

David Schmeidler (1939–2022): The man

M. Ali Khan

David Schmeidler passed away on the night of 17th March, and the author is grateful to be asked to pen a few lines. It is obviously too early to attempt an impersonal obituary of the *oeuvre* that is Schmeidler's bequest to the economics profession, not to mention the author's obvious limitations for such a task. It will take time and extended study.

As one of Schmeidler's students makes clear, the master was not a talker.

He was a man of few words, even more than nowadays. In a working paper he once used no more than five words to describe the whole Savage uncertainty model: "Acts map states to consequences."¹

Not only taciturn, but also without sham or pose or self-advertisement or entrepreneurship, a man absorbed in the problem and the subject at hand, a subject that stands by itself, purified from all attachment to the psycho-sociological here and now. Never one for theatricality and empty sentimentality, I too shall defer to his honoured memory by sticking to facts, and shall further limit myself to a subset of his work even within Walrasian economics itself – to his publications in the four-year period 1969-1973.

It is difficult for those not in my generation to appreciate the verve and dash with which David Schmeidler burst on the mathematical economics scene. I joined graduate school the year he finished his Ph.D. and my teachers impressed on me to keep my eyes on his ascent; by the time I finished, he had published 13 papers, the first being his highly cited debut on the *nucleolus*. The average length of twelve of these papers is under six printed pages, and if one is to include the paper with Debreu, no loquacious talker himself, the number jumps to seven. Two of the (Econometrica) publications are less than two pages: one is a full paper, and the other has no references! The economy of expression, and the catholicity of citations, is fully mirrored in the publications themselves, and space does not all an elaboration of their many subtleties, and Hildenbrand's (2005) testimony must suffice.

I met David Schmeidler by correspondence in 1967. In a letter to Robert Aumann I asked for help on a mathematical problem which I could not solve. A few months later I received a letter from David containing the solution.

¹ Wakker (2020) recounts an episode: "Just tell me the main axiom. I directly wrote my truncation continuity axiom. David looked away for two minutes in silence, then said "OK I see, and ended the meeting. And I knew that he had seen the whole thing. Again, he was a man of few words."

Indeed, the *cognoscenti* are well-aware that one of his more important insights of this period was not published under his name: an alternative proof of Aumann's celebrated 1964 equivalence theorem appeared, with full acknowledgement, in Hildenbrand's 1972 Berkeley survey. And since I am sticking to objective counts, the number of entries in the name index of Hildenbrand's book is itself most revealing: next to Debreu and the author himself, Schmeidler ties with his teacher Aumann to outrank Scarf, Shapley, Arrow and Hahn. The papers of this 4-year period opened many doors for his fellow-workers, and though it may sound strange to say, their full impact has yet to be fully gauged.

There are scholars who do not draw their identity from the discipline they eventually lead, but rather define their subject by their very style and signature.² In giving purpose to my intellectual life projected to the register of Walrasian general equilibrium theory, David Schmeidler belonged to a rather select few. I mourn the loss of his presence, both professional and personal.³

² He once joked to me that there are authors one does not need to read: reading their titles is enough to know the paper. His last email to me on June 10, 2020 said "Very good paper, except the title."

³ An extended version of this obituary is available on the authors' website.

David Schmeidler – Decision Theorist

Mark Machina

David Schmeidler's primary (though not sole) contributions to decision theory were in response to Daniel Ellsberg's (1961) demolition of the classic Savage (1954) theory of subjective expected utility, which represents an individual's risk preferences by expected utility, and (of greater interest to Schmeidler) represents their beliefs under subjective uncertainty by a unique, additive probability measure over subjective events. Ellsberg's simplest counterexample to SEU required nothing more than an individual's typical ranking of bets on a urn with exactly 50:50 red:black balls versus bets on an urn with an unknown proportion of red:black balls.

Schmeidler's (1989) earliest axiomatic generalization/weakening of SEU was to replace its Riemann integral with respect to an additive probability measure with the Choquet integral with respect to a *nonadditive* measure ("capacity"). Such a form retains the classical concepts of a cardinal utility function, the representation of beliefs by a type of 'measure,' and the representation of preferences over acts by a type of 'integral,' yet it is sufficient to accommodate all of Ellsberg's examples. In such an

integral, the weight given to an outcome's associated event is determined by its ranking relative to the act's other outcomes and their own associated events.

Schmeidler's second contribution to modelling non-classical beliefs, joint with Itzak Gilboa (1989) and termed "maxmin expected utility," retains the concepts of expected utility and additive subjective probability, but drops the property that the probability measure is *unique*. Rather, 'beliefs' are represented by a *family* of additive probability measures, where an individual's evaluation of a prospect is determined by its minimum expected utility with respect to this family of measures. As alternative models of Ellsberg's phenomenon of "ambiguity aversion," it is not surprising that the two models are related: conditions on a capacity sufficient to ensure ambiguity aversion imply a special case of maxmin preferences.

A third contribution – more in response to Allais's (1953) attack on expected utility than to Ellsberg's attack on subjective probability – was our own exploration of how Savage's Sure-Thing Principle could be modified so that it still implied classical probabilistic beliefs but did not also imply expected utility risk preferences (1992). I still remember our lying on the grass after a conference, trying (successfully, but only much later) to figure out the replacement axiom.

As colleague, co-author, and even friend, David Schmeidler did not suffer fools lightly. And to demonstrate you were not a fool, he required indirect proof – the assumption was that you *were* one unless and until you could establish a contradiction. Sadly, he passed before most of us were able to complete our proofs.

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