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Editorial

When I was about to move to Erasmus University Rotterdam several years ago, there were two topics of conversation that mentors and colleagues from all over the world would bring up. One of them was EIPE, the Erasmus Institute for Philosophy and Economics, which I was about to join, and its associated Research Master and PhD programme. The other topic was the work of decision theorist Peter Wakker and his research group. Those more interested in philosophy of economics and philosophy of science would bring up EIPE, those more interested in decision theory would bring up Peter Wakker.

Having enjoyed many years of reading The Reasoner, it is wonderful to have the honour of editing this issue. As a philosopher of economics based in Rotterdam, it was the perfect excuse to sit down with Peter Wakker and ask him about his intellectual biography and career, chess, experiments, the Archimedean axiom, and – of course, probability, statistics, and Savage. I hope you enjoy reading the interview as much as I did talking with Peter about these topics.

This issue also kick-starts the column 'What's hot in Economics & Philosophy?', in which I will regularly review some trends in the burgeoning interdisciplinary field between philosophy and economics. This time around, it will focus on some things that have been going on at EIPE in Rotterdam – where the 20th anniversary celebrations of the institute have started in March 2017 with an international conference. The upcoming editions of the column will be a lot less inward-looking, though!

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Features

Interview with Peter Wakker

Peter Wakker is a leading decision theorist, based at the Erasmus School of Economics at Erasmus University Rotterdam, working with a team of behavioural economists, which features, amongst others, Aurelien Baillon, Han Bleichrodt, and Kirsten Rohde: https://www.eur.nl/ese/behec/. Peter Wakker's work comprises many crucial contributions to decision theory and statistics, both concerning theory and empirical work. He is perhaps most widely known for his ap-

plied work in behavioural economics, contributions to medical decision-making, and prospect theory (Wakker, P.P. (2010), "Prospect Theory: For Risk and Ambiguity", CUP). His honours include the Medical Decision Making Career Achievement Award, the Frank P. Ramsey Medal from the INFORMS Decision Analysis Society and an Honorary Doctorate from the University of St. Gallen. His website harbours extensive 'annotated references on decision and uncertainty': https://personal.eur.nl/wakker/. I talked with Peter Wakker in March 2017 and would like to thank Hans Peters and Emanuele Di Francesco for their help in preparing this interview.

Conrad Heilmann: Peter, many thanks for taking the time to talk! Let's start at the beginning. When did you first become interested in research?

Peter Wakker: As a six-year-old, I wanted to be a custom's agent at the border, like my father, and then I wanted to be a veterinarian for a while. Around 12, I started to have this idea that I would become a researcher. I read books by famous scientists...– but I was not thinking about mathematics yet, that would be too abstract for a kid of 12. I was broadly interested in almost all disciplines of science, except economics (laughs)...– It was a close call: if it had not been mathematics, it would have been zoology. All my life I have been interested in that, too, reading books about it. But around age 15, I started to get into logical reasoning and that fascinated me. So then it became clear that mathematics fits me best, but I was in one of the last cohorts that had to take classes in everything. I took Greek and Latin, German, French, English, and all the natural sciences as well. I am really happy about that education. It was good, and there was no economics in there.

CH: You then studied mathematics as an undergraduate?

PW: I started studying a mix of mathematics and physics, but physics is horrible, you have to be in the laboratory...– you really have to work! In mathematics, you just sit in your chair, you have a cup of tea, you think a bit: and there it is. So, I quickly stopped physics, and it became only maths. (laughs) We had a course in quantum mechanics and I liked that and every other course that involved a probability concept. That's how things came to life for me.

CH: What kind of statistics did you study?

PW: When I took courses in statistics, from the beginning, when I heard about classical statistics, I thought: this is just playing with numbers. I was born a Bayesian. At some stage, our teacher told us: 'You cannot assign a probability to life on Mars, because either it exists or it doesn't', and that: 'Frequency is the interpretation of probability', and I immediately thought: 'No, that doesn't make sense!' You have to balance what you know; you have to make a decision. At some stage I talked to the teacher and he said: 'There is a crazy Italian who has such ideas', and he wrote 'de Finetti' on a piece of paper. With that piece of paper I went to the library, and I found his books. Reading them I thought: 'This is what I will work on all my life.'

CH: And then you started to work on a PhD in decision theory?

PW: Well, it was more complicated than that. I really wanted to work on Bayesian decision theory, but nobody wanted to supervise me on this topic. Luckily, people in Leiden still took me in, because of my high grades and because I already had a paper published. So, they accepted me despite my stubborn attitude, but I was mostly on my own trying to find out what to work on. I even independently invented the convexity property for preferences. I have written, hand-written, hundreds of pages on all kinds of properties of convexity...You can find it in every economic textbook, but I didn't have any idea about economics...– At some stage, however, I was getting desperate. Famous professors don't want to discuss the foundations of the field with some unknown PhD student who is not well articulated. So that was a tough time and I thought that I'd probably fail. All my friends were going to conferences and getting published, and I didn't know how to get that done. So, I thought I would probably fail and said to myself: 'Well, then I'd become a teacher for secondary school', and I got the degree for that.

CH: Yes, I saw on your CV that you have a secondary school teaching qualification – so this was really intended as a sort of professional lifeline?

PW: Yes, those were difficult, desperate times. But I was lucky: at a conference, I met Stef Tijs. [Stef Tijs, the 'godfather of game theory in the Netherlands', is amongst other contributions known for his 'τ-value', see Tijs, S.H. (1987), An axiomatization of the τ-value, *Math Soc Sci*, 13:177–81.] He had taught me in mathematics in Nijmegen and he told me: 'Peter, those things that you are interested in, they are happening in economics, you should look into economics journals!' Then I had to turn to economics.

CH: That's quite a story! How, then, did your PhD come along?

PW: Stef Tijs became my supervisor, but he was not a full professor yet, so Pieter Ruys became my supervisor as well. I was doing a bit of game theory with Stef Tijs, and that was very nice. But for me, he was too much of a mathematician, and he didn't see much relevance in other things, so I disagreed with him on that.

CH: Hans Peters told me about very lively discussions in the PhD seminars at the time between all of you...

PW: That's right. But we never clashed; Stef Tijs is a wise person, and he let me do whatever I wanted. He introduced me to David Schmeidler at a conference. I visited him in Tel Aviv for six weeks as a PhD student, and then I worked on his model and did some theorems about that and he liked that. So that was part of my thesis.

CH: Not only in your PhD, but also afterwards, you have

worked on many different things – a lot of experimental work, all the contributions to prospect theory, and more foundational work.

PW: In my heart, it's theory. But I did lots of experimental work. Experiments are good: people who only work with mathematical models have never seen anything empirical, and they do not really understand what they are talking about. So if you also do the empirical work, you will understand what the concept means, and you'll connect to reality. My eight years of applied work in a hospital help a lot with that, too. [Wakker, P.P. (2008), Lessons Learned by (from?) an Economist Working in Medical Decision Making. *Med Decis Making* 28:690–8.] But, also, I was a little bit pragmatic.

CH: Pragmatic – in what way?

PW: Well, if you want to get the attention of people for your ideas, you must show to people that you are worth their attention; you must prove that. And people can only think that if you play their game. I can now work more closely on things that are really my interests, but I have all kinds of ideas about my biggest interest that I've not written yet and maybe I'll do it after some time. Right now, I need all my effort to keep my career, and the people and the group, to get some reputation. So, I need all my effort for that. If you have different ideas, it's not easy to sell them in a way in which the academic world and its rules allow for. So I had to publish papers in the way that journals accept papers. I was writing all kinds of papers that I didn't write with my whole heart.

CH: Looking back, what are the topics that are most 'close to your heart'? Do you also mean non-research things like chess?

PW: When I was around 18, I thought about becoming a professional chess player, but my competitors were always defeating me and I had to stop at age 25. Chess stays with me, though. I daydream of opening moves, and I keep reading and playing games of grandmasters, wanting to understand them. But now, I never play the game myself. Chess overlaps too much with my work. It's the same part of the brain. Playing chess is at the cost of my work. In my spare time, I prefer to do different things, like listening to music and cooking food.

CH: So, your one and only true calling remains Bayesianism?

PW: Indeed, my aim is basically to convert all humankind to become Bayesian: that is the goal of my life! And this includes all statisticians! (laughs) This was from the beginning my big interest and I couldn't believe that people could be doing other things, especially statisticians. But at this moment the behavioural approach is popular, and some people are unfortunately using it normatively. Ambiguity (unknown probability) now is really popular, and people are saying that policy-makers should be ambiguity averse. I do a lot of work on ambiguity, but I don't think it's rational to use any of these models normatively.

CH: There is a rumour that you once refused to co-author a paper if it would not identify Savage's decision theory as the rational standard…

PW: I know what you are referring to. At some stage, I was about to co-author an introduction for a collection in honour of David Schmeidler. But I could never co-author a paper that explicitly says that deviations from Bayesianism can be rational; I will just never do that. And all my co-authors were very eager to write that. So, I told them from the beginning that it was not going to happen; it can happen, but with a footnote that I disagree. I know that as a co-author you need to be a bit flexible, but this has always been – and will always be – my position…

CH: Returning to your research plans, could you say a bit more about those things that you still want to do?

PW: I have in mind a sort of a theory, that would be more of a mathematical and philosophical theory, and that epitomises the normative state of the Bayesian model, and mostly the Sure Thing Principle in Savage's axioms. This has usually been the dividing line between all different kinds of theories and I want to go back to that.

CH: So, that would be a reformulation of Savage?

PW: Yes, but at a fundamental level. Well, in my youth I did some physics that I liked a lot, and the concept of energy in physics is really beautiful. By God, I would love to have invented that concept! Energy has no concrete meaning, like place or time. It is a concept that we constructed ourselves, but it summarises all kinds of things. And then there is conservation of energy. I think in decision theory there is something similar, which I call 'conservation of influence': that's a sort of re-interpretation of preferences in decision making, to make it more natural why all these principles, like the Sure Thing Principle, are good principles. This would involve quite a bit of philosophy: about causation, determinism, and free will. And that should give context to the Bayesian approach. So, you can see, if it is only my intellectual interest, philosophy is close to me and economics is as far as it can be. But my actions are the opposite! (laughs)

CH: Can you say more about the concept of 'influence'?

PW: It governs the decision-making of preference. If I prefer A to B, then my influence is to replace B by A. So I don't say that I prefer A to B, I say that it's my influence that A happens instead of B. And this is the basic start of the concept. It's just a reformulation, but if you use that terminology, all kinds of things change. I have a keyword referring to it in an annotated bibliography on my homepage, so all my fans can follow.

CH: I think I am hardly alone in wanting to see that concept further developed! Would you also relate this to empirical work?

PW: Well, I think that having worked empirically helped me to write in direct and meaningful ways and to notice if I write something that is not clearly verifiable. For me writing a preference axiomatisation and doing an empirical test is about the same. If you know how to measure something, then you

can write preference axioms, and then you also know how to empirically measure them.

CH: Even though there are many axiomatisations that are less well implemented than others…I am thinking of things like the Archimedean axiom.

PW: Indeed, we are in the country of Brouwer, the Dutch mathematician, who influenced me with his constructivism. The Archimedean axiom is not observable, but we sometimes use it because we have the tendency to work with infinite models. And because we work with these models, anytime we work with empirical reality, we are punished by these axioms. We cannot really test continuity. And so we pay a price for this – maybe unfortunate – axiom.

CH: You would like to avoid this?

PW: Yes, definitely! I really like the axiom conditions of finite models, that everything in principle is solvable, and then you discover that getting an EU axiomatisation is much more difficult. The Archimedean axiom makes sense mathematically, but it takes away all kinds of relevant empirical questions. The mathematical question that I would like to solve more than anything else in my life is to have necessary and sufficient preference conditions for expected utility in a finite model. This is very complex, and nobody really knows how to do it. I would like that more than anything else…– So, no Archimedean axiom! (laughs)

CH: That is an excellent closing statement to this fascinating conversation. Thank you very much, Peter!

News

Inferring policy from experiment, 15 May

A mini-conference 'Inferring policy from experiment' was held on 15 May at the University of Kent. The event focused on the epistemic and practical issues in using research evidence in policy-design particularly in the areas of medicine and public health, and featured talks by Nancy Cartwright (Durham & UCSD), Sarah Wieten (Durham), and Mike Kelly (Cambridge).

The opening talk of the event was by Nancy Cartwright, titled 'Two approaches to evidence based health policy—intervention-centred, context-centred'. Both approaches were characterized in terms of their focus questions, target of analysis, and evidential requirements. According to Cartwright's taxonomy, the intervention-centred approach focuses on characteristics of a policy (Does it work? For whom? What does it cost? etc.), studies repeatable causal processes, and requires evidence in support of causal generalizations. By contrast, the context-centred approach focuses on causal arrangements in the target context of a policy, and studies what causal processes these arrangements afford. This requires robust models of how new intervention-outcome pairs can be brought about given those arrangements. As a consequence the evidential requirements for this approach are demanding.

Cartwright proceeded to argue that the intervention-centred approach makes most sense when the intervention has an 'inbuilt' tendency towards the intended effect. Think for example gravity with respect to the effect of making heavy bodies fall. Whether the intervention-centred approach works for health policy depends on whether health interventions typically have such an inbuilt tendency towards effects that a policy-maker is interested in. For example, there is evidence that deworming programs improve children's reading scores, but this effect is hardly due to such an inbuilt tendency of the policy-intervention. Rather, the policy has an immediate effect of killing worms, while the effect on reading scores depends on complicated, context-specific causal pathways. The pure intervention-centred approach is thus risky in situations where one lacks knowledge of appropriate supporting causal mechanisms in the target context. It is these supporting causal mechanisms, the analysis of which is the starting point of the context-centred approach. The downside of the context-centred approach is that it may seem prohibitively demanding – learning the details of all the complicated biological, psychological and social mechanisms relevant for the effects of health policy is next to impossible. However, one may learn reliable markers of relevant mechanisms, as well as *cautions* that signal that a given policy might not work in a particular context. Such markers and cautions are not infallible guides to implementation of a policy, but searching for them offers some leverage for dealing with the uncertainty of the pure intervention-centred approach. From this, Cartwright concluded that no matter which approach one adopts, one should hedge one's bets and plan for failure.

The second talk was by Sarah Wieten. Her talk, titled *What good are pragmatic trials*, offered a critical evaluation of some of the recent arguments in favor of pragmatic trials over explanatory trials. Explanatory trials test an intervention in highly controlled and idealized conditions, thus securing internal validity but arguably compromising external validity as the target population is likely to be dissimilar to the study population. Presumably, pragmatic trials that relax some of the idealized conditions do not suffer from the dissimilarity problem to the same degree. Wieten argued that the arguments in favor of pragmatic trials are, while true in one sense, misplaced with respect to the actual interests of a clinician who will implement the intervention. According to Wieten, the similarity mentioned above is relevant for answering whether the effect of an intervention will be the same in the study and the target populations. But this, according to her, is not the most pressing query from a clinician's point of view. Rather, one needs an answer to the question: what is the causal effect of treatment in the target population given its observed characteristics. For this query it is important just that the target population does not exhibit features not represented in the study population at all – approaching perfect similarity is not automatically a virtue. This requirement can be achieved in highly idealized trials as well as pragmatic ones. Wieten then elaborated this fundamental point to make two further arguments. Firstly, pragmatic trials seem to deliver the claimed extrapolatory benefits only if the transfer of results from the study population to the target is supported by evidence of underlying mechanisms. Secondly, Wieten argued that the assumed tradeoff between internal and