A Personal Tribute to David Schmeidler's Influence

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UN HOMMAGE PERSONNEL À L'INFLUENCE DE DAVID SCHMEIDLER

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This paper describes my relationship with David Schmeidler and how he inspired me. My first meeting with David was on August 29, 1984, in a conference on Operations Research in Osnabrück in Germany. Two positive impressions preceded it. First, my colleague PhD student Jean Derks in Nijmegen had once told me enthusiastically about David's nucleolus cooperative game solution (Schmeidler [1969]). Second, and I could not anticipate how this would change my life, my supervisor and mentor Stef Tijs had given me a copy, that is, a paper copy (of copy of copy of . . . in those days), of Schmeidler [1982], later published as Schmeidler [1986], [1989]. Stef introduced me to David on August 29, 1984, suggesting I might visit David. David suggested that that might be but kept it short. He was a man of few words, even more then than nowadays. He later attended my lecture on a part of my PhD thesis that led to Wakker [1993], and I guessed that he liked it. Then a 6-week visit was arranged, which took place early 1985.

With my Bayesian sympathies I had been learning, the hard way, what a clever and diplomatic *Econometrica* referee stated as follows: "I urge the author to turn his talents toward nonexpected utility." My despair had reached a level by that time making me willing to work on just any nonexpected utility model that would come by. Well, David's it was. I could not know then what I know now: how incredibly lucky I was to run into the biggest and most creative innovator of our field. David's model, nowadays called Choquet expected utility, was nice I thought, although at first I thought it had an unnatural kink that felt ad hoc. When I asked David about it he kept it short, knowing that some insights only come with time. He instead asked if I could do for his model what I had

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done for expected utility in Osnabrück. It led to Wakker [1989], [1994] and further to the biggest stream of works in my career. These works, supported by interactions with David, made me appreciate and understand the depth of his ideas. He had already asked a good PhD student, as he put it, to do such work for Savage's [1954] approach. This is how I met Gilboa, and the mentioned work was Gilboa [1987]. It continues to be one of the strongest papers in our field, one reason being that it does not need any multistage assumptions, which are known to be problematic for nonexpected utility. Abdellaoui and Wakker ([2005], 4) explain that Gilboa's [1987] axioms are intuitive and appealing.

David also took me up on writing Schmeidler and Wakker [1987], using the occasion to teach me the fundamentals of decision theory. Once David entered my office in Tel Aviv, and asked what I was working on that moment. I had been struggling for a day at least to prove some convex inequality. David looked for a minute, then gave me exactly the hint that would lead to the solution, and left, ending this five-minute conversation. I knew what this means in the language of mathematicians. David had given me a probe of his strength. "Message understood."

Later, David asked me how I extended the Choquet integral from simple to general acts. I started writing definitions on the blackboard, but David interrupted me. "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. In a working paper he once used no more than five words to describe the whole Savage uncertainty model: "Acts map states to consequences."

David did not need to read much literature. Whatever was needed to solve a problem, he would invent by himself. He is one of the few human beings whose creative brains are strong enough to bring breakthroughs. I think that this self-invention style is crucial for David's breakthrough contributions. His invention of Choquet expected utility was typical. The problem of Ellsberg's unknown urn had been known long before, as had been the mathematical functional of the Choquet integral. But the probability of someone having these pieces of knowledge at the same time, and understanding that they can be combined, is 0. The invention could only come about by brains strong enough to just break through any wall between them and their aim. David understood the conceptual importance of Ellsberg-type urns by inventing them by himself, and that the Choquet integral could help out, again, by inventing the required mathematics by himself. Only later people told David about Ellsberg [1961]. And only later Claude Dellacherie told David in a conference that his integral had been known before as the Choquet integral (Choquet [1954]). As people told about the related functional by Quiggin [1982]. Yaari [1987] came later.

David's invention of Choquet expected utility has much in common with Nash's [1950] invention of his equilibrium, one of my favorite Nobel prizes. The concept of equilibrium is not far from the related concept in physics, already alluded to by von Neumann and Morgenstern ([1944], 32), and Cournot [1838] had used a special case of it in a situation where needed. The mathematical tool of fixed-point had been available before too. But the probability of someone having these pieces of knowledge, conceptual and mathematical, at the same time, and understanding that they can be combined, is 0. Von Neumann did not make it. The invention could only come about by brains strong enough to just invent

and connect whatever was needed, both conceptually and mathematically, and this is what Nash did. Only later von Neumann would tell Nash that his proof was just an application of the fixed-point theorem.

When I visited David in early 1985, Tzachi Gilboa was his PhD student. In retrospect, we can be happy that David and Tzachi, two such exceptional talents, had come together. With his abundance of creativity, David was not very communicative. Gilboa was his intellectual match, but is all-round in communication and everything. It took the two of them together to impact and even define our field as has now happened.

I admire how David, together with Gilboa, did not just enjoy the fame coming from their ambiguity work (including Gilboa and Schmeidler [1989]), but had the courage to engage in the entirely new case-based decision theory (Gilboa and Schmeidler [2001]). It is something like a Copernican revolution in decision theory, by not letting the action come (only) from changes in available choice options, but, instead, (also) from changes in available information. It leads to a whole new way to learn about human decision-making.

I kept up with David's many inventions throughout my life, taking many inspirations from them again and again. In this respect, I am not the only one! I did have the privilege of communications with David on many occasions. I know now, 35 years later, how incredibly lucky and privileged I have been as a PhD student in 1984 to run into David Schmeidler, and to be able to learn from him, the biggest and most creative innovator of our field.

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