Interdisciplinary Perspectives on Behavioral Economics, 22-23 May

Lisciandra, Chiara

Published in:
The Reasoner

The Reasoner

Volume 11, Number 7
July 2017

thereasoner.org
ISSN 1757-0522

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Editorial

It's an honour to serve as the guest editor of this issue of *The Reasoner* and to share the following interview with Katharine Mach, Director of the Stanford Environmental Assessment Facility, Senior Research Scientist in the Department of Earth Systems Science at Stanford University, and Visiting Investigator at the Carnegie Institute for Science. Mach's work intersects with many topics of interest to readers of *The Reasoner*, including uncertainty representation, evidence aggregation, expert elicitation and deliberation, decision under uncertainty, and communication across various sectors. On top of her day-to-day research, she has been deeply involved in the work of the Intergovernmental Panel on Climate Change as (among other responsibilities) a co-director of science for the Working Group 2 contribution to the Fifth Assessment Report (2014) and co-author of the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. The following are excerpts from a longer interview conducted as part of a series of interviews with researchers and practitioners from a range of fields on topics relating to decision under uncertainty (All interviews will be available soon on the project webpage: `https://desevun.org`). I spoke to Mach on the sidelines of the workshop Coping with Uncertainty: Normative Approaches, Current Practice, in Paris on the 22nd of May, 2017.

Casey Helgeson
GREGHEC (CNRS, HEC-Paris)

Features

Interview with Katharine Mach

Casey Helgeson How would you describe your field of research?

Katharine Mach I'm broadly interested in how knowledge interfaces with decisions, and I would describe my field of research as integrative assessment of climate change risks and options. And as a field, it's inherently interdisciplinary. It's basically saying there are many complex problems relevant to societies in real time where we have lots of different lines of evidence that can be integrated to provide a depiction of the state of knowledge to support choices and actions. Often times

those evaluations of what we know and what we don't are really assisted by bringing experts together with decision makers in the process of assessment. So another way of thinking of assessment is that it's synthesis science supporting solutions, where you have experts interacting with decision makers.

CH: Can you talk about the methods or techniques involved in the type of assessment you do?

KM: So I think of there being four pillars in the space of assessing climate change risks and options that are particularly interesting. And they're especially relevant when you've got contested priorities and deep uncertainties that will be persistent.

Pillar number one is that good climate change assessment – getting at what we know and what we don't – has to integrate different lines of evidence. And that can be incredibly challenging. You can say in a stylised way that it involves integrating results that are quantitative, like numerical projections out of models, with understanding that's a lot more qualitative. So when you have your best available projections of possible futures and then you need to use them to inform decision making, the question becomes 'What about all the processes not in those models?', 'What about the limits?'.

A second priority in assessment is how you apply expert judgement. And there are really fascinating extended disciplines that are devoted to how we can think about judgement – what's objective?, what's subjective?, how can you elicit that person-by-person?, how can you tap group convenings? – but applying rigorous expert judgement is in many ways the challenge and the opportunity of assessment, and is something that happens individually and in groups.

A third big part of climate change assessment is how we unfold possible futures and how they link to our current decisions and actions. The traditional model is predict-then-act: you spell out, for example, a few global-scale futures of what happens if we continue with high emissions of heat-trapping gasses as compared to really ambitiously reigning in those emissions. Those are helpful for organising research communities, but they're not necessarily very effective at getting at what information people need to make good decisions. So you can also tap other scenario approaches – scenarios evaluating extreme possibilities, or scenarios that start with the decision and then find worlds in which goals are met versus missed – and also scenario-based approaches that are much more interactive, not just a scientific projection that is then passed out into the world, but something that brings experts and decision makers together.

The final pillar is really thinking about those expert–decision maker–stakeholder interactions in processes of assessment to build shared knowledge of what questions are relevant to choices about, for example, how we decarbonise our economy, or how we prepare for risks in coastal areas that are flooding, and then in an interactive, iterative way, shape understanding of the state of knowledge towards those decision-relevant questions.

CH: And how does assessment then feed into decision making?

KM: Climate change assessments – whether it's the Intergovernmental Panel on Climate Change (IPCC), national assessments, or even city-scale assessments – are trying to provide the state of knowledge to support complex decisions that will have persistent uncertainties. So then the next question is 'How do you think about the possible influence of those assessments on decision making?'. And it's actually been a somewhat in-depth field of inquiry. About 15 years ago there was a rather big evaluation led out of the Harvard Kennedy school that was trying to understand the influence of environmental assessments to date. And they started with the idea that you would see an assessment of a complex problem, and then there would be a policy in place that hadn't been there before, or some concrete change in behaviour – and that would be the definition of an influential assessment. And what became really clear is that assessments of climate change or other environmental issues where you've got these persistent uncertainties have a much more complex interaction with decision making and society as compared to an 'assessment happens, world changes' type of model. Often times they're really just giving you an understanding, a framing of the issue, so that you can put together the basis of agreed-upon facts. And that can be a major advance of assessment, to say 'Here's the state of knowledge', and then in terms of our options on the table for climate change mitigation, here are the ones that come down to choices around leadership as compared to choices around costs as compared to taking the issue of climate change mitigation and making it into a business opportunity. Or some other decision makers might say 'Well, our entry point is actually going to be more about the co-benefits: the way that climate change mitigation affects everything else that society is trying to achieve'. So I would say that the major advances in this space have been recognising that informing decisions under severe uncertainty is often about separating understanding that's more objective as compared to how that understanding interfaces with different goals so that people can hope to parse the complexities moving forward.

CH: What is the role of stakeholders in assessment?

KM: A good assessment is as much about the process of the assessment as compared to the products. And a good process brings experts in interaction with the decision makers from square one. Rob Lempert this morning spoke about of the importance of convening power. Why are people coming together? Do they have a reason to be interacting around a given decision? So for example, the IPCC has interacted with the UN Framework Convention on Climate Change (UNFCCC) because it is in many ways the ultimate body for coming up with a global treaty to address climate change. There are many other ways that that happens, so in my California context for example, there is very high ambition at the state level of coming up with a strategy for deep decarbonisation and achieving it over the next decade and two. So they're looking to use interactions between experts and decision makers at that state scale to come up with a basis of knowledge upon which they can develop policy. At the very start, you're saying 'Well, what are the rules of this assessment process?', and the process will

be effective if those rules are agreed upon by decision makers coming together with some clear convening power and experts understanding those rules. That interaction between experts and decision makers first of all starts around what questions can be addressed in the assessment process, what information do decision makers need, and how can you think about how that interfaces with different lines of evidence available. Often times decision makers, or stakeholders more broadly defined, will provide inputs on the developing assessment – whether it's a report or a presentation of evidence in different forms – as it's being developed, really indicating 'Is it useful?'. And then at the end, if an assessment has worked well, in many cases the product of an assessment – a report or a data base – is just the beginning, and the assessment becomes the basis for sustained dialogue through time that can inform that process of learning that's inherent to virtually any decision or policy process around climate change risks and options.

CH: What are you working on now?

KM: There are two areas that I'm prioritising in my research on assessment, really trying to advance the science of assessment through trying new things and evaluating them in real time. So one that I've been focusing on very heavily is this integration of quantitative and qualitative evidence. As a specific example there, the project that I'm now launching through an assessment initiative at Stanford is looking at the linkages between climate change and violent conflict. And it's a bit of a challenging field in that if you take more quantitative approaches, for example through statistical approaches drawing from econometrics, you can see in some cases strong linkages between weather variation – a really hot decade, or a really dry year – and an increased likelihood of violent conflict breaking out. In some approaches statistically you can see that linkage, and in others you see that it's not there necessarily at all times. And then if you jump to understanding process through more qualitative or case study-based approaches, putting all of those different lines of evidence together is incredibly challenging because it ranges from 'climate change potentially has a very large effect on outbreaks of violent conflict' all the way through to 'everything else seems to be way more important and it's hard to even point to any impact of climate change on violent conflict'. So integrating quantitative and qualitative evidence in that kind of a circumstance is as hard as assessment gets, and it's a place where I actually think there's big opportunity for advancing our approaches to assessment.

And the second thing that I'll emphasise is related to what needs to advance, from my perspective. I think what often happens in different climate change assessments is you bring experts together, you put them around the table, and you assume that rationality just magically emerges – that you have transparent revealing of top insights in a group process. But actually what happens when you bring experts together is sometimes a magnification of the types of biases we all are prone to in making judgement. Those types of dynamics get amplified in group deliberations so that they often are both overconfident and conservative. And in something like violent conflict where different research approaches give you really different answers, bringing people together right off the bat often leads to a whole lot of disagreement and missed opportunities for using some of the methods that exist to get at individual expert perspectives – so for example through expert elicitation – and then combining that with the benefits that can happen around a more open-ended discussion among groups of experts.

CH: If pressed for general practical advice on how deep or persistent uncertainties should be dealt with in a decision process, what are the most important messages you would want to get across?

KM: I will use two examples of general practical advice, where I've been at this interface of an assessment happening in real time, or just being brought to completion, and there being decisions that are taken.

So one was at the global scale. Following the Fifth Assessment Report of the IPCC that came out in 2014, there was in real time an interaction with the Paris agreement that was being negotiated at that time. And there was a request from the UNFCCC to have what's called a structured expert dialogue, where – it was actually over the course of two years – experts came together with decision makers, where the decision makers were the parties to the international climate negotiations and the experts were largely from the IPCC, but also from other contexts. And the discussion was in essence 'What should the long term global goal be for the Paris agreement?' Should it be staying 'well below 2 degrees celsius', or should adaptation to climate change (preparing for impacts and risks) come into that? How do you think about the architecture in which adaptation and mitigation link together with sustainable development as a whole? And so the key thing there was that it was a dialogue. It was open-ended. It wasn't something where there's an answer provided to decision makers. It was strongly recognised that when Trinidad and Tobago versus Saudi Arabia had questions on the evidence, they wanted to explore 'What does this mean for small island nations that are particularly vulnerable to sea level rise?' as compared to 'How do we think about stranded assets and what that means for fossil fuel exporting nations?'. So that was something where the key aspect was an emphasis on dialogue and there not necessarily being a right scientific answer, but there being different ways that the state of knowledge interfaces with the choices on the table.

One more example that I'll give relates to work we're now doing within the state of California. California has a goal to reduce its emissions by 40% by 2030. This is decarbonisation very similar to what Europe is aiming to achieve as a whole in terms of its pledges put forward under the Paris agreement, and that plays out differently, country by country. It's deep decarbonisation with the emissions dive really starting to happen. And there, it's not only dialogue, it's also figuring out 'How will learning happen through time?'. We can evaluate the risks, we can evaluate our options in responding to them, but as implementation starts to happen and decisions are translated into action on the ground, assessment really has a new mandate to rise up to, which is 'How do you inform processes of learning through time?', recognising that we have no ability to say exactly how prices will drop over a multi-decadal time frame, or which technologies will end up being the winners.

In Defense of Brogaard-Salerno Stricture

Brogaard and Salerno (2008: Counterfactuals and context, *Analysis* 68(1), 39–46) argued that counter-examples to contraposition, strengthening the antecedent, and hypothetical syllogism involving subjunctive conditionals only seem to work because they involve a contextual fallacy where the context assumed in the premise(s) is illicitly shifted in the conclusion. To avoid such counter-examples they have proposed that the context must remain fixed when evaluating an argument for validity. That is the Brogaard-Salerno Stricture.

The condition imposed by the Brogaard-Salerno Stricture—BSS, for short—seems to be 'a basic tenet of standard semantics'. Tristan Haze (2016: Against the Brogaard-Salerno Stricture, *The Reasoner* 10(4), 29–30), however, has recently objected that intuitively valid argumentative forms such as conjunction introduction do not satisfy BSS. He presents the following counter-example:

> (P1) If Mary had not had breakfast, she would have lunched sooner. (P2) If John had worn black shoes, he would have worn black socks. (C) Therefore, if Mary had not had breakfast, she would have lunched sooner, and if John had worn black shoes, he would have worn black socks.

The context—Haze suggests—does not remain fixed, since (P1) and (P2) involve different contextually determined background facts. But since the argument is still intuitively valid, BSS cannot be correct.

I'm not convinced by that argument. Haze's claim is that (P1) and (P2) have different 'background facts'. But they have different background facts just in virtue of being premises about different topics, and nothing in BSS prevents premises to be about different topics insofar as they can be part of a single and constant context. BSS solely poses a constraint upon shifting the context, i.e., upon using more than a single context when evaluating arguments for validity. This constraint is not violated in Haze's argument: the context we use to evaluate the truth of the conclusion is the same as the context used to evaluate the truth of the premises. Thus, it cannot be a counter-example to BSS.

For comparative purposes, let's put Haze's argument in a situation where a violation of BSS *does* occur. Suppose the context in which we evaluate the premises is composed by the following facts: Mary is on a special diet that requires her to have light bites every couple of hours; John has an OCD that propels him to always match his socks with his black shoes. Given these background facts, the premises are true. Now, suppose that the context in which we evaluate the conclusion involves slightly different facts: Mary has to fast the entire morning and John's OCD impels him to match black socks with his black shoes only on Sundays. If one now uses conjunction of introduction to draw an inference from (P1) and (P2) to (C), the argument becomes invalid. That is precisely what BSS would predict, after all no single context was kept fixed throughout our evaluation.

To what is worth, Haze is not denying that contextual restrictions need to be placed on argumentation. He does regard determining the proper contextual restrictions on argumentation an important task. His only concern is that BSS is not up to the task. However, as I tried to show, BSS seems to do a very good job when properly understood.

In fact, BSS does more than placing contextual restrictions on argumentation. It has further interesting consequences not usually acknowledged. First, as Brogaard and Salerno (2008: 41) show, BSS not only serves to block counter-examples to instances of unpopular argumentative forms such as contraposition, strengthening the antecedent and hypothetical syllogism that involve subjunctive conditionals. It is also the only way to avoid counter-examples to instances of the venerable *modus ponens*.

Second, as noticed by Lycan (1999: It's Immaterial (A Reply to Sinnott-Armstrong). *Philosophical Papers*, 28(2), 133–136), if we keep the context fixed when evaluating the validity of arguments, counter-examples to contraposition, strengthening the antecedent and hypothetical syllogism involving *indicative conditionals* all disappear. In fact, even counter-intuitive classical argumentative forms such as the paradoxes of the material conditional are valid when the context is kept fixed. For instance, from 'John will not drink sulfuric acid' it is legitimate to conclude that 'If John drinks sulfuric acid, he will gain super powers'. The conclusion is false only in a context where the antecedent is true; but I cannot assume the conclusion's antecedent is true without illicitly shifting the context and disregarding that the antecedent was false in the premise. Thus, BSS seems to support argumentative forms from classical logic.

If the reasoning above is correct, it would give us a reason to accept the truth-functional hypothesis, according to which indicative conditionals of the natural language and the material conditional of classical logic have the same truth conditions. The fact that classical argumentative forms that involve indicative conditionals are valid under BSS provides a reason to think that indicative conditionals are material. After all, how else would we be able to explain this match between formal logic and natural language if indicative conditionals weren't material?

Perhaps even more interestingly, a similar reasoning to the above can be used to defend that subjunctive conditionals are material. After all, classical argumentative forms involving subjunctive conditionals are also valid under BSS, as shown by Brogaard and Salerno's discussion of some counter-examples to contraposition, strengthening the antecedent and hypothetical syllogism involving subjunctive conditionals. All suggests that a similar defense would work with the paradoxes of the material conditional involving subjunctive conditionals. The paradoxes of material conditional ensure that from the premise 'John will not drink sulfuric acid' it is legitimate to conclude that 'If John had drunk sulfuric acid, he would have gained super powers'. The conclusion is only false when the antecedent is true, but the premise is only true when the antecedent is false. Thus, there seems to be a promising route indicating that BSS also implies that subjunctive conditionals are material. Pursuing this route, however, is a task for another occasion.

Matheus Silva
Federal University of Santa Maria

News

Interdisciplinary Perspectives on Behavioral Economics, 22–23 May

On the 22nd and 23rd of May, an interdisciplinary workshop took place at the University of Helsinki, Finland. It was organized by Magdalena Malecka and Michiru Nagatsu from TINT, Centre of Excellence in the Philosophy of the Social Sciences.

For two days, researchers from different disciplines got together in the "Forest House" of the University of Helsinki to reflect upon some of the issues that affect behavioral economics nowadays. As the title of the workshop suggests, the focus was on the interdisciplinary relation between economics and psychology in behavioral economics. In the opening speech, the organizers explained that their aim was to encourage a philosophy of science reflection on foundational issues in behavioral economics.

Other questions that were addressed during the two days were for instance: What is the state of the art of the field? What is the relation of behavioral economics to orthodox economics? And why do we care about these questions after all?

The workshop had a lineup of speakers mainly from philosophy, economics, and sociology, with expertise in the history and philosophy of economics or with specific interests in the foundations and development of behavioral economics.

Participants (roughly around 80 people) were mixed in terms of discipline, affiliation and age. People arrived from everywhere: Moscow, Seattle, Ottawa, Madison, plus several European cities. The attendants ranged from young PhD candidates to more established Professors. As part of the audience, several MA students from different faculties attended the workshop too. There were also people from research institutes outside the university.

This mix brought a particularly nice atmosphere to the event. This was made possible in part by the fact that there were no parallel sessions, something that helped to have a coherent discussion. On the other hand, the workshop was relatively large and it was a bit of a marathon: we had roughly 10 talks per day, including two keynote speakers.

I was among those participants who first believed that some of the attendants were keynote speakers: Wade Hands, Daniel Hausman, Don Ross. It turned out that none of them was! In fact, they were there either because they submitted an abstract or just because they were interested in the topic. I experienced this as particularly positive about the workshop: TINT managed to attract submissions for contributed papers from many leading figures in the field, who would otherwise have been great candidates for keynote speakers themselves. This also determined that the overall quality of the contributions was very high.

The two keynote speakers were Erik Angner (University of Stockholm) and Arno Riedl (Maastricht University). The title of Erik's talk was "We are all behavioral economists now". In this talk, Erik gave a description of the state of the art of behavioral economics as a research program essentially in line with neoclassical economics. This view is gaining terrain in the literature: for example, even with important differences, the Stanford economist Raj Chetty in his 2015 AEA lecture argued on similar lines, that we should take a pragmatic interpretation of behavioral economics assumptions, á la Friedman.

Arno Riedl's talk was entitled "Human Social Behavior: Brains and Institutions". In this talk, he gave a very informative overview on two strands of experiments he has been conducting: one strand adopts techniques from neuroscience to investigate neural mechanisms underlying pro-social behaviors; and the other adopts new experimental designs to study ?spill over? effects of institutions on pro-social behaviors.

To have a more detailed idea of the topics of the talks, the abstracts can be found on the web page of the workshop. Moreover, the organizers are planning to publish a journal special issue with contributed papers.

Broadly speaking, a general observation may frame the overall debate, even if at the cost of simplifying it a bit. One speaker in the History of Science session (Vladimir Avtonomov, National Research University-Moscow) provided an interpretation of the history of economics as a discipline where two canons tend to replace each other over time, on the basis of different views about the degree of abstractions that is permissible in economics (it?s more complicated than this, but grant me the simplification for the sake of this summary). On the one hand, one canon is more general, based on deductive principles and the assumptions of homo oeconomicus; the other canon, in contrast, is less abstract, based on direct empirical observations, and takes into account the specific motivations behind economic behavior.

In some respects, this very contrast emerged during the workshop as well. There were some talks that defended a theoretically driven approach to economics, and challenged the attempts to integrate economics with psychology, sociology, or political science; other scholars favored instead a bottom-up approach to economics, as, for instance, in the spirit of evidence-based economics.

To give an example: In one presentation, a speaker (Armando Menéndez Viso, University of Oviedo) quoted a passage from Richard Thaler's book "Misbehaving" (2015), according to which "without the rational framework there are no anomalies from which we can detect misbehavior" (p. 251). The following day, another speaker (Michael Joffe, University College London) objected Thaler's view, claiming that the rational choice framework should be abandoned altogether. This is because, if we start from a theory and don't find results in accordance with it, then there is no puzzle, it's just that the real world doesn't correspond to the theory that we formulated of it. When this happens, it's too bad for the theory. Yet, several questions came from the audience asking how in practice we should proceed without a rational framework, even only as an organizing principle, and what the risks of abandoning it are, such as to end up with a bunch of isolated effects, etc.

After the talk on the two canons, someone in the audience asked about the take-home message: should we thus think that there is no progress in economics? That it is a continuous recurrence of two competing views and that we are not moving forward but in a circle? And again, the speaker replied that, as we philosophers know, progress is a tricky, difficult issue. How do we evaluate progress? What is it really?

The discussion continued on the question whether we should see the rational choice theory paradigm in a similar way as Ptolemaic theory: we know that it's wrong, but until we have a Copernicus in economics, we will hold to that view. Some people objected that the parallel with physics doesn?t hold, because of the normative aspects of economics that make it more urgent to solve the dispute.

To conclude, it was encouraging to see that the economists were debating philosophical questions, and that even more than others, they welcomed excursions to other fields and methods, as for instance from psychology, sociology and anthropology. It is rarely the case that a workshop is so illuminating and thought provoking. It offers a good example of the great work that has been done in the last years at TINT and that its members will continue to do in the future.

Chiara Lisciandra
University of Groningen, Faculty of Economics and Business

Fifth LSE Graduate Conference in Philosophy of Probability, 2-3 June

The Fifth LSE Graduate Conference in Philosophy of Probability took place on 2-3 June 2017 at the Centre for Philosophy of the Natural and Social Science, LSE. This was a philosophy of science graduate conference addressing students in Philosophy, Cognitive Science, Physics, Medicine, Computer Science, and related fields.

The keynote speakers were Julia Staffel (Washington University St. Louis), Anna Mahtani (LSE), Maria Carla Galavotti (University of Bologna), and Sylvia Wenmackers (University of Leuven). We accepted eight graduate speakers, and all of them had members of staff from the LSE (and Sylvia Wenmackers) as commentators.

The first day started with Maria Carla Galavotti's talk. A veryengagingsurvey of the history of probability was given: from its birth to its current different interpretations. Several famous names were mentioned, including Donald A. Gillies, who was an audience member! A lively Q&A followed the talk, with questions about the origins of axiomatization in probability theory sparking an interesting debate.

There were two more talks in the morning: Dean McHugh, Grzegorz Lisowski & Max Rapp (Amsterdam), who talked about inquisitive semantics and the lottery paradox, and a comment was offered by Roman Frigg (LSE), and Pablo Zendejas Medina (Pittsburgh) who talked about whether or not one should always accept cost-free information, and a comment was offered by Campbell Brown (LSE).

In the afternoon, there were two more talks by graduate speakers: Jeremy Steeger (Notre Dame), who talked about a quantum version of the probabilists' Dutch book theorem, and had as a commentator Miklós Rédei (LSE), and Alexander Carver & Paolo Turrini (Imperial), who talked about the influence of paths in the levels of segregation in the Schelling Segregation Model, and had as commentator Peter Sozou (LSE).

The firstday endedon a high notewith Sylvia Wenmackers's talk on infinitesimal probabilities, and on the possibility of assigning non-zero infinitesimal probabilities to remote contingencies. This was a very interesting talk, and quite entertaining too: 'I *heart* infinitesimals' was seen floating around in one of the slides. Additionally, the interest in infinitesimals was incredibly well motivated, which made the importance of the talk very clear. There was also a reference to the fact that both Sylvia and Julia (Staffel) were graduate speakers in an earlier edition of this conference, with Matt Parker (also a commentator at this edition and a member of the audience) having been Sylvia's commentator at the time!

After the talks, there was opportunity to talk more about probabilities (and other things as the level of consumed wine increased) at the conference dinner.

The second day started with Anna Mahtani'skeynotetalk. Anna spoke about vague credence as an alternative to imprecise probabilism. It all started with one's credence in "Sardines" ("Should you have a precise credence in the proposition that the neighbour has a box of sardines?"), and then a claim was made that instead of viewing the content of your credence as a set of probability functions, we should instead take the expression "credence" to be vague. The Q&A was very lively and some questions had to be postponed until the coffee break.

In the morning there were two other talks by invited graduate speakers: Milana Kostic (MCMP) talked about updating with restrictor conditionals, and Sylvia Wenmackers commented on her paper, and Gary Mullen (Leeds) discussed the no option puzzled, and had as a commentator Richard Bradley (LSE).

After lunch, two more invited gradate speakers: Boris Babic (Michigan) talked about generalised entropy and epistemic risk, and Matt Parker (LSE) commented on his paper, and finally the last graduate speaker was James Wilson (Bristol), who talked about accuracy and probability kinematics, and had Anna Mahtani as a commentator.

The conference ended with Julia Staffel's talk on a puzzle about outright beliefs. Julia offered a new hypothesis about how beliefs change across contexts and showed how her account better explains how we reason and update both graded and outright beliefs. The Q&A was again very lively, and Julia was very happy to hear everyone and take their suggestions on board.

The day ended with informal drinks at the White Horse, at the LSE, and everyone seemed happy whether it was because they enjoyed the conference or because they were relieved it was over, I am not sure. I think there is a high probability that it was the former!

Goreti Faria
London School of Economics and Political Science

Simulation and Thought Experiment, 8–9 June

The Geneva Centre for Philosophy of Science (Michal Hladky, Guillaume Schlaepfer, Marcel Weber) in collaboration with the DFG-SNSF Research Unit "What-if?" has organised a conference on Simulations and thought experiments [simthexp.wordpress.com] that took place at the University of Geneva on the 8th and the 9th of June 2017.

Simulations and thought experiments are used across scientific disciplines to produce and explore new theories, explanatory hypotheses and arguments that may guide us to new experiments and ultimately to new knowledge. Speakers have explored the similarities and differences of these notions from the

perspectives of philosophy of science, epistemology and philosophy of mind. Several have noted that the traditional literature in philosophy of science has attributed a special status to experiments, which in turn shaped the discussion about (computer) simulations on one hand and thought experiments on the other. More recent discussions show that the epistemic primacy of experiments can not stay undisputed and that it would be useful to analyse the common structure of real (RE), numerical (NE) and thought experiments (TE) (Arcangeli, El Skaf). On the other hand, imagination is often seen as less constraining than computer simulations, but Anouk Barberousse has convincingly challenged this view.

Rawad El Skaf and Daniel Dohrn have focused on the common structure of TEs and their epistemic dimensions. In his excellent presentation, Dohrn reconstructed Schlick's criterion establishing whether a question is meaningful. Schlick's test is based on TEs and demonstrated their utility even in the empiricists' framework. Contrary to the usual accounts restricting TEs to in principle executable experiments or reducing them to arguments, El Skaf's account shows that TEs are sui-generis tools to first reveal inconsistencies and then to resolve them. As TEs often introduce underspecified scenarios, their nomological (im)possibility is not under question and does not affect the unfolding of the TE. It is after the obtention of the result of a TE that several options to resolve the contradiction are considered. The removal of the contradiction brings about new theoretical knowledge.

Margherita Arcangeli has provided an analysis that presupposes a common structure of REs/NEs/TEs. Arcangeli drew distinctions between them based on the role of mental simulations (MS) at the performance level. MS may refer to two different capacities: mental modelling (objectual MS) and imagining (mental MS). Imagining being crucial for the performance of TEs. Moreover, TEs call for MS in a way that both NEs and REs do not, although MS can have a role to play in their elaboration. Arcangeli exposed also the connection between the narrative aspects common to REs, NEs, TEs and the role of imagination.

Similar distinctions were introduced by Nenad Miscevic who presented thought experimenting in political philosophy. Miscevic distinguished between a tradition that imagines political arrangements from the 3rd person perspective (Plato, Al Farabi, T. More, Fourrier, Cohen), and another from the 1st person perspective (Contractualists: Hobbes, Locke, Rousseau, Kant, Rawls, Scanlon). This division fits within the mental model view of thought experiments where 1st person perspective is tied to the specific kind of mental models that he dubbed mental simulations.

Anouk Barberousse considered that simulations (NE) are more than TE in silico. Computational constraints impose limits on NE, which the power of imagination appears to escape, she argued. Nevertheless, computed results can take us much farther than our imagination capacities.

Michael Stuart questioned the epistemic continuity thesis, which claims that there is an epistemic continuity between RE, TE and NE. According to Stuart, the epistemic continuity thesis holds if one considers the epistemic role of producing understanding. He also suggested that developing a better account of imagination is the way to better explain this phenomenon.

The notion of simulation was at the centre of the naturalistic account of nonnatural representation provided by Gualtiero Piccinini. It was shown that the standard accounts are not able to provide an analysis of states with propositional content that is not in the actual environment of an organism. The usual analysis of imagination, planning, hypothetical thought and counterfactual reasoning leads to attribution of a malfunction. Piccinini's account based on offline simulations of possible environments combined with a tracking function of these allows for an analysis avoiding malfunction attributions and brings us closer to the naturalistic, mechanistic, neurocomputational account of intentionality.

Claus Beisbart addressed the question of how RE, TE and NE support and extend counterfactual reasoning. Beisbart distinguished between thinking through counterfactual scenarios, i.e., highly idealized models, and establishing counterfactual conditionals about real-world systems. The latter being more difficult to account for. He used a meta-linguistic account and a possible world semantics of counterfactuals and concludes that the meta-linguistic account meshes well with the view that TE and NE are arguments.

A highly interesting formal account of epistemic confirmation in the case of analogue simulations was provided by Stephan Hartmann. The case study is based on analogue simulations of black holes by Bose- Einstein condensates that can be described by syntactically similar expressions on the pertinent level, but differ in the underlying micro-physical constitution. We note, that as simulations are often used in cases in which direct physical manipulation is not feasible, the provided strategy, if corresponding universality assumptions hold, can be reused in other empirical disciplines such as neurosciences.

We would like to thank all the speakers for their stimulating talks, to the participants for rich discussions, to the University of Geneva, Swiss National Science Foundation (SNSF), Fondation Mlle Marie Gretler and the DFG-SNSF Research Unit "What-if?" for their support.

Michal Hladky
Guillaume Schlaepfer
Geneva Centre for Philosophy of Science

Calls for Papers

New Trends in Rational Choice Theory: special issue of *Topoi*, deadline 27 August.

Foundations of Clinical Reasoning: An Epistemological Stance: special issue of *Topoi*, deadline 31 August.

Knowledge and Justification: New Perspectives: special issue of *Synthese*, deadline 1 September.

Reason & Rhetoric in the Time of Alternative Facts: special issue of *Informal Logic*, deadline 1 September.

What is a Computer?: special issue of *Minds and Machines*, deadline 30 September.

Scientific Discovery and Inferences: special issue of *Topoi*, deadline 15 October.

Disagreement: Perspectives from Argumentation Theory and Epistemology: special issue of *Topoi*, deadline 31 October.

The Reasoner Speculates

Bayesian Miracles

Teaching an undergrad course in decision theory, I usually talk about the phenomenon of confounding $P(A|B)$ with $P(B|A)$ (Ignoring Base Probabilities) with examples as in Kahneman-Tversky's works. A few points that I like to mention in class in this context are:

1. Bayes was aware of the issue, of course. He used an argument for the existence of God which ran roughly as follows (see McGrayne, 2011): observing the complex (and wonderful?) world that we live in has conditional probability 1 given that God exists (if we take "God" to mean the god of the scriptures, who created the world in six days etc.). It's hard to assign a conditional probability to observing this world (and living in it) if God does not exist, but it stands to reason that this conditional probability should be small. And then w'd like to revert the order and ask what's the probability that God exists given the world that we observe, and this requires a prior on the existence of God. (Notice that this argument is practically identical to the "Intelligent Design" one. Considering how many times I mention the word "evolution" in a typical decision theory class, I think that the Intelligent Design argument deserves to be mentioned at least once.)

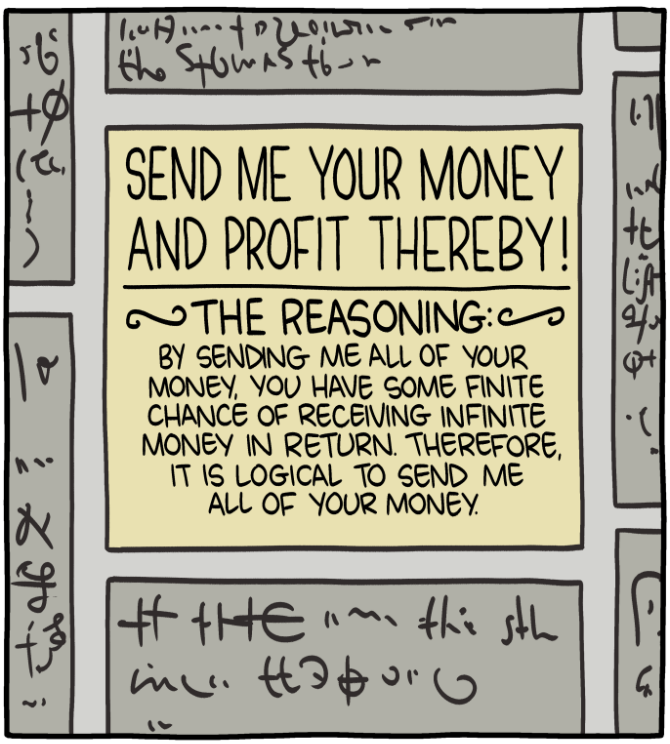
2. There is little doubt that some type of reasoning that allows one to infer causes from effects is very basic. For example, our daily experiences show that babies as well as pets can look for the source of a change in light or of a sudden noise. They seem to be going backwards from high probability of the effect given the cause to high probability of the cause given the effect. In this sense probability theory can be viewed as a formal model of ways of reasoning that evolution (!) has equipped many species with.

3. A relevant question is: Is the above necessarily Bayesian-style reasoning? Can't it be explained by something like maximum likelihood? The latter has less moving parts and, in particular, doesn't require a prior, and has a greater claim to objectivity. However, maximum likelihood is insufficient as a model of human reasoning. In particular, it needs some subjective input such as preference for simplicity to avoid overfitting. (Larry Samuelson and I have a paper that attempts to make this point more rigorously). Relatedly, people do not conceive of all possible theories, and a model of human reasoning needs to say something about which theories are likely to pop up in one's mind, and probably about some a priori ranking over them. Evidently, this can be done by a subjective prior.

4. There is early evidence for reasoning that can be modeled as Bayesian, and that is hard to capture by maximum likelihood. The notion of miracles in the Bible is based on this idea: God wishes to prove His power. He brings about a certain outcome that is otherwise highly unlikely, and its occurrence is used to convince the audience of Gods existence, power, etc. For example, when God tries to convince Moses that he should lead the people of Israel, negotiate with the Pharaoh, and generally embark upon a political career Moses has little appetite for, the latter says,"What if they do not believe me or listen to me and say, 'The Lord did not appear to you'?" (Exodus 4, 1). God then shows him the miracles he is about to perform to convince the people of Israel and the Pharaoh of the greatness of God, of his choice of Moses as a leader, etc. Notice that, if the audience were to use maximum likelihood, one wouldn't need to resort to extreme measures like turning rods into snakes and vice versa in order to make the point. It would have sufficed to pick any sign that is more likely to occur if God is than if He is not. The point of the miracle is that the observation would be very unlikely if God did not exist (or wasn't powerful, or hadn't chosen Moses, or) and, because of this, the observation results in a strong belief in God. The monotonicity of this belief with respect to the "greatness" of the miracle requires more than maximum likelihood.

5. Finally, to remove any doubt: the point of the above is that this reasoning, inferring a cause from its effect, can be modeled by a simple Bayesian model with four states, and it seems to be an extremely fundamental mode of reasoning. This does not mean that Bayesian reasoning is always the best way of modeling how people think, or that it is always rational to think this way. Sometimes, it takes a miracle to be Bayesian.

Itzhak Gilboa
Economics, Paris and Tel Aviv



Fun Fact: Before he turned to philosophy, Blaise Pascal made his living via mail fraud.

Medieval Reasoning

When thinking of the Middle Ages, reasoning is probably not the first thing that comes to mind. However, during those (approximately) ten supposedly "dark" centuries, there were times when philosophical speculation achieved a truly impressive level of complexity and technical sophistication, the likes of which would not be reached again until the 20th century and onwards. For the Western Latin writing world, this is particularly true of the 12th, 13th and 14th centuries. Even to the untrained eye, medieval metaphysics, epistemology, natural philosophy and logic appear surprisingly familiar but also radically alien. At a cursory glance at even the less renowned venues, it is easy to find several interesting snippets of something that we would recognise as reasoning in a modern sense and yet it is in a context or with purposes that to a modern reader would appear unfamiliar or puzzling. For example, in the 13th century Peter of John Olivi wrote astonishingly articulated economical treatises (De contractibus, De moneta), that could be considered forerunners of modern Political Economy. Around the same time, Ramon Llull composed several works dealing with electoral systems, grounded upon his (in)famous Art, which notoriously shows algebraic features and has been interpreted by some as the medieval version of modern graph-theory (e.g. A. Bonner, The Art and Logic of Ramon Llull: A user's guide, Brill 2007). However Olivi, a Franciscan friar, was writing about economy mainly to argue in favour of Franciscan poverty and to warn against the spiritual and material temptations of money; Llull was concerned, on the one hand, with outlining a reliable system to select the best abbot for a convent and, on the other hand, with building a combinatorial tool for discussing matters of faith in such a convincing way as to ensure conversions. I could offer many more similar examples, pointing at several philosophical and theological texts that would leave us with a similar feeling of recognition and puzzlement: sometimes such puzzlement comes from these theories' aims or contexts, while at other times it comes from the theories themselves nonetheless it does not take that initial recognition away. We understand enough of these somewhat peculiar theories to see that the Dark Ages may not actually be so dark after all: some of their features still remain foreign and elusive enough to be fascinating, while also leading us to believe that we might still have something to learn from them. To truly grasp what was going on and possibly make good of it, the trick seems to be in the balancing act between puzzlement and recognition. The study of medieval logic stands as a good example. The historiography of medieval logic is hardly a new field e.g. Prantl's Geschichte der Logik im Abendlande was published in 1855 , yet there are still many corners left to be explored.

In the last century, medieval logical theories had already piqued the interest of both logically minded medievalists and historically minded logicians. There are several features, common to medieval logical and philosophical works, that undoubtedly justified such an interest. For instance, many of these theories are extremely technical and they are usually structured systematically as a succession of principles, rules, and proofs. Moreover, medieval philosophy and logic show a recurring interest in the analysis and clarification of language. In many medieval logical theories we recognise something astoundingly familiar and interesting for our own endeavours, be it (among others) John Buridan's definition of formality, the widespread presence of theories that look quite like connexive logics, or the careful examination of what logically follows within modal, epistemic, and temporal contexts. A popular way of making such familiarity more explicit and accessible to modern readers has been (and still is) to formalise these theories into a symbolic language. This kind of operation brings together theories that are distant in time and makes them easier to compare. However, on the one hand, something always gets lost in translation and that something might be the really interesting bits. On the other hand, there are some aspects of medieval logic that just do not seem to fit properly within any of our contemporary schemes. Medieval logic appears to be a field that cannot be mapped directly over contemporary logic however contemporary logic turns out to be defined, which is itself not very obvious. For instance, medieval logic is not mathematical, but at least in the later Middle Ages it is formulated in a highly regimented version of (medieval) Latin, which was already at that time a quite artificial, semi-dead (or semi-living) language. The properties of this regimented language seem to be an essential aspect of medieval logic itself on that subject, I recommend having a look at the recent volume on Formal Approaches and Natural Language in the Middle Ages, edited by L. Cesalli, F. Goubier and A. de Libera.

Overall, medieval logic has several features and aims that are somewhat unusual from a certain contemporary perspective, though not unheard of - e.g. a predominant interest in semantics; pronounced ontological concerns; a pervasive reflection on ordinary reasoning processes and ordinary language "from within" (coupled with the lack of a clear distinction between logical and meta-logical language); an emphasis on meta-logical and philosophical questions; a centrality of dialectical procedures, and so on.

Even today many historians of medieval logic themselves disagree over what medieval logic actually is: contrary to the "formalisers", many believe it not to be logic at all. Some scholars find it to be too deeply rooted within the semantics of natural language and too "contaminated" by ontological, grammatical and dialectical concerns to be logic in any meaningful sense, preferring instead to label it as a kind of argumentation theory. Furthermore, even if we know the details and the mechanics of some more technical-looking doctrines, we still lack a sensible explanation of what's going on: in some cases, paraphrasing Paul Vincent Spade, we simply don't get what medieval logicians are doing. Surprisingly enough, this kind of assessment tends to be supported more by historians than by historically minded logicians or philosophers of logic. It is undeniable that, especially among the original medieval contributions to the history of logic (the so called logica modernorum, "logic of the moderns"), there are indeed doctrines such as "obligations" (obligationes),"expositions" (expositiones), or "proofs of propositions" (probationes propositionum) (a very interesting workshop on this still understudied topic was held recently in St. Andrews; you can find the abstracts here) that

have been quite baffling for modern historians. But, once these doctrines have been properly contextualised, they turn out to be far less mysterious than they at first seemed. Moreover, neither the sometimes obscure peculiarities of these doctrines nor the "expanded" interests of medieval logic tout court would lead us to believe that medieval logic has little or nothing to do with logic at all. Certainly, medieval logical theories are not akin to abstract mathematical constructs, while broader medieval definitions of the logical field seem to emphasise at an essential level its connection with actual reasoning. A common medieval distinction of two (deeply interwoven) parts of logic could be enlightening: a part of logic, amounting to metalogic and philosophy of logic, was the ensemble of theories describing good reasoning and regulating what it should be (logica docens); the other part, logica utens (literally "using" or "practicing"), amounted to actual logical practices and inferential techniques, it was often pronouncedly dialogical and dialectical, and included applied logic in plenty of philosophical and theological contexts. Even if we are ready to admit that some gaps persist between medieval and contemporary logic, it is exactly in those murky spaces that some interesting insights might be waiting. For instance, at least some of the peculiar features and interests of medieval logic have some analogue in several recent (and not so recent) developments within our own logical approaches (e.g. beginning with the comeback of semantics, or within projects like Natural or Informal Logic, etc.) and within disciplines which, strictly speaking, are bordering to logic but often claim to have some degree of formality and logic at their core e.g. Formal Linguistics, Argumentation Theory, some branches of and approaches to Philosophy, etc. As for the philosophical and meta-logical discussions of some still controversial topics (for example the nature of logical following, since, to quote the evergreen Haskell Curry, "no one knows exactly what a logical consequence is"), medieval logic does not seem to have any evident intrinsic disadvantage against its contemporary counterpart; even the use of a regimented version of Latin could sometimes (such as, for example, for the analysis of entailments) be a resource. Most importantly, medieval logic has potential to be an excellent place to think about the relation between Logic and its own history. Certainly, the mathematisation of logic was, in a sense, an enormous step forward: it brought great advantages in terms of clarity and manipulability, and it allowed us to tackle questions and issues that were literally inconceivable in an Aristotelian or medieval framework. It might be tempting to dismiss any previous logical approaches as "illogical" or at least "diminished", by claiming that our own way of doing logic is the one true way. However, such a claim would be hastily made and possibly unfounded. In one of the most brilliant and controversial recent publications in the field (Articulating Medieval Logic, 2014), Terry Parsons demonstrates that it is possible to give a reformulation of Peano's first order arithmetic in a (partially artificial) fragment of medieval logic, expressed in a language ("Linguish") modelled on medieval Latin. Parson's project is interesting both from a historiographical and philosophical point of view. On the one side, Parsons shows a "third way" of studying medieval logic from within, in a philosophically and logically interesting manner, articulating its properties and reformulating contemporary theories within it rather than going the other way around. On the other side, by doing so, he argues quite convincingly that medieval logic is indeed logic properly speaking, even by contemporary standards: "Historically, symbolic logic did not come with a seal of approval on it. Instead, it achieved its present status by providing a system in which it was possible to formulate central claims of mathematics and (less evidently) science, in which the valid derivations correspond to what mathematicians already recognize as valid reasoning. It is possible then that some other system of logic might accomplish the same goal, while being quite different from modern predicate logic. It need only provide for the formulation of central parts of mathematics and science." (p. 269)

To sum up, medieval logic and medieval reasoning offer many interesting surprises that go beyond any historical or antiquarian curiosity: in those not so dark centuries we do not simply find some odd forerunners of fully-fledged contemporary theories, but sometimes an alternative and equally valid approach, or another point of view on questions and projects that are still our own. "Logic is eternal, so it can wait", supposedly said the electrical engineer Oliver Heaviside; however logic seems to have its own history and we do not have any good reason to deny nor dismiss it. Logic (and reasoning) might as well be one of those constructs that Ian Hacking calls "human kinds", i.e. "a kind shaped in part by our conception of it", and as such it is subject to a "looping effect": its own changing, in itself, affects the way we think about it, and the way we think about it in turn determines what it is. Therefore thinking about how people reasoned in the Dark Ages might be time well spent and not just for historians. Who knows? It could be quite enlightening!

For a more complete and systematic overview have a look at The Cambridge Companion to Medieval Logic (2016). Are you interested in learning about about these topics? Would you like to discuss them with a group of logicians, historians and philosophers? Pay a visit to Medieval Logic & Semantics: it's the blog for you! Let's go medieval... on Logic!

Graziana Ciola
Philosophy, Scuola Normale Superiore, Pisa

What's Hot in ...

(Formal) Argumentation Theory

My bookshelves are warped by the weight of books lining up to be read, but one recent publication jumped straight to the head of the priority queue. Dan Sperber and Hugo Mercier's *The Enigma of Reason: A New Theory of Human Understanding*, summarises a programme of research building on their 2011 paper: 'Why do humans reason? Arguments for an argumentative theory' (in *Behavioural and Brain Sciences*). Sperber and Mercier propose that reasoning evolved to produce and evaluate arguments when communicating. It is this understanding of the evolutionary function of reasoning that underpins a comprehensive explanatory framework for the wealth of psychological evidence suggesting that reasoning often leads us astray; evidence that contradicts the view that reasoning typically leads us to more reliable beliefs and better decisions. However, while their theory explains the waywardness of the lone reasoner, it also explains why and how

reasoning keeps us on the path to better beliefs and decisions when *we reason together*, in groups and through dialogue.

In a nutshell, their theory argues that reasoning evolved to support communication. To avoid being misled, it is advantageous for an addressee to evaluate reasons (i.e, arguments) for the received information, and look for counter-arguments, before accepting the received information. In turn, it is to the advantage of the sender that he produce arguments supporting the information being communicated, in order that it be accepted. Reasoning thus increases the quantity and epistemic quality of the information humans are able to share, by allowing communicators to argue for their claim, and by allowing addressees to assess these arguments.

This evolutionary function of reasoning implies that a lone reasoner is disposed to seek reasons in support of his beliefs, and overlook reasons that argue for the contrary, especially when such beliefs are contentious and the reasoner anticipates that they will be challenged. This, for example, manifests in the classic confirmation bias, which the argumentative theory suggests is a *normal* feature of reasoning. Moreover, individual decision makers are disposed to harness reasoning to the extent that they anticipate communicating their decisions to others; hence evidence to the effect that we favour decision options that can be easily justified and are less at risk of being criticised, rather than because they satisfy some criterion of rationality.

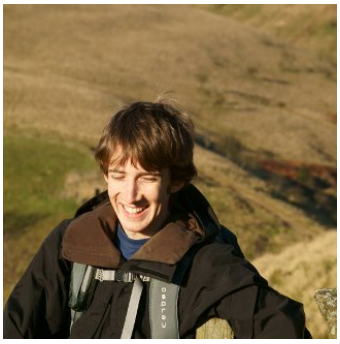
However, the argumentative theory also implies that reasoning serves us better when performed in groups and in particular when reasoning jointly through dialogue, under the assumption that interlocutors are motivated to have a common interest in the truth or the right decision. In these contexts, the dispositions of speakers and receivers to respectively seek arguments for claims, and evaluate and seek counter-arguments, yields better outcomes. That this is so is supported by evidence reviewed by Sperber and Mercier. These benefits of dialogical reasoning have, I believe, important implications for research into logical and computational models of argument and dialogue (see my review of these models in the June 2017 issue of The Reasoner). For example, such models may be developed for deployment in computer systems engaging students in dialogical interactions, or in supporting discussion amongst citizens in deliberative democracy. These use contexts assume an intent to get to the truth of the matter or make better decisions. A more challenging speculative use of argumentation technologies, is in the dismantling of the echo chambers erected by social media. These belief bubbles arise due to filtering algorithms feeding news and opinions that entrench people's ideological positions, and, as in classic examples of groupthink, even make those positions more extreme. Such algorithms are digital incarnations of our dispositions to seek arguments that confirm what we believe, but now unbounded by the limitations of a human reasoner reasoning alone. One might envisage AI technologies trawling the web to curate and present arguments, opinions and news that challenge the beliefs of bubble dwellers. But is this what a 'typical' user would really want ?

Disclaimer: Any semblance of impartiality in the above review is entirely coincidental (as you'd expect given my research agenda).

SANJAY MODGIL
Informatics, King's College London

Uncertain Reasoning

I want to go to the cinema, and there are two cinemas I could go to. One cinema is showing a boring documentary, and the other is showing an exciting action movie. I prefer to watch the action movie, so I should go to the cinema showing that one. So I make up my mind to go to the action movie. This is a pretty simple decision problem, but it seems like I have chosen correctly. Going to that movie involves getting a bus across town, it's a hot day, the bus is crowded and the journey is unpleasant. I watch the movie and enjoy it (and enjoy it more than I would have the documentary), then I get the bus back across town, again standing in the unpleasant heat. I get home and conclude that, all told, I would have enjoyed going to the local cinema (just a few minutes walk away) to watch the less exciting movie. What can we conclude from this story? There was nothing wrong with how I solved my decision problem: I would get more utility from the action movie than from the documentary, so I should go to the action movie. That seems correct in so far as it goes. The problem is that I solved the wrong decision problem: the problem I was really interested in was the one that took into account the disutility involved with travelling across town on a hot day. We tend not to talk too much about how we frame decision problems – about what counts as a good way to frame a decision problem – but perhaps we should.



Last month I was at a conference in Paris: "Coping with Uncertainty: Normative Approaches, Current Practice". This brought together various groups of people interested in the theory of decision making (economists, philosophers...) but it also included several speakers more focussed on the practical side of decision making: those who actually make decisions or who facilitate decision making by others. One thing that struck me – something that should have been obvious in hindsight – is that the hard work of practical decision making is not in calculating the expected utility (or whatever) but in actually framing the decision problem in the first place.

When mathematical economists or philosophers want to discuss a decision problem, they typically take for granted that the states, outcomes and acts are fixed exogenously, and the real work is in working out what to do given that. When experimental psychologists or behavioural economists try to elicit subjects' attitudes to various things, they will typically assume that the subjects take the experimental setup at face value: the urn really does have this many marbles of this and that colour in it, the choices you have really are restricted to this or that bet . . .

But when, say, the government of the Netherlands is determining its policy on flood defenses, it is **hard work** and requires significant expertise to even frame the problem in terms of the right states, outcomes and acts. For example, let's look at the Robust Decision Making framework developed by the group at RAND. (See, for example, Lempert, Popper and Bankes "Shaping the Next One Hundred Years: New Methods for Quantitative, Long Term Policy Analysis", RAND Corpo-

ration) The process involves input from scientific experts (in the flood example we're talking hydrologists, climate scientists, dam engineers, economists...) but also various stakeholders (representatives of the people who live in the flood plain, representatives of the government agencies who would maintain the flood defenses, wildlife protection agencies...).

And the process is dynamic: you send round your draft of what decision you're facing and encourage interested parties to show you how you need to make your states more finegrained (to account for causal factors you had considered irrelevant), to add detail to the outcomes that you might originally have thought unimportant, or to add new acts that you hadn't considered. So in taking for granted the decision problem setup, "theoretical" decision theorists are missing a lot of interesting detail as to how real world decisions actually get made.

At this point, many people might be thinking "isn't this just Savage's small world/grand world discussion again?" I don't think it is. Savage said that whenever we construct a decision problem, we have to do it at a fairly coarse-grained level so as to make it tractable: we construct a small world. We abstract away from all the fine-grained but irrelevant detail (I care about whether the coin lands heads, not about which direction precisely the nose of the head on the coin is facing). The point here is that what is irrelevant, and what ought to be explicitly taken into consideration in the decision problem, is a difficult question; a question that requires input from all sorts of experts and that requires real expertise to properly elicit.

What to conclude from this? First, I don't want to suggest that we've been doing decision theory all wrong! I think it is perfectly OK for people to discuss what ideal agents would do in particular situations, and they can stipulate that everything not explicitly included in the state space is irrelevant to the agent. Perhaps what I want to conclude is that there's interesting theoretical work to be done in looking more closely at the practice of real-life decision making and asking questions like: "What is the right decision problem to solve?"

Seamus Bradley
Philosophy, University of Tilburg

Evidence-Based Medicine

Last month, an editorial was published in The BMJ on the Evidence-based medicine manifesto for better healthcare. It was written by Carl Heneghan, Kamal R Mahtani, Ben Goldacre, Fiona Godlee, Helen Macdonald, and Duncan Jarvies.

The authors point out that '[i]nformed decision making requires clinicians and patients to identify and integrate relevant evidence'. But they argue that much of the available evidence is poor quality or fails to be relevant to patient care:

> Most published research is misleading to at least some degree, impairing the implementation and uptake of research findings into practice. Lack of uptake into practice is compounded by poorly managed commercial and academic vested interests; bias in the research agenda (often because of the failure to take account of the patient perspective in research questions and outcomes); poorly designed trials with a lack of transparency and independent scrutiny that fail to follow their protocol or stop early; ghost authorship; publication and reporting biases; and results that are overinterpreted or misused, contain uncorrected errors, or hide undetected fraud.

And they go on to provide a list of problems with current evidence, including the problem that most research spending goes to waste, the results from half of all trials are unpublished, and the increasing cost of clinical drug trials is preventing the development of new medicines.

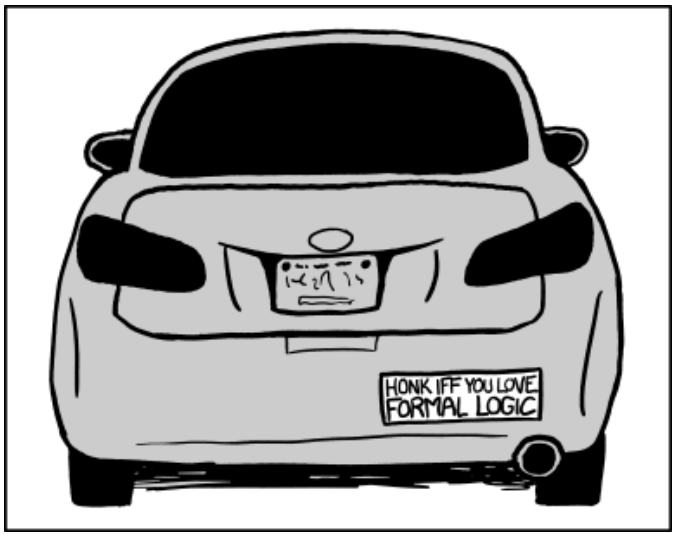
However, the authors also provide a list of the steps required for developing more trustworthy evidence: "Expand the role of patients, health professionals, and policy makers in research; Increase the systematic use of existing evidence; Make research evidence relevant, replicable, and accessible to end users; Reduce questionable research practices, bias, and conflicts of interests; Ensure drug and device regulation is robust, transparent, and independent; Produce better usable clinical guidelines; Support innovation, quality improvement, and safety through the better use of real world data; Educate professionals, policy makers, and the public in evidence based healthcare to make an informed choice; Encourage the next generation of leaders in evidence based medicine".

In order to take these steps towards improving the available evidence, the authors invite others to contribute to the EBM Manifesto:

> It is an open invitation for others to contribute to and join a movement towards better evidence by providing a roadmap for how to achieve the listed priorities and to share the lessons from achievements already made. Its aim is to complement and unite existing efforts as well as create new ones.

More information is available on the manifesto page of Evidence Live. There is also a video available outlining the project. And there is a podcast interviewing Fiona Godlee and Carl Heneghan about the Evidence manifesto: *It's time to fix the E in EBM*.

Michael Wilde
Philosophy, Kent



Events

July

RMaC: Representation, Meaning, and Content, University of Turin, 4–6 July.
VoKH: The Varieties of Knowing How, Essen, Germany, 6–7 July.
JD&RaR: Jonathan Dancy & Reasons and Reasoning, Saarland University, Germany, 6–8 July.
SaAiP: Symmetries and Asymmetries in Physics, Leibniz-University Hannover, 6–8 July.
AAfL: Conference of the Australasian Association for Logic, University of Adelaide, 6–8 July.
MitP: Making it (too?) precise, University of Geneva, 7–8 July.
PA: Philosophical Analysis, Krakow, Poland, 7–9 July.
PLS: Panhellenic Logic Symposium, Delphi, Greece, 12–16 July.
MM: Multilevel Modeling Using HLM workshop, University of Connecticut, 17–21 July.
OCL&TM: Workshop in OCL and Textual Modeling, Marburg, Germany, 20 July.
YSM: Young Statisticians Meeting, Keele University, 27–28 July.

August

MLwG: Mining and Learning with Graphs, Halifax, Nova Scotia, Canada, 14 August.
CW: Causality Workshop: Learning, Inference, and Decision-Making, Sydney, Australia, 15 August.
LFoUaL: Logical Foundations for Uncertainty and Learning, Melbourne, Australia, 19 August.
PLaAM: Philosophy, Logic and Analytical Metaphysics, Brazil, 21–23 August.

Courses and Programmes

Courses

EASSS: 19th European Agent Systems Summer School Gdansk, Poland, 7–11 September.
Computer Simulation Methods: Summer School, High Performance Computing Center Stuttgart (HLRS), 25–29 September.

Programmes

APhil: MA/PhD in Analytic Philosophy, University of Barcelona.
Master Programme: MA in Pure and Applied Logic, University of Barcelona.
Doctoral Programme in Philosophy: Language, Mind and Practice, Department of Philosophy, University of Zurich, Switzerland.
HPSM: MA in the History and Philosophy of Science and Medicine, Durham University.
Master Programme: in Statistics, University College Dublin.
LoPhiSC: Master in Logic, Philosophy of Science and Epistemology, Pantheon-Sorbonne University (Paris 1) and Paris-Sorbonne University (Paris 4).
Master Programme: in Artificial Intelligence, Radboud University Nijmegen, the Netherlands.
Master Programme: Philosophy and Economics, Institute of Philosophy, University of Bayreuth.
MA in Cognitive Science: School of Politics, International Studies and Philosophy, Queen's University Belfast.
MA in Logic and the Philosophy of Mathematics: Department of Philosophy, University of Bristol.
MA Programmes: in Philosophy of Science, University of Leeds.
MA in Logic and Philosophy of Science: Faculty of Philosophy, Philosophy of Science and Study of Religion, LMU Munich.
MA in Logic and Theory of Science: Department of Logic of the Eotvos Lorand University, Budapest, Hungary.
MA in Metaphysics, Language, and Mind: Department of Philosophy, University of Liverpool.
MA in Mind, Brain and Learning: Westminster Institute of Education, Oxford Brookes University.
MA in Philosophy: by research, Tilburg University.
MA in Philosophy, Science and Society: TiLPS, Tilburg University.
MA in Philosophy of Biological and Cognitive Sciences: Department of Philosophy, University of Bristol.
MA in Rhetoric: School of Journalism, Media and Communication, University of Central Lancashire.
MA programmes: in Philosophy of Language and Linguistics, and Philosophy of Mind and Psychology, University of Birmingham.
MRes in Methods and Practices of Philosophical Research: Northern Institute of Philosophy, University of Aberdeen.
MSc in Applied Statistics: Department of Economics, Mathematics and Statistics, Birkbeck, University of London.
MSc in Applied Statistics and Datamining: School of Mathematics and Statistics, University of St Andrews.
MSc in Artificial Intelligence: Faculty of Engineering, University of Leeds.

MSc in Cognitive & Decision Sciences: Psychology, University College London.
MSc in Cognitive Systems: Language, Learning, and Reasoning, University of Potsdam.
MSc in Cognitive Science: University of Osnabrück, Germany.
MSc in Cognitive Psychology/Neuropsychology: School of Psychology, University of Kent.
MSc in Logic: Institute for Logic, Language and Computation, University of Amsterdam.
MSc in Mind, Language & Embodied Cognition: School of Philosophy, Psychology and Language Sciences, University of Edinburgh.
MSc in Philosophy of Science, Technology and Society: University of Twente, The Netherlands.
MRes in Cognitive Science and Humanities: Language, Communication and Organization: Institute for Logic, Cognition, Language, and Information, University of the Basque Country (Donostia San Sebastián).
Open Mind: International School of Advanced Studies in Cognitive Sciences, University of Bucharest.
Research Master in Philosophy and Economics: Erasmus University Rotterdam, The Netherlands.

Jobs and Studentships

Jobs

Lecturer: in Practical Philosophy, University of Kent, deadline 1 July.
Post-doc: in Vagueness and Indeterminacy, University of Milan, deadline 3 July.
Lectureship: in Logic/Epistemology, University of Oxford, deadline 4 July.
Research Associate: in Machine Learning and Neuroimaging, University College London, deadline 8 July.
Post-doc: in Econometrics and Statistics, University of Minster, Germany, deadline 15 July.
Post-doc: in Statistical Methods, University of Bergamo, Italy, deadline 21 July.
Associate Professorship: in Statistics, University of Oxford, deadline 30 August.
Postdoctoral Fellowships: British Academy, deadline 6 October.

Studentships

3 PhD's: in Didactics of Mathematics and interactions with Informatics, University of Montpellier, France, deadline open.
PhD: in Epistemology/Philosophy of Mind, University of Fribourg, Switzerland, deadline 30 September.