The Reasoner

Volume 10, Number 7
July 2016

thereasoner.org
ISSN 1757-0522

Contents

Editorial

Formal epistemology is a booming field of inquiry. Despite the general label, the field as a whole is in fact quite diverse. In particular, the distinction between quantitative, probabilistic approaches on the one hand, and qualitative, logical approaches on the other hand seems to be significant. But what exactly does this distinction amount to? Is it only a matter of the techniques and methods being used, or is there substantial disagreement between the proponents of each of them with respect to the nature of the phenomena being modelled? What about the similarities and connections between the two frameworks, which in recent years have been studied by people such as Kevin Kelly, Hannes Leitgeb, Branden Fitelson, among others? These are all important questions, and to address them Catarina Dutilh Novaes and Rohan French had a chat with their Groningen colleagues Jan-Willem Romeijn and Barteld Kooi, who have both done significant work in formal epistemology (Jan-Willem mostly on the probabilistic side, Barteld mostly on the logical side, with dynamic epistemic logic).

Catarina Dutilh Novaes

Catarina Dutilh Novaes
Rohan French
University of Groningen

Features

Interview with Barteld Kooi and Jan-Willem Romeijn

On Thursday the 31st of March 2016, we, Dutilh Novaes and Rohan French, got together with our Groningen colleagues Barteld Kooi and Jan-Willem Romeijn to talk about formal epistemology. Here are some highlights from this conversation.

Rohan French

Catrina Dutilh Novaes: Jan-Willem and Barteld, thanks Catrina Dutilh Novaesfor making time for this interview! We will be talking about different approaches in formal epistemology, in particular the so-called logical (qualitative) approaches and the so-called probabilistic (quantitative) approaches. As a first (imperfect!) approximation, we may say that the main

difference between the two seems to be that logical approaches tend to look at belief as an all-or-nothing thing—an agent either believes a proposition or she does not—whereas probabilistic approaches treat belief as a matter of degrees. Given that Barteld has worked mostly on the qualitative side, but also with interest in the interaction between probability theory and logic, and Jan-Willem has worked predominately on the quantitative side, I was wondering what you both think the most salient differences are between the two frameworks. Should we prefer one of them over the other?

Barteld Kooi: What you notice as a logician talking to people who do probability theory is that logicians tend to focus very much on language—they define a language and then give a semantics for it—while people in probability theory and statistics basically just use set theory, talking of functions, events as sets etc. So that's a difference, this sensitivity to language, which I feel that you don't see so much on the other side. The interface between the two then consists in seeing the system as a language and then treating it as a logic. If you then start thinking about the interface, a number of questions are much easier to treat if you have the full framework of set theory at your disposal, while if you first define a language and then have to give a semantic theory and completeness proof, that's a lot more work before you get to things which are interesting, than if you just work against the background of set theory and think "we need to solve this exact problem". So it seems that logic is a bit slower sometimes when it comes to heavy machinery. What is the fruitful interaction there? I think the fruitful interaction, from the logic perspective, is that some issues which may seem more linguistic sometimes seem to be overlooked by quantitative approaches. For instance, think of Van Fraassen's Judy Benjamin problem (See van Fraassen, B.C. (1981: "A Problem for Relative Information Minimizers in Probability Kinematics," *British Journal for the Philosophy of Science* 32:375–379).). It's often put in terms of having a probability space and having to do some update and get the right answers, and if you look at the logic perspective you have to look at the fact that there's a message coming to Private Benjamin, and you have to ask 'in what language is it coming?' and 'what is the communication protocol being used?', and so on. That makes it a more pragmatic issue than just the issue of moving from a set to a subset and having to figure out which subset to go to.

Barteld Kooi

Jan-Willem: That's actually a very useful approach, focus on the protocol, which Joe Halpern and others have stressed. There are lots of probabilists who think this is actually a really important insight. You should pay attention to the protocol because that influences what sort of probabilistic update youre going to do, so that's actually really nice, fruitful, interaction right there.

CDN: So the qualitative/logical approach pays more fine-grained attention to linguistic matters, Jan Willem?

JW: I second what Barteld just said about the style of working. I think for a lot of probabilists its really mostly about modeling. You're not worried about the proof theory that might be sitting in the background, possibly wrongly so. This is something that someone like Jeff Paris might say when someone comes up with a nice probabilistic model—"you really should prove that this is a coherent model—that this model isn't just hiding some weirdnesses away in the corners". So I think that's one immediate way in which help from the logicians' side can be very fruitful, because logicians are more keenly aware on how to get organised on the consistency front than the modellers are.

Aside from policing consistency, another sort of thing that logic can bring to probabilistic enterprises is a way of enriching the instruments that a probabilistic modeller has. For example where, for instance, you have a statistical problem which you can straightforwardly model, you might have constraints on the behaviour of your variables of a logical kind, and you find that you need knowledge from the logic side to beef up your probabilistic model. I think that sort of thing is an obvious cross-over, where people could use the knowledge that logicians have a bit better than they're doing now.

CDN: So what can the logician learn from the probabilistically-minded person?

JW: Probabilists, as modellers—I'm going to generalise, I think this is very inadequate for some people who are very keenly aware of these issues—have perhaps a finer feeling for what sort of target system they're looking at, so they may be more sensitive to the specific epistemological issues which need to be addressed, and they build a model around that. Logicians are a bit further removed [from the phenomenon], maybe, because they're concerned with far more general issues—how does the whole system work; rather than just getting one model they talk about all the models that fall under these general headers. So logicians can take cues from probabilists about what sort of models are salient for the idea of applying logic to problems in epistemology. As Barteld was already saying, set theory is what we're really using as probabilists, just measures and sets, but that's versatile as a material to wrap around all these epistemological issues we have. Logic is, not in a negative way, perhaps more dinky and more abstract.

Jan-Willem Romeijn

CDN: So if I understand you correctly, you both seem to think that either way, as far as logical approaches and probabilistic

approaches, there's not really one which is going to be the right one in terms of really giving you an accurate description of the phenomenon, for example that belief really is a matter of yes or no, so we should use logic; or belief really is a matter of degrees, so we should use probabilistic approaches. As I understand it, the two of you seem to think that there's nothing in the nature of the phenomena which says that one approach will be the right one and the other the wrong one.

BK: Indeed I don't think so, and I think what Jan Willem just said is very important. It often depends on the question that you want to answer, and indeed any formal person can go off on a technical detour and think "oh this may be interesting in itself", and maybe logicians are more prone to that than probabilists. But no, I don't think there's a fundamental difference or fundamental choice to be made there. To think about where probability really helps logic, so to say, as we've mostly been thinking about how things can go the other way, there are only a few places where it can enter logic, and this is something we see with all sorts of other areas as well. It can enter the language, and then the language is about probability, but the rest of the logical apparatus is standard. Of course probability could instead enter at the notion of validity, and make validity a probabilistic or graded matter. In that case, just as with possible worlds in modal logic, maybe in your logical language you don't notice that there's any probability involved, but underlying that there's a probabilistic system which determines what's going on.

JW: I'd like to react to that, but I'd first like to react to the earlier question about there not being any priority between these two ways of approaching things. One thing I want to give attention to on this point is that very important and interesting work is currently happening precisely where people are trying to make these systems meet in an original way. There's work by Hannes Leitgeb and Kevin Kelly and Branden Fitelson on the issue of how we relate quantitative expressions of belief to more qualitative expressions of belief. That's very fruitful because it then shows something which we already know by other routes, like the de Finetti/Ramsey route of saying that probabilities are really about logic and so on, namely that probability theory presents consistency constraints on degrees of belief just like classical logic provides consistency constraints on qualitative belief states. Here we have a clear case where it looks like the best resolution is to just think that we have a continuum of formal systems. When you look at the sciences, which is something that I'm very keen on doing, of course there's lots of statistics. Almost all empirical sciences use statistics, and that's probabilistic. And then there are other inference machineries that people use in the sciences, modelling-wise, like economists and psychologists use probabilistic models. Game theorists and economists, for example, will also use bit and pieces of logic—under the influence of Aumann and Harsanyi and so on. By building bridges we can perhaps also involve more of logic as an instrument for the sciences.

Rohan French: So returning back to the old logical empiricist method of doing 'real logic' and model theory to understand scientific theories?

JW: There's a bunch of different ways that could happen. There's, of course, the methodology of the sciences; could it use logic? It's definitely using probability. There's modelling in the sciences, building models of target systems—be they ecologies, or societies, or economies or what have you—and in that modelling activity, do we use probability theory?—yes—do we use logic?—sometimes, but not so much. Then there's the third one, doing metascience, discussing what science really does. There lots of probability is being used to explain what sciences are, there's also a bit of logic, but since Suppes and that whole tradition of no-metamathematics but mathematics to understand the sciences—this whole semantic approach to theories—it's mostly been set theory and probability theory people have used there. I think there's something to be gained there by incorporating more logic, I think. These are three points where I think that, by building bridges, we can bring in more logic to those areas of the sciences.

RF: Is it right to say that the two of you are sketching a picture where the difference between probabilistic and more logical approaches is one of whether, when looking at the target phenomena, we either use all these powerful tools, of modern set theory and probability theory, and strip things back to get to something that's a useful/realistic/helpful/fruitful model of the phenomena on the one hand, and on the other we start out with the meagre resources of, say, Hintikka-style epistemic logic and then we try to built up, adding the extra expressive resources we need in order to get at the target phenomena—adding public and other announcements, adding explicit acknowledgments of the awareness of agents etc. Am I right that this is basically the way the two of you are seeing things here?

BK: That definitely occurs. This is what we talked about earlier, being more sensitive to communication protocols and what have you, but also having a very rich model of what change is. But take the lottery paradox, that's really about the interface between the logical approach and the probabilistic approach. There, really, there's a connection being suggested. Suppose we think of belief as just having a really high probability, that seems reasonable, but then we run into this problem [lottery paradox]. This seems to press you to either abandon this connection, or instead give up some core logical principles. I think lots of the work that's going on now is investigating these kinds of connections. So if we're thinking about these yes/no issues, we have a supervenience of these binary things on an underlying probabilistic model, we are forced to wonder about whether we really can separate out these two levels, and figure out how the levels relate.

JW: So I just want to get clear on this pruning idea, Rohan. Is the idea that we take a rich structure and we boil it down, or we take a poor structure and build it up and hopefully meet somewhere in the middle.

RF: Yeah, thats the kind of picture I was sketching.

JW: I think that as far as the probabilistic side is concerned, it does look a bit like that. For example, take the interesting stuff on using sets of probability functions instead of single probability functions to model credences, something which Levi had already talked about in order to bring probabilistic approaches closer to AGM and belief revision issues. That

whole program has been going for a while, that we need to have slightly impoverished versions of probability theory to properly model target phenomena, and there we really do see that pruning going on. And on the building up side, I think Barteld's work on building probability theory as a component of a logic is adding bells and whistles to epistemic logic, making it both dynamic and probabilistic; I think that's a good example of some of the ways we can enrich structures like that.

CDN: Preparing for this interview, I was googling around and came across the webpage for that conference in Amsterdam some two years ago that you both attended on exactly that, qualitative and quantitative approaches to formal epistemology. How did that go?

BK: It went very well! I think Sonja [Smets] organised it, and the exact things we've been describing here were going on there.

JW: I think there's a shared feeling that this is a way forward. I think for instance that Alexandru [Baltag] and Sonja are looking at orderings of strengths of belief, that's not yet with a metric, that's an ordinal structure...

CDN: But that's already departing from the straightforward classical approach where you wouldn't have any ranking.

JW: And that's really tying into a lot of stuff in belief revision.

RF: So building on the work by people like Spohn and Rott which shows this super strong connection between this kind of ranking theory and belief revision.

BK: In a sense that's taking a qualitative approach to a quantitative question. But this is also what you see when you look at Halpern. He's not saying, here are the Kolmogorov axioms and that's it; there are also these plausibility measures, Dempster-Shafer and lots of other approaches to what he'd call uncertainty. There you see it's unclear that there's a shift between set theory with measures and real numbers and then...

[[JW's phone rings]]

CDN: We'll keep that in the interview for 'naturalness'.

BK: Indeed!

[[JW' phone rings screechingly loudly!]]

BK: then moving to these plausibility measures, they're just an ordering which is much closer to a logician's heart than these numbers, while from a theoretical perspective they're really pretty much in the same ballpark, and the things you do with them are very similar.

JW [after brief phone call]: I just want to connect up to something I said earlier on. We as philosophers are interested in these formal systems for our own interests in the integrity of knowledge, the coherence of our beliefs or what have you, and those epistemological questions spill over into the philosophy of science of course! But I think lots of people outside philosophy faculties are interested in the sorts of things that philosophers can bring to the table, because they're working with information and they need to manipulate information and extract what is salient for them. I'm talking about a broad category of scientists, definitely including those in the big data sphere, where you see a lot of computer scientists—and computer science is applied logic you might say—and there you have an opportunity for us to really bring in interesting new tools, and interesting insights into broad issues which involve the manipulation and analysis of information. I think looking at how difficult it is to get scientists to adopt simple, new, statistical methods, it really is a huge challenge to get them to look at those methods, and it is an even bigger challenge to get them to make use of all of those very important things that philosophers have noticed about the ties between logic and probability theory. So we should get these things out there, and show scientists and others that they have all these tools at their disposal that you can use if you want to manipulate or extract salient stuff from your information set. We've got all these nice tools, and some people really need them. For instance if you're doing a big data cancer research project, you might really have a use for ranking your propositions and doing updates when you gain new data, much like we've been designing these systems in order to keep things coherent and in order to use information in the best possible way.

CDN: Right, so that should be an important focus for formal epistemology, broadly understood: make it increasingly clear to people who work in other fields that the findings and tools being developed can also be very relevant outside philosophy.

On this positive note, let us wrap up this interview. Thank you all for sharing your views!

News

The History of Science and Contemporary Scientific Realism, 19–21 February

"The History of Science and Contemporary Scientific Realism" Conference, was held between the 19th and 21st of February, 2016 at Indiana University-Purdue University Indianapolis, USA. The conference was part of the AHRC funded project "Contemporary Scientific Realism and the Challenge from the History of Science", led by Peter Vickers (Durham University) and Timothy D. Lyons (Indiana University-Purdue University Indianapolis). This three-day conference featured 28 historians of science and philosophers of science representing universities from 10 countries. In particular, aiming for genuine interaction between disciplines, it brought together not only leading philosophers in the realism debate but also distinguished historians of science. The plenary speakers were Anjan Chakravartty (Notre Dame), Helge Kragh (Copenhagen), Stathis Psillos (Athens), Eric Scerri (UCLA), Jutta Schickore (Indiana), Betty Smocovitis (Florida), and P. Kyle Stanford (UC Irvine).

The conference aimed at shedding light on episodes in the history of science (both distant and recent) which have received little attention in the realism debate, especially cases where historical actors had significant explanatory/predictive successes

with a theory now rejected; and bringing historical case studies from different fields (e.g., biology, chemistry, fundamental physics, genetics, and geology) to bear on philosophical positions, especially 'scientific realism'. A primary question addressed by many of the papers was to what extent the theoretical constituents that are now rejected led to significant predictive or explanatory successes. In addition, new perspectives and approaches were introduced to shed light on the debate.

For instance, detailing the surprising successes of Nicholson's atomic theory, Scerri inquired as to just how such incorrect theories can be so progressive. To answer, he proposed an 'organic' theory of science. Psillos, although acknowledging important conceptual shifts in science, defended a realist stance: retention pervades theory-change, expressing as it does the history of evidence. Stanford sought a Middle Path between realism and instrumentalism by embracing as a common ground "the Maddy-Wilson Principle": there is a reason a given cognitive instrument enjoys empirical success. He identified the locus of tension that nonetheless remains and proposed that historical evidence can be used to resolve it. Despite the extraordinary predictive success of symmetry principles in the Standard Model, Chakravartty emphasized that the ontological commitment they afford realism remains unclear. Against the structuralist's top-down approach and, on behalf of a more traditional realism, he defended a bottom-up dispositional property interpretation.

The case study approach to the realism debate was itself carefully examined. Smocovitis looked to research on the plant genus Crepis, articulating how concern over environmental specifics came to dilute the content behind 'ideal types' and 'model organisms'. More broadly, she reflected on how the historian's approach differs from the philosopher's. Kragh introduced numerous examples from the history of cosmology but questioned the objectivity of criteria for favouring a given case. He argued that, even if necessary, such studies are insufficient to resolve the realism debate. Similarly, while granting the utility of case studies, Schickore proposed a shift from the history of theories and predictions to methodologies, and she illustrated her proposal's promise by appeal to nineteenth-century medical biology.

More information on this conference, including the titles and abstracts of the talks, can be found at the project website.

Peter Vickers
Durham University

Explanation, Normativity, and Uncertainty in Economic Modelling, 16–17 March

This two-day interdisciplinary conference at the Department of Philosophy, Logic and Scientific Method at the London School of Economics brought together philosophers and economists to discuss philosophical issues of economic modelling. Anna Alexandrova and Robert Northcott kicked off the event by arguing that we should shift the evaluative question from whether economic models provide explanations of real life phenomena to how well they actually do so. They argued that answering this question required a systematic study of how theoretical modeling compared with many alternatives (e.g., observational or historical case studies) in this regard.

Kevin Hoover's talk, delivered as a public lecture, argued that the apparent puzzles concerning economic models' ability to serve our explanatory purposes are artifacts of a misconception of what it means for a model to represent a target phenomenon. Underpinning his assessment with a view of economic models as akin to analogies, Hoover presented a view of the function of models that accounts for their successful roles as instruments for the acquisition of especially empirical knowledge, in the face of economic reality in which knowledge is acquired in a piecemeal fashion.

Robert Sugden presented a joint work with Geraldo Infante and Guilhelm Lecouteux questioning a popular suggestion to treat real individuals' deviations from the postulates of conventional rational choice theory as mistakes and to attempt to reconstruct their true rational preferences for purposes of policy recommendations. The talk criticized this approach for its implicit reliance on a dualistic model of the human being, in which an inner rational agent is trapped in an outer psychological shell.

Caterina Marchionni, using examples of the Prisoner's Dilemma and Hotelling's model of spatial competition, defended three theses: that economic models represent mechanisms; that models can provide potential explanations of economic phenomena; and that the move from potential to actual explanations involves empirical and non-empirical forms of confirmation. Philipp Wichardt explored the connection between economic practice and the fictional view of models and their representational capacities proposed by Roman Frigg. Wichardt argued for the fruitfulness of viewing models in this light using Akerlof's market for lemons as an example.

Richard Bradley explored methods for dealing with uncertainty concerning models' ability to capture the underlying structures of the economy. His talk focused on three approaches: Bayesian model averaging; robust control theory; and the application of confidence measures on probabilistic predictions. Wouter den Haan provided more examples of these techniques and pointed towards the need of finding a way that avoids the problem of the overly risk averse robust control techniques and Bayesian averaging.

In addition to the keynote talks, Roberto Fumagalli, Giulio Gipsy Crespi, Jennifer Jhun Soyun, Yang Liu, Osman ağlar Dede, Patricia Rich, Daniel Malinsky, and Roel Visser gave contributed papers addressing various exciting issues in the philosophy of economics. The full conference programme with abstracts is available here.

James Nguyen
Mantas Radzvilas
Nicolas Wüthrich
London School of Economics
Jurgis Karpus
Kings College London

Mathematical Philosophy, 7–9 April

The Munich Center for Mathematical Philosophy (MCMP) hosted the 2nd Munich Graduate Workshop in Mathematical Philosophy on April 7–9, 2016. The workshop was organized by MCMP members Seamus Bradley and Gregory Wheeler and supported by the Alexander von Humboldt Foundation. The theme of this year's workshop—formal epistemology—attracted talented graduate students across Europe working on formal approaches in epistemology and overlapping areas in cognitive science. Further to graduate presentations, the work-

shop featured three keynote addresses and three tutorials, each led by an MCMP member.

On the first day, Christian List (LSE, keynote) discussed the similarity between belief-binarization and judgment aggregation. List suggested that the problem of defining full belief through degrees of belief can be construed as a judgment aggregation problem. After laying out some desiderata comparable to those in the judgment aggregation literature, he showed that aside from trivial cases, there is no belief-binarization rule that satisfies all of the conditions.

Chloé de Canson (Cambridge) introduced a resolution of the problem of formulating new theories in Bayesian epistemology, which requires both probability and proposition kinematics—more precisely, this involves conditionalizing on the evidence that a new theory has been introduced and expanding the relevant algebra to include the new theory.

Christoph Merdes (LMU/MCMP) presented models explaining the emergence of collective irrationality and described how interventions might be implemented to reverse the irrationality. Merdes suggested manipulating the network structure triggering a belief revision through a weighted averaging rule or establishing a central influence in the network by adding a single node connected to each agent. He ended with a discussion on the limitations of such interventions in practice.

Lavinia Picollo (LMU/MCMP) gave a tutorial on logics for belief and knowledge. After a review of propositional and first-order modal logic, Piccollo moved on to epistemic logic, doxastic logic, dynamic epistemic logic, and public announcement logic, and concluded by discussing how degrees of belief may be situated in a logic.

On the second day, Jeanne Peijnenburg (Groningen, keynote) dicsussed the gradability of knowledge. Knowledge of vague statements, for example, is an instance when knowledge may be gradable rather than categorical. However, it is unclear how to precisely measure the degree of knowledge in such case. Peijnenburg proposed a novel account of what it means to know in part and a way to measure partial knowledge.

Alexandru Marcoci (LSE) challenged Adam Elga's defense of the principle of indifference for centered worlds. Marcoci claimed that an agent in a state of uncertainty, like in Elga's Dr. Evil and Duplication examples, conceives of alternative ways the world could be prior to obtaining new information while a similar agent who is certain of such information from the start does not. Depending on how the alternatives are construed, the uncertain agent may be in a completely different epistemic position from the certain agent. Thus, Elga's examples fail to motivate a principle of indifference for centered worlds.

Pavel Janda (Bristol) argued that Kierland and Morton's epistemic utility argument vindicating both 1/2 and 1/3 responses in the Sleeping Beauty problem depends on the use of a non-standard decision rule. Janda showed that if a standard decision rule of maximizing expected lifetime utility is employed in solving inter-temporal decision problems instead of minimizing expected disutility, then the 1/3 response no longer holds. Thus, Beauty's degree of belief upon wakening should always be 1/2.

Pia Schneider (LMU/MCMP) discussed how accuracy alone is insufficient for determining the epistemic goodness of an agent's belief state. Schneider demonstrated the insufficiency using a Gettier-style example in which an agent has incoherent degrees of belief, but the agent's degree of belief in a considered proposition is more accurate than a neighboring agent whose degrees of belief are coherent. In short, the incoherent agent is closer to having a true belief than the coherent agent, but the former's belief state is less accurate than the latter's.

Gregory Wheeler (LMU/MCMP) led a tutorial on sets of desirable gambles, which generalizes the theories of lower previsions, sets of probabilities, and lower and upper probability. After describing the basic details of the framework, Wheeler turned to the application of imprecise probability in epistemology and responded to the alleged challenges raised in recent literature including dilation and sequential decision making. On the final day, Hans Rott (Regensburg, keynote) discussed stability accounts of knowledge and belief. By way of illustration, Rott showed that neither belief nor knowledge is stable. The reasons for their instability differ, however, for he claimed that knowledge is not unstable as a result of belief being unstable. Despite the common assumption that knowledge entails belief, the instability of each state does not depend on this relation.

Mario Günther (LMU/GSN) presented part of his joint work with collaborator Holger Andreas on an analysis of 'because' that strengthens Gärdenfors' Ramsey Test for conditionals. Rott's formulation of the Ramsey Test, Günther argued, does not account for the asymmetry of 'because'. He suggested that suspending judgment on the antecedent and consequent of a conditional, reintroducing the antecedent, and then administering the Ramsey Test resolves the problem.

Nina Poth (Bochum) discussed the 'complex-first paradox' in which children acquire complex concepts (e.g., dog) prior to simple concepts (e.g., green). The empirical discovery is perplexing for one would intuitively expect the opposite. To explain this cognitive phenomenon, Poth extended the Bayesian theory of word learning proposed by Xu and Tenenbaum showing that children infer the meaning of complex concepts from a few instances gathered through experience faster than they learn the meaning of simple concepts. Finally, Ulrike Hahn (Birkbeck/MCMP) gave a tutorial, which discussed how experiments in psychology not only aid the development of descriptive theories of human behavior, but also prompt revisions to our theoretical intuitions on normative theories. Hahn emphasized the instrumental value of designing experimental materials in psychology due to the fact that such a practice shapes how one thinks about the scope and applicability of normative theories.

Overall, the workshop was very insightful bringing to attention novel solutions to new and old problems in epistemology.

Lee Elkin
Munich Center for Mathematical Philosophy

Aspects of Defeasible Reasoning, 4 May

Held on occasion of hosting a delegation from the University of British Columbia at Vancouver, workshop contributions broadly addressed the formal study of our defeasible inferential habits. Wolfgang Spohn (Konstanz) presented a ranking-theoretic account of abduction in the Peircean sense of 'inference to the best explanation'. His own ranking theory offers new ways of harmonizing apparent conflicts between approaches that model this inference via Bayes' theorem, and opposing approaches that view Pascalian probability as a deeply inappropriate choice—also on the grounds that abduction may well involve belief-conditionalization, but not probability-updating. Ranking-theoretically speaking, the pos-

terior implausibility of a hypothesis simply adds its prior implausibility and the unexpectedness of the evidence given the hypothesis. This measures the badness or goodness of an explanation. Thus, by being tailored to make full rather than graded belief differentially tractable, ranking theory immediately speaks to a compatibilist approach to abduction such as Peter Lipton's.

Addressing whether epistemic chains must rest on some foundational element, or ground, Jeanne Peijnenburg and David Atkinson (Groningen) showed that, if probability-raising serves as a necessary condition for epistemic justification of one proposition by another, then the justification of a target proposition by a chain of propositions depends on (i) the transmission of justification from the ground, and (ii) the emergence of justification from the conditional links in the chain. Illustrating a general result, as the chain becomes longer, so the relative importance of (i) decreases, and the importance of (ii) increases. Indeed, in the formally infinite limit no transmission from the ground is left; rather, the justification of the target has now emerged solely from the conditional links.

Treating the formalization and evaluation of analogical reasoning, itself used in a variety of disciplines to support that a given hypothesis is plausible, Paul Bartha (UBC, Vancouver) identified two distinct interpretations of plausibility. On a probabilistic interpretation, the hypothesis has non-negligible probability, while on a non-degree or modal conception, the hypothesis is worthy of serious investigation. Though the former has dominated theories of analogical reasoning (including computational models), as he argued, the study of particular analogical arguments suggests that the modal interpretation is fundamental. Particularly an analysis in terms of ranking functions holds promise because it combines relevant features of both interpretations: ranks are both modal and a matter of degree. A further attraction of this analysis is that an analogical argument typically delivers an "order of magnitude" probability for its conclusion, and this corresponds to one interpretation of ranking functions.

Gillman Payette (UBC, Vancouver) considered the question whether deontic logics based on preference orders between possible worlds, or states, would allow for conflicts between axiological ideals and what one ought to do? To formalize this conundrum, he presented both 'ought to do' and 'ought to be' obligation sentences. Here, what ought to be is given by propositions that are evaluated as being strictly better than their negations, relative to an ordering relation that is "lifted" from worlds to sets of worlds. And what one ought to do are best actions according to the lifted relation. Since the details of lifting the preference order now decides the initial question, what is normally viewed as a change in (moral) values can alternatively be viewed as risk-averse decisions over what one can do.

Frank Zenker (Konstanz) presented joint work with Justine Jacot and Emmanuel Genot (Lund) showing that models of agents who argue strictly in accordance with the norms of deductively valid inference can be obtained from suitably specifying agents' preferences, and from endowing them with realistic strategic reasoning-principles. The modeling assumptions, moreover, prove to be consistent with the normative Pragma-dialectical theory of an argumentative process that aims at resolving a differences of opinion on the merits of the case. Their work offers a new avenue of naturalizing classical logic, and makes it natural to inquire how non-deductive logics could similarly be induced by expressing constraints on the argumentative process, and its content, in terms of players' preferences and abilities.

John Woods (UBC, Vancouver) presented localized inconsistency in a theory as a deeply neglected problem for logics of defeasible reasoning, pointing out that human agents (e.g., with respect to deep memory) as well as mathematical or scientific theories, but also useful technological constructs such as databases, are known to be inconsistent. In proof-theoretic terms, this means every sentence in a globally inconsistent theory is a theorem, including in each case its own negation. Rather than advocate barring inconsistency by taking "protectionist measures" for consistency to save such systems from their seeming epistemic impotence, Woods urged that we study the reasoning that nevertheless manages to extract the good that theories do, even if—as often is the norm—every one of their sentences is provable.

The organizers, Frank Zenker and Thomas Müller, acknowledge funding for this event from the University of Konstanz's Internationalization Fund, the Department of Philosophy, the Department of Law (via Matthias Armgardt), and the Volkswagen Foundation.

Frank Zenker
Konstanz

Explanation and Evidence of Mechanisms Across the Sciences, 16 May

On 16th May, a workshop was held at the University of Kent focusing on the notions of explanation and evidence of mechanisms. The workshop was an activity of the research project Grading evidence of mechanisms in physics and biology, supported by the Leverhulme Trust.

Jürgen Landes from Munich Center for Mathematical Philosophy kicked off the workshop with a presentation on evidence aggregation, with special focus on applications in pharmacology. Landes presented a bayes net framework for representing evidential relations of heterogeneous datasets vis-à-vis causal hypotheses, and applied this to a case study of detecting adverse drug reactions. He employed this framework to defend the philosophical (and commonsense) view that more evidence from diverse sources is better than evidence from a single source; a view partly questioned by arguments raised by Bovens and Hartmann.

Next up was Gry Oftedal from the University of Oslo. Oftedal's presentation considered intervention and explanation in development of targeted cancer treatments. She argued that cancer researchers employ different explanations for cancer depending on the epistemic and pragmatic aims of their research, and these explanations in turn support different classifications of cancer. In one type of case the explanation and corresponding classification is based on the etiology as well as the location and physiology of a tumor, while in the other case it is based on a mechanistic explanation of the action of effective treatment interventions. It appears that, in principle, these classifications may even cross-classify cancer types, which might warrant rethinking of the classificatory practices.

After a brief break, Julian Reiss from Durham gave a talk on reasoning about targets based on surrogate sources of evidence. Reiss argued that contrary to what the recent interest in the problem of extrapolation might suggest, using evidence produced in one system or context to support inferences about

a different system or context does not involve any special "indirect" inference (extrapolation) that requires its own philosophical analysis. Instead he presented an account of evidence appraisal based on hypothetico-deductivism, and argued that this framework accounts for both "direct" and "indirect" evidential reasoning in the same manner; no separate account of the problem of extrapolation would thus be needed.

After lunch, the participants reconvened to hear a talk on metaphysics of mechanisms by Beate Krickel from Bochum. Krickel started by presenting Illari & Williamson's activity-based definition of mechanisms and highlighting its strengths. She then proceeded to point out some problems related to the central concept of activity, arguing that activities are not up for the job they have been ascribed in the literature on mechanistic explanation, as well as questioning the claim that activities are irreducible to other metaphysical categories such as laws. According to Krickel, an account of causality and mechanistic explanation based on the notion of activity faces a similar problem to the one plaguing Salmon's process theory of causal explanation; the inability to account for explanatory relevance.

Next in the program was a joint talk by Phyllis Illari and Brendan Clarke of UCL. Illari and Clarke discussed the problems of navigating the complexity and sheer number of detail one faces when appraising mechanistic evidence, arguing for the need of systematic heuristics for dealing with these problems. Illari and Clarke then discussed how Wigmore charts—an informal graphical representation of evidential relations used in law could be employed to bring structure to reasoning about mechanistic evidence in medicine. After a short break, Jaakko Kuorikoski from Helsinki capped off the workshop by presenting a mechanism-based approach to weighing evidence. Kuorikoski presented a framework in which evidence is evaluated in relation to a given explanandum, such that the importance of a piece of evidence is based on its ability to rule out members from a set of hypothetical mechanistic explanations for that explanandum. He then illustrated how this account works in social scientific explanation.

Veli-Pekka Parkkinen
University of Kent

Philosophy of Mathematics: Truth, Existence and Explanation, 26–28 May

The second conference of the Italian Network for the Philosophy of Mathematics (FilMat) took place at the University of Chieti-Pescara from the 26th to the 28th of May 2016. The network, established in 2012, gathers together Italian scholars in the philosophy of mathematics and closely related disciplines. This conference came after the first FilMat international conference (held in 2014 at San Raffaele University in Milan) and an initial pilot conference (held in 2012 at the Scuola Normale Superiore in Pisa) and was organized by M. Piazza (Chieti-Pescara), G. Pulcini (Campinas) and P. Graziani (Urbino).

The conference hosted five invited speakers (A. Varzi, CNYU; L. Incurvati, Amsterdam; V. Halbach, Oxford; M. Antonutti Marfori, IHPST and Salzburg; E. Moriconi, Pisa) and fourteen contributed talks by twenty international scholars. The talks focused around the three main themes of the conference, viz. truth, existence, and explanation in mathematics.

As regards truth, contributors mainly focused on truth-theoretic deflationism and its desiderata. Advocates of deflationism often require an adequate theory of truth to be (i) conservative but (ii) not relatively interpretable in the base theory. However, the non-conservativity requirement might be hard to sarisfy (C. Cieslinski, M. Lelyk, and B. Wcislo, Warsaw). J. Heylen and L. Horsten (KU Leuven, Bristol) showed, for instance, that a disquotational theory of truth is not conservative even on negative free first-order logic. A. Strollo (SNS, Pisa) then suggested that the philosophical rationale behind conservativity can survive beyond the conservativity requirement itself. On the other hand, an examination of the non-interpretability requirement across base theories would allow us to conclude that it is also ill-motivated (C. Nicolia, Munich).

As regards existence, two talks related to Hilbert's philosophy, and one to Frege's. F. T. Doherty (Cambridge) reframed the Hilbert-Frege controversy in terms of conceptual priority of consistency over existence. More technical considerations on Hilbert's first-order and second-order axiomations of geometry were provided by J. Baldwin (Illinois-Chicago). On the other hand, Frege's views on the ontology of arithmetic heavily depends on the legitimacy of introducing objects by abstraction principles. But abstraction is notoriously risky. For instance, the abstraction principle stating that (a) two well-orderings are represented by the same ordinal iff there is a one-one preserving correspondence between them is inconsistent, since it allows us to reproduce the Burali-Forti paradox. S. Florio and G. Leach-Krouse (Kansas State) provided a new analysis of the paradox in four additional assumptions: (b) second-order comprehension; (c) the conception of ordinals as objects; (d) quantification over absolutely all ordinals; (e) quantification over absolutely all relations. They then explored a "no-class" solution to the paradox, thereby rejecting (c).

As regards explanation, one central topic was Alan Baker's "Enhanced Indispensability Argument" for platonism (EIA). Almost all the examples in support of EIA are "optimality" explanations, viz., mathematical solutions to optimization problems (R. Knowles, Leeds); D. Molinni (Roma Tre) argued that a newly-introduced distinction distinction between mathematical explanation of particular phenomena and mathematical explanations of whole scientific theories further supports EIA's premises, thus producing an 'enhanced Enhanced Indispensability Argument'. Finally, Antonutti defended a form of mathematical naturalism in its connections to EIA. Two other themes related to mathematical explanation were explanatory mathematical proofs (J. Salverada, University College) and the ("unreasonable") applicability of mathematics to physics, in its relation to the applicability of mathematics to mathematics itself (M. Ginammi, SNS).

Some speakers focused on justification (within or without) mathematical practice, including mathematical depth and mathematical understanding (J. Folina, Macalester College). On this respect, a well-known distinction concerns "intrinsic" (viz., concepts-related) vs. "extrinsic" (viz., consequences-related) justification of an axiom; N. Barton (Birkbeck College), C.Ternullo (Kurt Gödel Research Center) and G. Venturi (CLE Universidade de Campinas) claimed, however, that justification in set theory (intrinsically) involves both intrinsic and extrinsic aspect at once. L. San Mauro (Vienna Univ. of Techn.) engaged instead in a practice-based analysis of the Church-Turing Thesis in computability theory.

The last four invited talks tackled issues in the philosophy of logic. Varzi shed new light on the competition between model-theoretic and proof theoretic approaches describ-

ing some proof-theoretic characterizations of all (and only) the non-tautologies of classical logic. Halbach presented a "substitutional" account of logical validity, according to which a sentence is logically valid iff all of its substitutional instances are true. Incurvati critically assessed the so-called "overgeneration" argument against the logicality of second-order logic. Finally, Moriconi discussed the opposition between provability and the notion of truth in light of incompleteness theorems.

The conference explored a wide variety of topics in the philosophy of mathematics and logic, and proved how lively the debate in this area is at present, and how many novel directions are being investigated. The FilMat Network, which also welcomes expressions of interest for membership from scholars in the philosophy of mathematics, has already announced its third international conference, to be held in Palermo in 2018. Follow the link for information about the FilMat Network.

Luca Zanetti
Institute for Advanced Studies, Pavia

Methodologies in Science, 10 June

This year the theme of the spring term workshop of the research group in theoretical philosophy at the University of Copenhagen was methodologies in science. The aim of the workshop, which was organized by Thor Grünbaum and Sune Holm, was to introduce the audience to some current themes concerning our understanding of methodologies in the sciences, in particular the "special sciences."

The day began with a stimulating presentation by Francesco Guala (University of Milan) on anti-naturalism in the philosophy of social science. In his talk Guala provided an overview of some of the main arguments that have been advanced for an anti-naturalist approach. In particular Guala examined what he identified as "the classic anti-naturalist strategy", which is to identify a feature of social reality that is overlooked by current social science. Guala provided incisive criticism of exemplifications of this strategy by those who argue that social phenomena are causally or constitutively dependent on representation. An important take-home message was that philosophers of social science should pay much more attention to practicing social scientists. It is, Guala argued, the social scientists who are best positioned to assess the efficacy of specific methods and philosophers who want to improve the methodology of social science must engage with scientific practice at the same level of analysis.

The next three speakers focused mainly on issues arising in the philosophy of biology. Lucy Holt, (University of Copenhagen) provided an engaging discussion of the notion of internal teleology. Her talk outlined attempted definitions of internal teleology and its contrast, external teleology, with a view to assessing the relevance of these notions in evolutionary and and synthetic biology. Holt concluded by raising several interesting questions concerning the relationship between internal and external teleology in artificial organisms. Sune Holm outlined how engineering methods are currently being introduced into biological research aiming to enable rational and modular design of living systems. Holm suggested that the methods of synthetic biology indicates the basis of criticism of interventionist accounts of causation in biology. In her talk "Do measurements of specificity tell us about causal importance in living systems?" Gry Oftedal (University of Oslo) discussed the introduction of measurements of the specificity of causal relations aiming to, e.g., quantify the specificity of genes and other causes in living systems. Oftedal developed detailed criticisms of recent attempts at quantifying causal specificity, and pointed to some of the limits of what specificity measurements can tell us. Finally, the day ended with a more general talk by Samuel Schindler (Aarhus University) on "Prediction and testability." Schindler argued that testability is an extremely weak condition for scientificity. He then suggested that some of Popper's central motivations for identifying testability as a necessary condition for scientificity are in fact better described as concerns with the issue of ad hocness. Thus, Schindler suggested that the more central issues concern what it means for a hypothesis to be ad hoc.

The Copenhagen University Research Group in Theoretical Philosophy will host its next workshop in November 2016.

Sune Holm
University of Copenhagen

Calls for Papers

Big Data and Business Analytics Ecosystems: special issue of *Information Systems and e-Business Management*, deadline 16 October.
The Background of Constitutive Rules: special issue of *Argumenta*, deadline 10 November.

What's Hot in ...

Uncertain Reasoning

Logic and probability are closely related, but they are not the same. One difference which is easy to appreciate is the lack of compositionality of the latter. Suppose θ and ϕ are two sentences in some (propositional) language and v is map from the set of sentences to the binary set $\{0, 1\}$. We say that a connective $*$ is compositional if $v(\theta * \phi)$ is a fixed function of $v(\theta)$ and $v(\phi)$. This condition is satisfied by the semantics of classical logic, which is therefore compositional. For example conjunction satisfies

$$v(\theta \wedge \phi) = v(\theta) \cdot v(\phi)$$

where $\cdot$ is the standard product. Many-valued logics are also compositional, so this is not a property which uniquely characterises classical logic.

Probability, as we anticipated, is not compositional, and indeed it should not be, as pointed out by J. Paris (1994: *The Uncertain Reasoner's Companion*, Cambridge University Press). To see that it is not always compositional, consider two probability functions P and Q defined on (the atoms of) the propositional language $L = \{p, q\}$ as follows:

- $P(p \wedge q) = P(p \wedge \neg q) = P(\neg p \wedge q) = P(\neg p \wedge \neg q) = 1/4$;
- $Q(p \wedge q) = Q(\neg p \wedge \neg q) = 1/2; Q(\neg p \wedge q) = Q(p \wedge \neg q) = 0$.

The additivity of P leads us immediately to see that P and Q agree on p and on q (i.e. $P(p) = Q(p) = P(q) = Q(q) = 1/2$) despite the fact that we just set P and Q to disagree on $p \wedge q$.

To see that this failure of compositionality is indeed desirable, let us reason by contraposition. Suppose P is such that $P(\theta) = P(\neg\theta) = 1/2$. If P were compositional, the following would have to hold by substitution of equal values

$$P(\theta \wedge \theta) = P(\theta \wedge \neg\theta),$$

hardly an acceptable feature of a measure which claims to formalise rational reasoning!

That being said, logic and probability do share a number of common properties, both formal and conceptual. Among the latter, coherence stands out. Classical logic (and many of its generalisations) builds directly on the notion of coherence. Semantically this takes the guise of *satisfiability*. In the simplest possible terms, a sentence is satisfiable if there exists valuation which maps it into 1. This generalises to sets in the obvious way: a set of sentences is satisfiable if there exists a valuation mapping all its elements into 1. In pictorial language, the elements of a satisfiable set *cohere*.

Coherence lies also at the foundation of the subjective interpretation of probability, as proved among others by Bruno de Finetti. Coherent is an assigment of betting odds which does not lead the bookmaker who published it to face the possibility of sure loss, i.e. loss which they will suffer independently of how the uncertainty is eventually resolved. The so-called Dutch Book Argument shows that for odds to be coherent it is necessary and sufficient that they satisfy the standard axioms of probability.

Making analogies among analogies, it is rather natural to ask whether coherent betting odds cohere in the logical sense recalled above. In other words, suppose two bookmakers publish coherent books (in the sense of de Finetti); will they be jointly coherent? This question belongs to the metamathematics of probability logic, and it mirrors the meta-logical result of joint consistency, which identifies the conditions under which the union of two consistent sets is itself consistent.

The conditions under which a suitably defined *product* of coherent books is itself consistent are identified in the recent paper by D. Mundici (2016: Coherence of de Finetti coherence, *Synthese*, Online First, 01 June 2016). The author motivates this interesting result by pointing out that "were it not the case it would raise a question mark over the utility of de Finetti's notion of coherence". And it would indeed. The paper does not develop the meta-logical route outlined above. However it develops an extremely interesting and self-contained analysis of the algebraic, topological, set-theoretic and measure-theoretic confluence of concepts and methods which results in de Finetti's notion of coherence.

Hykel Hosni
Philosophy, University of Milan

Evidence-Based Medicine

It was reported in the news last month that the consumption of very hot beverages has recently been classified as probably carcinogenic to humans. It was also reported there that drinking coffee was regarded as unclassifiable as to its carcinogenicity to humans. These classifications were determined by a working group of scientists at the International Agency for Research on Cancer (IARC).

How did the working group arrive at these classifications? A subgroup considered the evidence from studies of cancer in humans. This involved evaluating the strength of all the relevant epidemiological studies taken together. As such, this subgroup seems mainly concerned with evidence of a correlation between the exposure and cancer in humans. It evaluates the studies taken together against the following criteria: the evidence is sufficient to establish that the exposure causes cancer in humans; there is limited evidence that the exposure causes cancer in humans; there is inadequate evidence; there is evidence suggesting lack of carcinogenicity. In the case of drinking very hot beverages, the subgroup concluded that there is limited evidence in humans that drinking hot beverages causes cancer. Another subgroup considered the evidence from statistical studies of cancer in experimental animals, and evaluated the strength of this evidence according to the same criteria. It the case of drinking very hot beverages, this subgroup also concluded that there is limited evidence in experimental animals that drinking very hot beverages causes cancer. The conclusions of both subgroups are then combined according to a set of guidelines in order to classify the exposure into one of five categories, in descending order of hazard: the exposure causes cancer in humans; the exposure probably causes cancer in humans; the exposure possibly causes cancer in humans; the exposure cannot be classified as to its carcinogenicity; the exposure is probably does not cause cancer in humans. The guidelines suggest that an exposure for which there is limited evidence of carcinogenicity in both human and experimental animal studies should be classified as *possibly* carcinogenic to humans.

However, the consumption of very hot drinks was classified as *probably* carcinogenic to humans. The reason for this is that there was another subgroup that considered mechanistic and other relevant data. This subgroup seems mainly concerned with evidence of a mechanism linking the exposure to cancer. In the case of drinking very hot beverages, the subgroup concluded that it was biologically plausible that drinking very hot beverages was linked to cell injury, and that in turn this may lead to cancer. These considerations contributed to the overall classification of drinking very hot drinks as probably carcinogenic to humans. A summary of the motivation behind this classification has now been published in an article in *The Lancet Oncology*. The overall report will be published as a volume of the IARC *Monographs*.

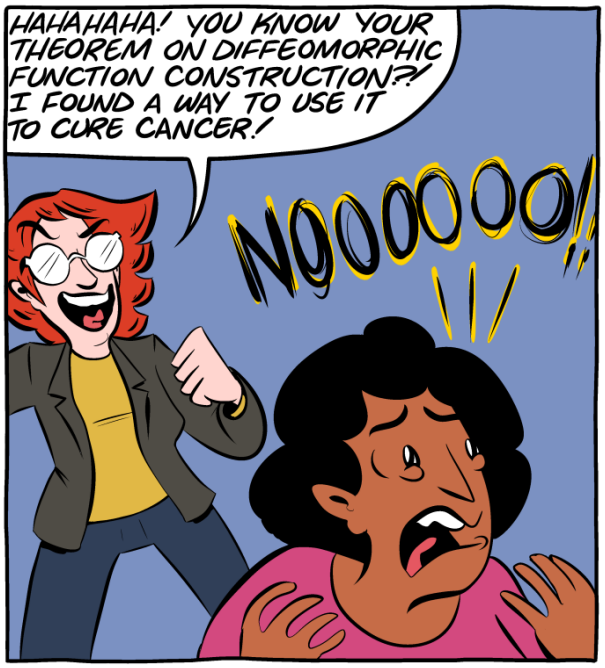
The overall classification of the probable carcinogenicity of drinking very hot drinks seems to be based on both evidence of correlation and evidence of a mechanism. As a result, this practice may be seen as providing support for the claim that there is an important role for evidence of mechanisms in medicine.

Michael Wilde
Philosophy, Kent

Events

July

LDA: Longitudinal Data Analysis, University College London, 30 June–1 July.
CPR: Contemporary Perspectives on Reductionism, Prague, 30 June–1 July.
AAL: Australasian Association for Logic Conference, Melbourne, 30 June–2 July.
PM: Perspectival Modelling: Pluralism and Integration, University of Edinburgh, 2–3 July.
IH&PoS: Integrated History and Philosophy of Science, University of Edinburgh, 3–5 July.
SDH: Webinar: An introduction to survey data on health, online, 4 July.



Funtime Activity:
Forcibly converting pure mathematicians into applied mathematicians.

EEN: European Epistemology Network Meeting, Paris, 4–6 July.
BSPS: The British Society for the Philosophy of Science Annual Conference, University of Cardiff, 7–8 July.
SRAI: Statistical Relational Artificial Intelligence, New York City, 11 July.
FOMUS: Foundations of Mathematics: Univalent Foundations and Set Theory, Bielefeld, Germany, 18–23 July.
NRA: Knowledge, Reasons, and Action, Erlangen University, Germany, 21–22 July.

August

BBD&I: Bayes, Big Data, and the Internet, Villa del Grumello, Como, Italy, 29 August–2 September.
P&QM: Probability and Quantum Mechanics with G. Bacciagaluppi, Utrecht University, 1–5 August.
ISCB: Conference of the International Society for Clinical Biostatistics, Birmingham, 21–25 August.
PMS: Post-Model Selection, Leuven, 22-23 August.
EJ: Workshop on Expert Judgment, University of Strathclyde, 26 August.
ECAI: European Conference on Artificial Intelligence, The Hague, Netherlands, 29 August–2 September.

Courses and Programmes

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 & 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.

Jobs and Studentships

Jobs

Lecturer: in Epistemology, University of Glasgow, deadline 3 July.
Assistant Professor: in Applied Philosophy, University of Nottingham, deadline 7 July.
Lecturer: in Statistics, University of Kent, deadline 10 July.
Research Assistant: in Medical Statistics, University College London, deadline 12 July.
Lecturer: in Statistics, University of Glasgow, deadline 17 July.
Post-doc: in Knowledge Beyond Natural Science, University of Stirling, deadline 18 July.
Lecturer: in Biostatistics, Universities of Liverpool, Manchester, Lancaster and Bangor, deadline 26 July.
Research Fellow: in Statistics and Astrophysics, Monash University, deadline 31 July.
Post-doc: in Machine Learning, University of Skövde, Sweden, deadline 12 August.
Professorship: in Statistics and Data Mining, University of Melbourne, deadline 31 August.
Professorship in Theoretical Philosophy: The Frankfurt School of Finance & Management, deadline 1 September.

Studentships

PhD position: in Data Science, IMT School for Advanced Studies, Italy, deadline 13 July.
PhD position: in Statistics, Durham University, deadline 20 June.