solutions among permissible probabilities can violate choice consistency requirements. Decisions based on imprecise probabilities should not do so as long as the methods for making decisions more precise are regarded as cumulative.

From my perspective SIPTA should be the society for indeterminate probability: theory and applications. Ever since the publication of Peter Walley's magisterial work in 1991 on what he called "imprecise" probabilities, my efforts to insist on the terminology I favor have faced an uphill battle to gain a hearing. Since Walley favors maximality over E-admissibility, the formal differences I require in contrasting imprecision with indeterminacy may not seem important. So the controversy has more substance to it than a merely terminological dispute has.

In the next issue, Prof. Levi answers:

How receptive was the philosophical/statistical community to these ideas, when you started advocating them?

You have pioneered work on E-admissibility and on convexity; how did you conceive these two important (and controversial?) ideas, and how did they become central pieces of your thinking?

Could you mention a few open problems and challenges that should receive attention from the community?

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The Fifth International Symposium on Imprecise Probability: Theories and Applications (ISIPTA '07)

The fifth edition of the Society's premier event, ISIPTA, has taken place at the Charles University, in Prague, Czech Republic. The event spanned four days (July 16-19); the first day was dedicated to tutorials and the remaining days were filled with technical presentations and invited talks. A great deal of information about the event can be found at the site www.sipta.org/isipta07.

Talks were delivered at the beatiful building of the Faculty of Mathematics and Physics, walking distance both from the impressive Church of Saint Nicholas and from the extraordinary Charles Bridge. Additionally, participants had the chance to check Prague's visual charm in two special occasions, a welcome party and a banquet, both by the Vltava river. An altogether memorable event.

Four tutorials were delivered on July 16:

- *Risk analysis: rough but ready tools for calculations under variability and uncertainty*, by Scott Ferson (Applied Biomathematics, United States).

- *An introduction to the theory of coherent lower previsions*, by Enrique Miranda (Universidad Rey Juan Carlos, Spain).

- *Generalized information theory*, by George Klir (Binghamton University, United States).

- *Decision theories for imprecise preferences and imprecise probabilities*, by Teddy Seidenfeld (Carnegie Mellon University, United States).

The conference also had two invited talks by prominent researchers:

- *Game-theoretic probability: Theory and applications*, by Glenn Shafer (Rutgers University, United States).

- *In the realm of probability: Limits to standard probability*, by Terry Fine (Cornell University, United States).

Following the format of previous meetings, this edition of ISIPTA had all technical contributions presented orally in a single track, and then later presented as posters. This format allows both a general overview of contributions (in the oral presentation) and a detailed discussion of results (in the poster session). A large variety of issues were discussed at the meeting, dealing both with theory and applications. Technical papers can be downloaded from the conference's web site; here is a list of them:

• *Credal networks for military identification problems*, Alessandro Antonucci, Ralph Bruehlmann, Alberto Piatti, Marco Zaffalon.

• *Uncertainty analysis in food engineering involving imprecision and randomness*, Cedric Baudrit.

• *Predicting the Next Pandemic: An Exercise in Imprecise Hazards*, Mikelis Bickis, Ugis Bickis.

• *Measuring Uncertainty with Imprecision Indices*, Andrew Bronevich, Alexander Lepskiy.

• *Inference in Credal Networks Through Integer Programming*, Cassio Campos, Fabio Cozman.

• *Credal Nets with Probabilities Estimated with an Extreme Imprecise Dirichlet Model*, Andrés Cano, Manuel Gómez, Serafín Moral.

• *Comparative Probability Orders and the Flip Relation*, Marston Conder, Dominic Searles, Arkadii Slinko

• *Multinomial nonparametric predictive inference with sub-categories*, Frank Coolen, Thomas Augustin.

• *Jury size and composition - a predictive approach*, Frank Coolen, Brett Houlding, Steven Parkinson.

• *On various definitions of the variance of a fuzzy random variable*, Ines Couso, Didier Dubois, Susana Montes, Luciano Sanchez.

• *Independence concepts in evidence theory*, Ines Couso.

• *On coherent immediate prediction: Connecting two theories of imprecise probability*, Gert De Cooman, Filip Hermans.

• *Immediate prediction under exchangeability and representation insensitivity*, Gert De Cooman, Enrique Miranda, Erik Quaeghebeur.

• *Constructing Predictive Belief Functions from Continuous Sample Data Using Confidence Bands*, Thierry Denoeux, Astride Aregui.

• *Relating Imprecise Representations of imprecise Probabilities*, Sebastien Destercke, Didier Dubois, Eric

Chojnacki.

• *Coherence and Fuzzy Reasoning*, Serena Doria.

• *Distributions over Expected Utilities in Decision Analysis*, Love Ekenberg, Mats Danielson, Mikael Andersson, Aron Larsson.

• *Multiparameter models: Probability distributions parameterized by random sets*, Thomas Fetz.

• *An extension of chaotic probability models to real-valued variables*, Pablo Fierens.

• *Some results on imprecise conditional prevision assessments*, Angelo Gilio, Veronica Biazzo.

• *Data-Based Decisions under Imprecise Probability and Least Favorable Models*, Robert Hable.

• *Climbing the Hills of Compiled Credal Networks*, Rolf Haenni.

• *Quantile-Filtered Bayesian Learning for the Correlation Class*, Hermann Held.

• *On the Explanatory Power of Indeterminate Probabilities*, Jeffrey Helzner, Horacio Arlo-Costa.

• *Information Processing under Imprecise Risk with the Hurwicz criterion*, Jean-Yves Jaffray, Meglena Jeleva.

• *Compositional Models of Belief Functions*, Radim Jirousek, Jirina Vejnarova, Milan Daniel.

• *Enhancement of Natural Extension*, Igor Kozine, Victor Krymsky.

• *Updating and Testing Beliefs: An Open Version of Bayes' Rule*, Elmar Kriegler.

• *On sigma-additive robust representations of convex risk measures for unbounded financial positions in the presence of uncertainty of the market model*, Volker Kräschmer.

• *Estimating Probability Distributions by Observing Betting Practices*, Caroline Lynch, Don Barry.

• *An independence concept under plausibility function*, Marcello Mastroleo, Barbara Vantaggi.

• *Coherence graphs*, Enrique Miranda, Marco Zaffalon.

• *Entropy minimization and imprecise probabilities*, Robert Nau, Robert Winkler, Victor Richmond Jose.

• *Imprecise probability methods for sensitivity analysis in engineering*, Michael Oberguggenberger, Julian King, Bernhard Schmelzer.

• *Luceno's discretization methods and its application in decision making under ambiguity*, Michael Obermeier, Thomas Augustin

• *Some Bounds for Conditional Lower Previsions*, Renato Pelessoni, Paolo Vicig.

• *Human reasoning with imprecise probabilities: Modus ponens and Denying the antecedent*, Niki Pfeifer, Gernot Kleiter.

• *Learning about a Categorical Latent Variable under Prior Near-Ignorance*, Alberto Piatti, Marco Zaffalon, Fabio Trojani, Hutter Marcus.

• *Conditioning in Chaotic Probabilities Interpreted as a Generalized Markov Chain*, Leandro Rego.

• *Qualitative and Quantitative Reasoning in Hybrid Probabilistic Logic Programs*, Emad Saad.

• *Coherent Choice Functions under Uncertainty*, Teddy Seidenfeld, Mark Schervish, Joseph Kadane.

• *Multilinear and Integer Programming for Markov Decision Processes with Imprecise Probabilities*, Ricardo Shirota Filho, Fabio Cozman, Felipe Trevizan, Cassio Campos, Leliane Barros.

• *Regular finite Markov chains with interval probabilities*, Damjan Skulj.

• *Minimax Regret Treatment Choice with Finite Samples and Missing Outcome Data*, Joerg Stoye.

• *Finite Approximations To Coherent Choice*, Matthias Troffaes.

• *Computing expectations with p-boxes: two views of the same problem*, Lev Utkin, Sebastien Destercke.

• *Linear Regression Analysis under Sets of Conjugate Priors*, Gero Walter, Thomas Augustin, Annette Peters.

• *The Logical Concept of Probability: Foundation and Interpretation*, Kurt Weichselberger.

SIPTA in the Wikipedia

If you type "SIPTA" in the Wikipedia's search engine (at www.wikipedia.org), you will not be disappointed: thanks to Alessandro Antonucci, there is now a page for the society at the world's largest online encyclopedia.

The current SIPTA entry is a preliminary effort; it must receive contributions from the community, as any Wikipedia page. If you have contributions to make, either contact Alessandro, or just log into Wikipedia and do it yourself!

Call for Papers: Int. Workshop on Interval/Probabilistic Uncertainty and Non-Classical Logics

This workshop will be held on March 25-28, 2008, in Japan. The general chair is Yoshiteru Nakamori (JAIST, Japan), and its advisory board is formed by Hiroakira Ono (JAIST, Japan) and Hung T. Nguyen (New Mexico State University, United States).

The goal of the workshop is to provide a forum for research exchanges on the need to combine uncertainty analysis with logics in intelligent systems. Its objectives are outlined as follows:

• to bring together researchers working on uncertainty formalisms in information and knowledge systems;
• to attract researchers working in social sciences (economics, business, and environmental sciences) who are interested in applying uncertainty-related tools and techniques;
• to promote the cross-fertilization between the fundamental ideas connected with various approaches used in the study of non-classical logics;
• to bring together researchers from various fields on non-classical logics and applications in order to foster collaboration and further research;
• to present and discuss open research problems and challenges.

The list of possible topics includes (but is not limited to):
• Interval computations: computational models, algorithms, and complexity;
• Other models of uncertainty in knowledge representation, especially models which combine interval and probabilistic uncertainty, such as Dempster-Shafer theory, rough set theory;
• Integration of pieces of information and knowledge known with interval and probabilistic uncertainty;
• Decision making under interval and probabilistic uncertainty;
• Combining interval and probabilistic uncertainty with logics;
• Applications with interval/probabilistic uncertainty analysis;
• Algebraic logics;
• Fuzzy and many-valued logics;
• Lattices with operators;
• Modal logics;
• Substructural logics;
• Topological semantics of modal logic.

Invited speakers:
G. Bezhanishvili, T. H. Cao, M. Gehrke, J. Harding, V. Kreinovich, J. Lawry, H. T. Nguyen,

Submission of Papers:
Potential authors are invited to submit their full papers by September 15, 2007. The submissions will be refereed by anonymous referees for scientific quality and for relevance for the workshop. Workshop proceedings will be published by Springer-Verlag in the "Advances in Soft Computing" series. Papers should follow the LaTeX series format as described at http://www.springer.com/. Submissions should not exceed 12 pages in the Springer-Verlag format. Please send your papers in PDF via email to Van Nam Huynh at huynh@jaist.ac.jp.

Program Committee:
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Important Dates:
Submission of Paper: September 15, 2007
Notification of Acceptance/Rejection: October 25, 2007
Camera-ready version: November 20, 2007
Workshop: March 25-28, 2008

Further Information:
Registration fees, accommodation, transportation information will be announced later. All inquiries should be directed to Van Nam Huynh at huynh@jaist.ac.jp.

Software section: The *Statool* package

The *Statool* package is a system for arithmetic on partially specified probability distributions. The project leader for this effort is Daniel Berleant; several other researchers have contributed to the project, and today *Statool* offers quite a bit of functionality, plus a detailed user's manual.

The package can be found at

http://ifsc.ualr.edu/jdberleant/statool/index.htm

The user's manual can also be found at this web site, as well as examples and discussion on the subject of p-boxes and interval arithmetic with probabilities. To run *Statool*, you just have to download a compressed file and uncompress it.

The package has been described in the following paper:

D. Berleant, L. Xie, and J. Zhang, Statool: a tool for Distribution Envelope Determination (DEnv), an interval-based algorithm for arithmetic on random variables, *Reliable Computing*, 9(2):91–108, 2003.

Material from the *Statool* user's manual:

Statool is a software tool for obtaining bounds on the distributions of sums, products, and various other functions of random variables. It uses the Distribution Envelop Determination (DEnv) algorithm. *Statool* takes two input random variables as X and Y and the relationship between them as "unknown dependent," "known dependent" or "independent." It then will obtain bounds on the distribution of various functions. There are the basic arithmetic ($+$, $-$, $\times$, $/$), max, min, and relational operators. The package can also handle generic input functions of X and Y.

The software only accepts two operands at a time; to be able to handle more than 2 operands, one can first operate on two of the operands and save the result. Then the result can be used as an operand on the next operation, and so on. Finally, it is also possible to specify dependences among variables through correlation and through bounds on joint distributions. The system emphasizes graphical output: the user can see the result of operations through graphs displaying bounds on distribution functions. Indeed, *Statool* has nice histogram and data editors, where all sorts of graphs can be displayed (and the display can be finely controlled by the user).

Files are a primary way to exchange and save the data in *Statool*. Four kinds of files are used to describe random variables:

- probability distribution files (.PDF): a list of intervals and probabilities.
- cumulative distribution file (.CDF): envelopes for the cumulative distribution.
- intermediate distribution file (.IDF): similar to a .PDF file, except that the intervals can overlap.
- sample data file (.SMP): used to show the probability distribution of sample data.

Only .IDF and .SMP files are used to input or output distributions; .PDF and .CDF files are used internally.

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