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CONTENTS

Editorial	
Features	
News	
What's Hot in ...	
Events	
Courses and Programmes	
Jobs and Studentships	

EDITORIAL

Last time I guest edited *The Reasoner* I said a few, admittedly rather inconclusive, things concerning the possibility for scientific realists to substantiate their 'no miracle' intuition—according to which the success of science must be due to the (approximate) truth of scientific theories—in novel ways. In particular, I wondered whether it would be at all possible for them to exploit something like a mixture of what Whewell called the 'consilience of inductions' and the idea of truth-conduciveness of coherence. Now four years have passed and, I must confess, things still look quite confused to me. Some fellow reasoners might have noticed that I have written a couple of pieces in defence of scientific realism in past issues. Indeed, I do think that scientific realism is something like a 'default position' in the debate, as we naturally tend to explain our successes in interacting with the world on the basis of the correctness of our hypotheses. At the same time, however, I still feel that those not intuitively convinced by scientific realism will find no compelling reasons for sub-

scribing to it—at least not by looking at the arguments that can be found in the extant literature. As a matter of fact, it has recently been held that the whole realism/antirealism dispute boils down to a clash of subjective probabilities—in particular, the prior probabilities that various people assign to the truth of any given scientific theory. This might well be the case, but I am still unsure: Is there a basis for claiming that, in fact, this is not all there is to say, and the scientific realism debate is not bound to be fruitless? If so, can realists tip the philosophical balance in their favour?

Besides, one's answers to these questions surely have larger repercussions. In particular, the concept of 'scientific progress' cannot but take different forms depending on whether realism, antirealism or agnosticism is endorsed. In relation to this, let me say just a few more words on what happened since our last meeting. I have moved back to my hometown Rome, where I have been lucky enough to find a position as research fellow and junior lecturer. I was also lucky enough to be awarded—as part of a larger network devoted to the study of 'the structure and dynamics of knowledge and cognition'—a national research grant for a project focussing exactly on the notion of progress in science. Thus, for better or for worse, it looks like I will have to struggle more with the above problems in the near future... On the other hand, other bright people around the world are doing the same, so there might be reason for optimism. Perhaps, we will be able to report on some results in *The Reasoner* in the coming months!

This leads us to our guest for this month. One of those involved in the above mentioned research project, indeed no less than the head of the network, is Vincenzo Crupi. We've known



each other for more than 10 years, and Vincenzo too is now back in Italy, working at the University of Turin. In view of his expertise and outstanding contributions on a number of topics which are certainly of interests for our readers, it is a pleasure that Vincenzo has agreed to answer a few questions.

MATTEO MORGANTI

Philosophy, University of Rome 'Roma TRE'

FEATURES

Interview with Vincenzo Crupi

Dr. Vincenzo Crupi is an assistant professor at the Department of Philosophy and Education, University of Turin, and also a visiting research fellow at the Munich Center for Mathematical Philosophy, Ludwig Maximilian University. His research interests are in the general philosophy of science, formal epistemology, and the psychology of reasoning. He has worked and written extensively on confirmation, judgment and decision-making, the conjunction fallacy and other themes. Check out his brand new [Confirmation](#) entry in the Stanford Encyclopaedia of Philosophy!

Matteo Morganti: Hi Vincenzo, I am very happy that you accepted my invitation to be interviewed for this issue of *The Reasoner*. How are things, dividing your (academic) life between Turin and Munich?

Vincenzo Crupi: Surely I have to thank you for the invitation! Initially, I was really surprised by the idea, given the amazing list of brilliant people that have been interviewed here. As for your question, I was lucky enough to be recruited at the Munich Center for Mathematical Philosophy when it was founded by Hannes Leitgeb back in 2010. I'm sure that by now most readers of *The Reasoner* know one way or another just how fantastic that place is. Only a permanent appointment in my hometown could have made me leave, I guess, and that's what I got in Turin after a while. Another lucky shot, for it's also one of the best places for philosophy in Italy, it turns out! But I retained a solid connection with the MCMP community: for the kind of research work I'm interested in, it's a huge opportunity.

MM: Some of your work might be already familiar to readers of *The Reasoner*, but could you tell us a bit about it, and how you got to do what you do today?

VC: In 2004 I was working in a cognitive science lab in Rovereto. Matteo Motterlini had put me at work on the notions of evidence, rationality and error in medicine in light of the empirical study of judgment and decision-making. There I met experimental psychologist Katya Tentori: she was running a pilot study on how people judge evidential support, in collaboration with Dan Osherson from Princeton. They had a now classic paper by Branden Fitelson as a key background reference. I joined that project, and kept working on the logic and psychology of inductive confirmation ever since. The behavioural investigation of how the assessment of evidential sup-

port affects reasoning in various ways has been particularly innovative, I think. But the theoretical side was no less engaging: in a series of contributions with Katya and other great collaborators (Michel Gonzalez, Roberto Festa and, more recently, Nick Chater), I identified a confirmation measure which generalizes deductive-logical relationships in a uniquely coherent way and then devised a general approach for characterizing axiomatically alternative probabilistic models of confirmation and closely related notions. Meanwhile, I also kept working on various aspects of rationality in the medical domain.

MM: It is manifest that you're sympathetic to a formal approach to philosophy. Is it something that you consider necessary in general, or only for a proper subset of the questions that you take to be genuinely interesting and relevant in philosophy? More generally, what is your view on the methodology of philosophy, and the possibility for philosophers to truly contribute to the 'growth of knowledge'?

VC: Difficult questions. My own path in philosophy has been far from straightforward. You know that well, don't you? When we first met at the LSE almost twelve years ago, you were already on your track with scientific realism, while my background had been mostly studying continental philosophy (indeed, hardcore stuff such as Heidegger!) Since then, I happened to learn from many smart people working with different styles and approaches. Eventually, the experience at the MCMP helped me to fully appreciate how formal methods can be applied effectively to a wide range of philosophical subjects—wider than is often assumed. Establishing a firm formal framework enables coherent strands of research to arise and yield genuine progress (Stephan Hartmann once approvingly called this "normal philosophy", I think). This is not to deny the inherent risk of scholasticism, though. Moreover, many kinds of good informal philosophical work remain of crucial importance anyway. So, you see, given my own background, I can't be at ease with any one-sided view of these issues: telling scholars what they ought not to do never pays off much, it seems to me. Precisely for this reason, by the way, I was so happy to get involved as a section editor of *Ergo*, a new, open access and entirely general journal of philosophy recently launched by Franz Huber and Jonathan Weisberg. I'm sure that *Ergo* will soon become a venue for high-quality philosophical work across the board (check out yourself at [here](#), and consider submitting!).

Having said all that, I did shape my inclinations over time. With confirmation and evidential support, my collaborators and I tackled a traditional issue in the philosophy of science and got new insight (and new challenges, too) by a combination of formal and behavioural methods. As far as I can see, this approach is increasingly popular and is contributing to the study of other key topics in interesting ways (conditionals, explanation, coherence, and more besides). Much research of this kind is being done right now within [New Frameworks of Rationality](#), an important program funded by the Deutsche Forschungsgemeinschaft since 2010. It's a network of twenty or so units across Germany and beyond, and the interaction among scholars from philosophy, psychology and computer science is intense and constructive. We even have a series of workshops devoted to how experimentally testable predictions and models of epistemological interest can inform each other. The next of these events—called [Operationalization 2013](#)—will be held in Freiburg in October (I happen to feature among the organizers, but Henrik Singmann, Marco Ragni, and Jan Sprenger were the real driving force). Future contributions of philosophy



to the 'growth of knowledge', as you say, cannot be secured or predicted, but can be facilitated to some extent, and New Frameworks is a great example in this respect.

MM: In the introducing editorial for this issue of *The Reasoner*, as well as in my previous one (in 2009!), I pointed at possible ways in which the dispute about scientific realism, the issue about whether coherence is truth-conducive and 'old' ideas such as Whewell's consilience of inductions might (or might not) be put together so as to provide a new basis for scientific realism. In a nutshell, I was (and still am...), wondering whether the realist 'no miracle' intuition could be vindicated by insisting on, but also extending, reformulating and refining, the idea of convergence of independent sources, theories etc. towards the same claims. Consider, for instance, Wesley Salmon's well-known claim that it cannot be a coincidence that there are so many *independent* ways of calculating Avogadro's number, all giving the same outcome. What are your views concerning the scientific realism/antirealism issue? Do you have any thoughts on the above (hypothetical) 'convergence' arguments?

VC: I find these suggestions sound fascinating. I don't have much to add myself, but I think they're at least consonant with some probabilistic analyses of coherence and explanatory power. A key point here is of course the role of independence, as in popular scenarios concerning more or less independent 'sources of information' (witnesses, tests, and the like) that the Bayesian framework seems especially well-equipped to handle. (Greg Wheeler has recent work along these lines, where many earlier results are also discussed.)

As for my own views, maybe I'd better call myself an 'anti-antirealist'. As a teacher, for instance, I noticed that, at least initially, many of my students struggle to see why one would want to confront the hurdles of working out a coherent antirealist position. This may well appear naïve, but I think it reflects a real issue, which arises if one looks at the sciences themselves. Antirealist standpoints do not seem to play much of a role for research in fields as diverse as, say, medicine and mathematics, while they remain fairly influential in certain areas of physics and cognitive science. But then, why is their prevalence so uneven? My impression is that antirealist attitudes are employed in a rather strategic fashion in real science and flourish under quite specific conditions. What are these conditions? I find the issue philosophically interesting, and yet I'm not aware of any principled answer.

MM: In my introduction, I mentioned our national grant on the structure and dynamics of knowledge and cognition. As the project's principal investigator, what are your expectations? Could you tell the readers of *The Reasoner* what your research unit will be looking at?

VC: Let me first say that I'm proud of this project for some very concrete reasons, namely, that it'll allow up to five young scholars across the country to do their research work in a moment in which the situation in academia is otherwise unfavourable. All the people involved are very good. A major trait that they share is the use of innovative methods of philosophical investigation in which the connection with the sciences (mathematical, physical, and/or behavioural) is real and strong. So I expect highly valuable work to arise from the project as a whole.

The research unit that I will lead in Turin will focus on a number of related notions (probability, confirmation, information, and truthlikeness) to analyse how they interact and how they give rise to different conceptions of epistemic utility, both

old and new. And, again, we'll try to look closely at cognitive science for inspiration and applications. Currently, I'm particularly interested in the connections between Bayesian confirmation and information theory, broadly construed. This is still part of collaborative work with Katya Tentori, Jonathan Nelson (a leading expert on these issues) and Björn Meder from the MPI in Berlin. Let me give you a hint by means of a toy example that I like. Suppose that a card was drawn from a standard deck and kept hidden from you. You're interested in the suit of this card, but can only be told (truthfully) about its colour. How useful (to wit, informative) would this query (test, experiment) be for your purposes? Alternative scenario: this time you want to know about the colour of the card drawn, and can get to know what the suit is. How useful would this latter query be for the purposes that are now involved? Many people (a good majority in an informal sample of logicians and psychologists of reasoning that I got at a recent workshop) find that the query concerned is clearly more informative in the second case than in the first one relative to the respective epistemic target. Yet standard information theory, based on the expected reduction of Shannon entropy, tells you that there is *no difference* between the first and the second scenario: a disturbing implication, to my mind (and one that some find just astonishing, at first.). In turn, this popular account of the usefulness of queries can be seen as grounded on one particular (and not very attractive) Bayesian measure of confirmation (so-called 'log probability ratio'). And so, here's a research question that I find intriguing: would it be possible to draw on the pool of the major alternative probabilistic measures of confirmation (they're now very well understood!) to come up with a model which gets cases such as this one right, while retaining various otherwise attractive properties? And how would such a model fare in other theoretical and behavioural applications?

MM: Now for the less ambitious questions... Is there anything particular that you would recommend our reasoners to read in the coming academic year (not necessarily a philosophical book or article)? Or any 'hot topic' for them to follow?

VC: Here's a handful of sparse and diverse remarks. This summer I'm finally taking my time to read *Thinking Fast and Slow* by Daniel Kahneman. The book hardly contains any bit of either academic philosophy or mathematical formalism, and yet I see it as kind of a must read for anyone with an interest in reasoning. For something of a very different kind, let me advertise a forthcoming special issue of *Erkenntnis* arising from last year's edition of the Formal Epistemology Workshop (held in Munich). It took us a while to put it together (it's edited by Ole Hjortland, Florian Steinberger, Branden Fitelson and me), but it will include a collection of great papers on rationality, decision theory, inductive probability, default reasoning, and more still. In a larger perspective, let me also add something concerning 'hot topics:' it seems to me that the issue of how various epistemic norms of inference and belief (full, comparative, and graded) can be justified and connected is attracting more and more of the best minds around in philosophy, yielding new exciting results and engaging challenges. That's something I'd definitely keep an eye on. Oh, I almost forgot, there's a brand new philosophy of science book out from Palgrave Macmillan that I've found just brilliant: it's called *Combining Science and Metaphysics*. I can't quite remember the name of author right now... but no, wait, it's you!

MM: Well, thanks a lot for this bit of advertisement, and for the whole interview! I hope we'll hear from you soon (of

course, when all problems with scientific realism and information theory will have been finally solved. . .).

VC: Then it might well take a while. . . But thank you very much for now, anyway!

Against Phenomenal Conservatism

Huemer endorses Phenomenal Conservatism (2007, “Compassionate Phenomenal Conservatism”, *Philosophy and Phenomenological Research*, 74, pp. 30–55):

(PC) If it seems to S that p , then, in the absence of defeaters, S thereby has at least some degree of justification for believing that p .

The question is whether appealing to *appearances* or *seemings* is a trustworthy method of fixing belief (MFB)? I argue that the phenomenal conservative is committed to the claim that appealing to appearances is an untrustworthy MFB.

Suppose that, independently of each other, it seems to S_1 that p and to S_2 that not- p . On (PC), in the absence of defeaters, S_1 has at least some degree of justification for believing that p and S_2 has at least some degree of justification for believing that not- p . In this case, the same MFB provides at least some degree of justification for both p and not- p . If the same MFB provides at least some degree of justification for contradictory beliefs, then that MFB is untrustworthy. The phenomenal conservative is thus committed to the claim that appealing to appearances is an untrustworthy MFB. More explicitly:

1. (PC) [Assumption for *reductio*]
2. It seems to S_1 that p and it seems to S_2 that not- p , independently of each other. [Premise]
3. Therefore, in the absence of defeaters, S_1 has at least some degree of justification for believing that p and S_2 has at least some degree of justification for believing that not- p . [from (1) & (2)]
4. If an MFB provides at least some degree of justification for contradictory beliefs, then it is untrustworthy. [Premise]
5. *Appealing to appearances* provides at least some degree of justification for contradictory beliefs. [from (3)]
6. Therefore, appealing to appearances is an untrustworthy MFB. [from (4) & (5)]

Supposing (PC), then, commits the phenomenal conservative to (6), which is at odds with (PC), in cases where it seems to S_1 that p and to S_2 that not- p .

The phenomenal conservative might try to block the inference to (6) by denying either (2) or (4). Against (2), he might insist that appealing to appearances doesn't justify contradictory beliefs. Against (4), he might insist that a method of fixing belief can be trustworthy even if it justifies contradictory beliefs. So I will say a few words in support of (2) and (4).

In support of (2), consider how appealing to intellectual (as opposed to perceptual) appearances (Brogaard, forthcoming, “Intuitions as Intellectual Seemings”, *Inquiry*) often leads to contradictory philosophical beliefs. In response to a hypothetical case, to one philosopher it seems that p , whereas to another philosopher it seems that not- p . For example (Mizrahi, 2013, “More Intuition Mongering”, *The Reasoner*, 7, pp. 5–6):

- To Jackson (1982, “Epiphenomenal Qualia”, *Philosophical Quarterly*, 32, pp. 127–136) it seems that Mary learns something new, whereas to Dennett (1991, *Consciousness Explained*, Boston: Little Brown) it seems that she doesn't.
- To Searle (1999, “The Chinese Room”, in Wilson and Keil (eds.), *The MIT Encyclopedia of the Cognitive Sciences*, MIT Press) it seems that the person in the room doesn't understand Chinese, whereas to Hauser (2002, “Nixin' Goes to China”, in Preston and Bishop (eds.), *Views Into the Chinese Room*, NY: OUP) it seems that he does.

These examples show that the scenario depicted in (2) is not only plausible but also common.

In support of (4), suppose (Mizrahi 2013, pp. 5–6):

I use a Litmus Test in order to find out whether a solution is acidic or basic. When testing the same solution, however, my blue Litmus paper sometimes turns red (thereby indicating an acidic solution) and sometimes stays blue (thereby indicating a base solution). In this case, I wouldn't put much trust in my Litmus paper as [a MFB] about the pH of the solution.

So, if a MFB yields contradictory verdicts, we wouldn't put much trust in it.

The phenomenal conservative might protest that appearances are still *prima facie* (i.e., defeasible) evidence. But the Litmus Test example shows that an untrustworthy MFB is no MFB at all. To say that a belief that p is defeasible is to say that further evidence, which is independent of an appearance that p , can defeat that belief. On the other hand, to say that appealing to appearances is an untrustworthy MFB is to say that the same sort of evidence, i.e., *appearances*, provides at least some degree of justification for both p and not- p . This is not evidence that is independent of an appearance, and thus it is a mistake to characterize it as a defeater.

In other words, if (2) is the case, then S_2 's seeming that not- p doesn't merely defeat but rather *undercuts* S_1 's seeming that p . That is, if it seems to S_2 that not- p , then S_1 's seeming that p provides no degree of justification whatsoever for believing that p , since a piece of evidence from another MFB is a *defeater*, whereas a piece of evidence from the same MFB is an *undercutter*. To see why, suppose I use radiocarbon dating to date a panel painting and get a result of circa 600 BC. Then I use dendrochronology to date the same panel painting and get a result of circa 1700. In this case, the dendrochronology result *defeats* the radiocarbon result (and vice versa), since I have incompatible pieces of evidence from two distinct methods. But now suppose that I use radiocarbon dating to date a panel painting and get a result of circa 600 BC. Then I use radiocarbon dating again but now get a result of circa 1700. In this case, the second result doesn't merely defeat but rather *undercuts* the first result, since now I have incompatible pieces of evidence from the same method. In other words, we wouldn't trust radiocarbon dating as a method of dating the panel painting because incompatible results were obtained using the same method, and thus this method is untrustworthy.

With this support for (2) and (4), the aforementioned *reductio* shows that, from supposing (PC), it follows that appealing to appearances is an untrustworthy MFB.

Note: An anonymous reviewer of *The Reasoner* has offered the following counterexample to (4): “Suppose that you have

an urn and know that it contains a Red, a Blue and a Yellow ball. Alice takes out one ball (you cannot see or otherwise know its colour). When Alice truthfully tells you that the ball is not Yellow, then this information gives you reason to believe that it is Red, and also reason to believe that it is Blue. The beliefs ‘the ball is Red’ and ‘the ball is Blue’ are clearly contradictory. But this does not make the source of justification untrustworthy.” For lack of space, discussion of this putative counterexample will have to wait for another occasion.

MOTI MIZRAHI

Philosophy, St. John’s University

Mill’s Proof of Hedonism

John Stuart Mill opens Chapter IV of his *Utilitarianism* with his (in)famous proof for hedonism. The argument consists of two parts—first, personal hedonism and, second, aggregate hedonism—and it runs thus: ‘The only proof capable of being given that an object is visible, is that people actually see it. The only proof that a sound is audible, is that people hear it: and so of the other sources of our experience. In like manner, I apprehend, the sole evidence it is possible to produce that anything is desirable, is that people actually desire it. If the end which the utilitarian doctrine proposes to itself were not, in theory and in practice, acknowledged to be an end, nothing could ever convince any person that it was so. No reason can be given why the general happiness is desirable, except that each person, so far as he believes it to be attainable, desires his own happiness. This, however, being a fact, we have not only all the proof which the case admits of, but all which it is possible to require, that happiness is a good: that each person’s happiness is a good to that person, and the general happiness, therefore, a good to the aggregate of all persons. Happiness has made out its title as *one* of the ends of conduct, and consequently one of the criteria of morality.’

Focusing only on the first part of his proof, viz., proof of personal hedonism, the argument may be reconstructed thus:

Version 1

(Premise 1) In perception, the only proof/evidence for something being perceptible is that it is actually perceived by people.

(Premise 2) Desire is like perception.

(Interim Conclusion 1) The only proof/evidence for something being desirable is that it is actually desired by people.

(Premise 3) Each person desires happiness.

(Interim Conclusion 2) Happiness is desirable to each person.

(Final Conclusion) Happiness is a good for each person.

However, the argument faces three problems. *First*, in virtue of the second premise that desire is like perception, the argument is obviously constructed along an analogical line of reasoning. But, analogical arguments are generally very unreliable unless there is some fundamental underlying principle for the analogy in question. Yet, Mill does not offer any evidence as to why perception and desire possess relevant similarities and lack relevant differences grounded upon some significant underlying principle. Moreover, Mill appears to be guilty of a

further mistake in his comparison between perception and desire, though this time on semantic rather than logical grounds. In making the analogy, Mill moves from “perceived” to “perceptible” and thereby licenses himself to move from “desired” to “desirable”. However, there is a significant semantic difference between “perceptible” and “desirable” in the sense that the former only connotes a *descriptive* state of affairs whereas the use of “desirable” bears both a *descriptive* and a *prescriptive* connotation. Thus, even if one reluctantly grants the plausibility of the analogy, Mill still seems to be *equivocating* on the use of the concept of desirability; and, this is a problem that shows its unwelcome presence one more time in the penultimate stage of the proof.

The *second* significant setback that the argument faces is how Mill rides roughshod over the problem of scepticism by the first premise. In line with the classical Cartesian sceptical arguments, one can have all the *subjective* experiences or perceptions that one has without there being any *objective* reality to those experiences or perceptions. Or, in a contemporary Stroudian style, the sceptical challenge could be couched in terms of seeking an answer to the question of how the perception of something can *explain* the object’s being perceptible (Barry Stroud, 2000 *Understanding Human Knowledge*, Oxford University Press). But, as Stroud argues, any such explanation can be attained only on pain of either *circularity* or *infinite regress*.

The *third* issue that the argument encounters relates to Mill’s model of action. Given the intimate connection that Mill makes between “desires” and “conduct or actions”, it appears that he subscribes to a ‘desire/belief reasons’ theory of justification of actions whereby desires are taken to play an essential role in the justification of actions. Specifically, in this model, if an agent does not have a desire to do something, then s/he could not possibly have a good reason to do it. However, this somewhat Humean outlook on the origin of actions can paradoxically—at least for Mill—transform itself into an argument for moral scepticism. If, for an agent, to have a reason to do something is to have a desire to do it, then considerations of morality in themselves would not be sufficient to offer the agent in question any reason to act. Thus, the mores of morality can have purchase on moral agents only if they have *first* the desire to follow them, which is tantamount to denying morality the capability of offering any objective reasons to act. Compare, Christine Korsgaard (1986: ‘Skepticism about Practical Reason’, *The Journal of Philosophy*, 83(1), pp. 5–25).

Nonetheless, in defence of Mill’s proof, one may reinterpret the argument by reading the first two problematic premises as no more than heuristic statements and thereby reconstruct the argument in the following manner:

Version 2

(Premise 1) The only proof/evidence for something being desirable is that it is actually desired by people.

(Premise 2) Each person desires happiness.

(Interim Conclusion) Happiness is desirable to each person.

(Final Conclusion) Happiness is a good for each person.

Obviously the argument even in its second reincarnation still faces the radical Cartesian or Stroudian sceptical challenge as well as the moral scepticism engendered by Mill’s theory of

action. Moreover, the argument is evidently an enthymeme as there is a missing premise to link the interim conclusion that happiness is desirable to the final conclusion that happiness is a good. To complete the argument, one may therefore stipulate the suppressed premise that “whatever is desirable to a person is a good”. Now, although the addition of the missing premise formally completes the argument and secures its validity, it ushers in a serious problem for the soundness of Mill’s proof of personal hedonism. Basically the stipulated suppressed premise is in breach of Hume’s Law that an “ought” cannot be derived from an “is” and thereby the premise commits the ‘is/ought’ fallacy. Even if, as Mill himself explicitly states in the quoted passage, the desire for happiness is ‘a fact’, logically it does not follow that it is also a good in itself. To bridge the chasm between facts and norms so swiftly does seem to spoil the potency of his proof.

MAJID AMINI
Philosophy, Virginia State

NEWS

What Can Category Theory Do for Philosophy? 9–11 July

From 9–11 July 2013, a workshop entitled *What Can Category Theory Do For Philosophy?* took place at the University of Kent, Canterbury, organized by local professor David Corfield. The aim of the workshop was to investigate the various forms in which category theory has figured into philosophical reasoning. Since its development category theory has surfaced in philosophy in rather different ways. As diverse as these influences, and the subject itself, was the range of talks given at the workshop.

Some were more technical and concerned methodological considerations about category theory as a mathematical method. Such was the talk by Constanze Roitzheim, who showed how the use of category-theoretic methods influenced the appearance of algebraic topology, most notably via the notion of Quillen model structure as a framework for abstract homotopy theory. Similarly, Andrei Rodin compared different forms of axiomatic thinking and described how category theory inspired a kind of axiomatic method different from Hilbert’s but closer to Euclid.

Besides that were talks about the use of category theory in logic by Brice Halimi, Yoshihiro Maruyama, Kohei Kishida and Hans-Christoph Kotsch. The first gave an overview of how notions from model theory can be captured category-theoretically, starting from a fibrational formulation of Tarskian semantics. The second discussed the many forms of duality that occur in logic and mathematics and which leaves spaces for various analogies of dualities in philosophy. The talks by Kishida and Kotsch concerned categorical semantics for quantified modal logic. The former pointed out the connection between sheaf semantics for first-order modal logic and Lewis’ counterpart theory, while the latter presented an abstract framework for models in arbitrary topoi for a certain version of higher-order modal logic.

Michael Ernst presented a paradox in naive category theory by considering graphs, which has consequences for Feferman’s ideas about foundations for naive category theory. Staffan Angere discussed ways to make sense of a unified notion of structure based on categorical notions. Karin Vereist proposed ideas

of how to capture the metaphysical notion of causation via an adjunction between suitably defined categories.

The last two talks by Ralf Krömer and David Corfield approached the question of the import of category theory in philosophy from a more general perspective. The first addressed the fundamental question what it means to apply categorical methods in philosophy and what possible limitations there are, by means of two examples. David Corfield, finally, mentioned the close relation to type theory. He put the recent development of homotopy type theory and its use for foundations of mathematics into historical perspective and compared it to the advent of logic at the beginning of the 20th century. The main question was whether homotopy type theory has the potential to change the way philosophy is done, and the way philosophical problems are conceived of, to an extent comparable to the influence of logic within analytic philosophy.

Of course, the general question about the import of category theory for philosophy is hard to answer and varies with the topic. However, the unifying idea and motivation for the participants was the conviction that the very rich and successful framework of category theory must have consequences for philosophy. Naturally, these may be more visible in areas that make heavy use of mathematical methods anyway. But at the same time category theory is general enough so as to apply to more genuinely philosophical issues, at least insofar as a formal approach to philosophy seems desirable.

HANS-CHRISTOPH KOTZSCH
MCMP, LMU Munich

Logical Models of Group Decision Making, 12–16 August

Social choice has deep roots in economics, but over the last decades researchers from other academic communities have been taking an ever increasing interest in it.

The *Workshop on Logical Models of Group Decision Making*, organised by Ulle Endriss (Amsterdam) and Eric Pacuit (Maryland, Tilburg) as part of the 25th *European Summer School in Logic, Language and Information* (Düsseldorf), brought together economists, computer scientists, philosophers and, of course, logicians to explore the use of logic in modelling social choice mechanisms.

The workshop was inaugurated by its organisers, who provided an overview of the field.

The second day began with Klaus Nehring (UC Davis), who presented a paper, co-written with Marcus Pivato (Trent), on clarifying the notion of majoritarianism in cases where there is no majority. To resolve disagreements among group members, Nehring proposed the principle of *supermajority efficiency*, which prescribes that smaller supermajorities should yield to larger ones.

Daniel Eckert (Graz) presented a paper, co-authored with Frederik Herzberg (Bielefeld), that draws a close connection between Arrow’s axiomatic foundation of social choice theory and Tarski’s work in model theory. Eckert argued that aggregation theory is reconstructible within a model-theoretic framework, allowing one to view topical results in social choice as model-theoretic preservation results.

On day three, Nikolai Poliakov (Moscow) discussed a refined version of Shelah’s theorem, stating that Arrow’s theorem is extendible to the case where individual choices are not

rational. Poliakov argued that Shelah's clonal approach may be employed for obtaining 'possibility theorems' in some specific cases.

Building on previous work by Negri and von Plato, Paolo Maffezoli (Groningen) delineated a Gentzen-style framework for reasoning about social-choice-theoretical notions. He presented a proof-theoretic account of the connection between individual preference relations and choice functions, and illustrated how to extend such frameworks to collective preference.

Lastly, Dominik Klein (Tilburg) introduced a new semantics for approval voting which takes into account both instrumental and expressive reasoning about voting behaviour. He also presented an extension of this paradigm to range voting.

The fourth day started with Valentin Goranko (DTU), who presented a paper, co-authored with Nils Bulling (TU Clausthal), on combining quantitative and qualitative approaches to the study of agents' strategic abilities in multi-player games. In particular, he illustrated a quantitative extension of the logic ATL*.

Dorothea Baumeister (Düsseldorf) presented her joint work with Gábor Erdélyi (Siegen), Olivia Erdélyi (Düsseldorf) and Jörg Rothe (Düsseldorf) on the computational aspects of several forms of manipulation and control in judgment aggregation. The notion of control by bundling judges was introduced, and the premise-based procedure was shown to be resistant to it in terms of NP-hardness.

On the last day, Roland Mühlenbernd (Tübingen) discussed a variant of Conway's Game of Life, devised with Simon Schulz (Tübingen), in which living cells play pre-games before the rules of nature are applied. In this setting, the living cells' spatial occupation share of the environment is considerably greater than in the basic version of the game.

Wojtek Jamroga (Luxembourg) presented his joint work with Marija Slavkovic (Bergen) on the computational complexity of arbitrary distance-based judgment aggregation rules. The focus was on the decision problem of verifying whether a given judgment set is representative of the collective opinion; it was shown that, generally, the feasibility of distance-based aggregation cannot be guaranteed.

The workshop was concluded by Rasmus Rendsvig (Copenhagen), who proposed a model of learning via social proof based on dynamic epistemic logic augmented with decision and interpretation rules. This model allows local, rule-based choices, and can also be employed to reason about them.

All papers and slides are available on the [website](#) of the workshop.

FRANCESCA ZAFFORA BLANDO
ILLC, University of Amsterdam

Calls for Papers

INFINITE REGRESS: special issue of *Synthese*, deadline 1 October.

BELIEF CHANGE AND ARGUMENTATION THEORY: special issue of *Annals of Mathematics and Artificial Intelligence*, deadline 15 November.

Logic and Rational Interaction

What should we believe? And how should we change our beliefs in light of new information? These are two of the most central questions in philosophy. Modern epistemology has two main tools for handling these questions, logic and probability. On one view belief is a set of propositions designated to give a faithful representation of chunks of the world. Therefore, it is argued that the belief set should have a certain level of coherence. In particular the set of believed propositions should be closed under logical consequence.

On the other hand the set of beliefs is not a uniform mass. Some beliefs are firm and tight, whereas others are of a more speculative character. In general, beliefs come in various degrees. In probabilistic models, these degrees of belief are given by the probability its holder ascribes to it being true. For instance believing something to a degree of 0.5 means to hold it as likely as not. On the other extreme, to believe something with probability 1 is to hold it absolutely and infallibly certain. The holder of such a belief should be willing to accept *any* bet on it being true.

The classical way for relating these two models is Locke's thesis, stating that a proposition should be in the belief set if and only if its probability exceeds a certain threshold. However things are not as simple as that.

In 1961 Kyburg presented his famous lottery paradox showing that in general probabilistic belief, a belief set that is closed under logical consequence, and Locke's thesis are jointly inconsistent. The paradox starts with a very large fair lottery that has a single winning ticket. For any given ticket the chance that it is losing is so high that Locke's thesis justifies the belief that it is indeed losing.

Now if the belief set is closed under logical consequence, we can combine these beliefs about every single ticket to a single belief about the set of all tickets, saying that the entire set consists of losing tickets only. But this is paradoxical, since we believe the lottery to have a winning ticket.

The last months have seen several thrilling attempts to reconcile logic and probabilistic reasoning about beliefs without falling prey to the lottery paradox put forward by Hannes Leitgeb (Munich) and Hanti Lin and Kevin Kelly (CMU).

In his paper 'The stability theory of belief', Hannes Leitgeb aims at reconciling probabilistic reasoning, logical closure and what he calls the 'Humean thesis', according to which believed propositions' probabilities are not just high, but stably so. He claims that the criterion of stability we use for identifying beliefs is not the same in every situation. Rather, it varies with the exact presentation of the situation, with the stakes of actions we might base on those beliefs and various other factors. Leitgeb argues that many realistic situations have probability assignments that are consistent with Hume's thesis and logical closure of the belief set. He gives a precise analysis of when these three desiderata are jointly fulfillable and when not.



Beliefs are not static, but they often need to be revised in light of incoming information. Both probabilistic and logical models have devised various tools for incorporating new information. Connecting probabilistic and logical modelling via Hume's thesis then raises the question of how these tools fit together. Ideally a probabilistic and a logic tool should be compatible such that a probabilistic update has the same effect on the belief set as the logical update does. Hanti Lin and Kevin Kelly show in [Propositional Reasoning that Tracks Probabilistic Reasoning](#) that the classic tools of Bayesian conditioning and AGM belief revision are incompatible in this sense. Furthermore, they show that bayesian updating is compatible with Shoham revision, a weaker version of logical updating. Their paper gives a precise analysis of the interplay between these two concepts.

In a second paper titled [A geo-logical solution to the lottery paradox](#), Lin and Kelly present a different approach to acceptance thresholds. Their idea is that the decision to believe in the falsity of some hypothesis E can depend upon the alternatives to E . If there is a prominent alternative at hand a comparably low threshold might be enough to accept $\neg E$, whereas if there is no such competitor on the horizon one might be reluctant to believe in $\neg E$ even if the evidence is fairly strong. Based on this idea Lin and Kelly devise a class of shutter rules for belief acceptance which they show to avoid the lottery paradox. Furthermore they apply the tool of geo-logic to reasoning about belief sets and show that shutter rules naturally correspond to geo-logical reasoning.

Leitgeb, Kelly and Lin discussed their work on belief at recorded round-table events entitled "Truth Table" and "Table of Contents", which can be viewed [here \(videos 147 and 148\)](#).

LORIweb is always happy to publish information on topics relevant to the area of Logic and Rational Interaction—including announcements about new publications and recent or upcoming events. Please submit such news items to [Rasmus Rendsvig](#), our web manager or to the [loriweb address](#).

DOMINIK KLEIN
TiLPS, Tilburg University

Uncertain Reasoning

Subjective Bayesianism reluctantly makes room for modelling higher-order uncertainty. In Bruno de Finetti's rendering, probability is the sum total of an agent's state of uncertainty with respect to some well-posed question, i.e., an event. And the logic of (unconditional) events is classically bivalent. This means that we shouldn't contemplate the idea that a normatively rational agent may be uncertain about their own uncertainty, in any meaningful way. In de Finetti (1974: *Theory of Probability*, John Wiley and Sons, p. 82) the point is illustrated as follows:

Among the answers that do not make sense, and cannot be admitted are the following: 'I do not know', 'I am ignorant of what the probability is', 'in my opinion the probability does not exist'. Probability (or prevision) is not something which in itself can be known or not known: it exists in that it serves to express, in a precise fashion, for each individual, his choice in his given state of ignorance. To imagine a greater degree of ignorance which would justify the refusal to answer would be rather like thinking that in

a statistical survey it makes sense to indicate, in addition to those whose sex is unknown, those for whom one does not even know 'whether the sex is unknown or not'.

Many Bayesians, including I.J. Good and L.J. Savage spoke in favour of the advantages of modelling various kinds of higher-order uncertainty, but they all failed to persuade the Italian statistician of the worthiness of the enterprise (see, de Finetti 1974: §19.3). Indeed his example seems to be watertight, for you either know the sex of a person, or you don't. And if you don't, you negatively introspect, and then you know that you don't know. You can't be uncertain about that.



Or can you? In a recent interview to the newspaper *La Repubblica*, Italy's leading oncologist and successful science populariser Umberto Veronesi, casts serious doubts on the absolute knowability of sex. Specifically, Veronesi comments on the significant rise in the number of required treatments for newborns of 'uncertain sex' at the San Camillo hospital in Rome. Here is an excerpt of Veronesi's reply.

'Uncertain sex' is, from the biological point of view, a pathological accentuation of bisexuality. We are all potentially bisexual. For males have breasts and their prostate is a sort of uterus, whilst the female clitoris is a sort of penis. Intersexual individuals—those whose sex is uncertain—exhibit a discrepancy between the information carried by chromosomes XX and XY for females and males respectively, and their genitals. In about 50% of cases, this dual sexual identity at birth is caused by a genetic failure of an enzyme which results in excessive levels of testosterone in the foetus. If the baby to be born is female, then a masculinisation of the genitals occurs: the clitoris has a size comparable to a penis and the vagina is practically nonexistent.

What I take to be an intriguing consequence of intersexuality is that it seems to make an interesting and practically relevant case for relaxing the binariness (or crispiness) of events, which is central to de Finetti's construction of the subjective theory of probability and indeed justifies his example above. Put otherwise, it seems to make sense to be uncertain about the sex of an individual who, as far as their genitals are concerned, may not be either male or female. And make decisions based on that.

HYKEL HOSNI

Marie Curie Fellow, CPNSS, London School of Economics

EVENTS

OCTOBER

APMP: 2nd International Meeting of the Association for the Philosophy of Mathematical Practice, University of Illinois at Urbana-Champaign, USA, 3–4 October.

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- CMMGRC:** 7th Centre for Metaphysics and Mind Graduate Conference, University of Leeds, 5 October.
- S & R:** Science and Reality, Rotman Conference, London, Ontario, Canada, 5–6 October.
- BAYES:** Workshop on Bayesian Spatio-Temporal Modelling, Edinburgh, 8–9 October.
- LORI:** 4th International Workshop on Logic, Rationality and Interaction, Zhejiang University, Hangzhou, China, 9–12 October.
- INVESTIGATING SEMANTICS:** Ruhr-University-Bochum, 10–12 October.
- EXPERIMENTAL PHILOSOPHY:** State University of New York, Buffalo, 11–12 October.
- PROBABILISTIC MODELING:** in Science and Philosophy, Bern, Switzerland, 11–12 October.

INDUCTIVE LOGIC AND CONFIRMATION IN SCIENCE

University of Kent, Paris Campus, 17–18 October

IDA: 12th International Symposium on Intelligent Data Analy-

sis, London, UK, 17–19 October.

FPMW: French PhilMath Workshop, Paris, France, 17–19 October.

CHESS: Inaugural conference of the Centre for Humanities Engaging Science and Society, Durham University, 18 October.

ICPI: International Conference on Philosophy of Information, Xian, China, 18–21 October.

LENLS: Logic and Engineering of Natural Language Semantics, Kanagawa, Japan, 27–28 October.

AAA: 1st International Workshop on Argument for Agreement and Assurance, Kanagawa, Japan., 27–28 October.

HAPoC: 2nd International Conference on the History and Philosophy of Computing, Paris, France, 28–31 October.

NOVEMBER

CHPS: 29th Boulder Conference on the History and Philosophy of Science, University of Colorado at Boulder, 1–3 November.

ARCHÉ / CSMN: 7th Arché / CSMN Graduate Conference, University of St Andrews, 2–3 November.

NKMTD: Naples-Konstanz Model Theory Days, Napoli, Italy, 6–8 November.

MADRID IV: Inferentialism in Epistemology and Philosophy of Science, Madrid, 11–13 November.

RISK DAY: Risk and Reliability Modelling for Energy Systems, Durham University, 13 November.

ACML: 5th Asian Conference on Machine Learning, Canberra, Australia, 13–15 November.

REDUCTION AND EMERGENCE: Reduction and Emergence in the Sciences, LMU Munich, 14–16 November.

PHILOSOPHY OF MEDICINE ROUNDTABLE: Columbia University, New York, 20–21 November.

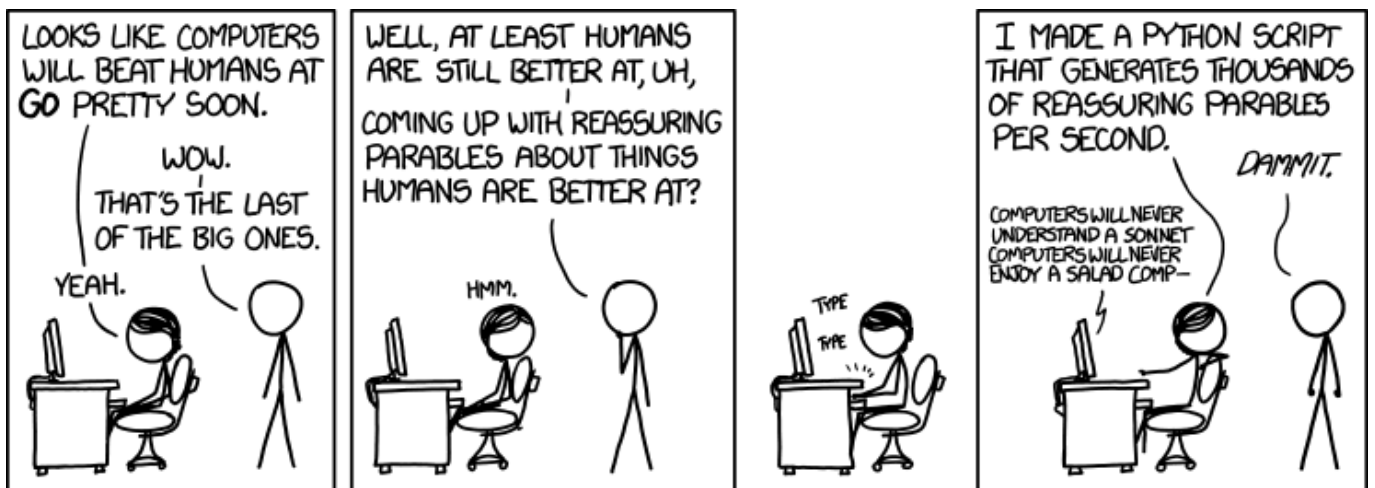
SCAI: 12th Scandinavian Conference on Artificial Intelligence, Aalborg, Denmark, 20–22 November.

AICS: International Conference on Artificial Intelligence and Computer Science, Bayview Hotel, Langkawi, Malaysia, 25–26 November.

ICOG: Inaugural Conference of Network for Postgraduate and Early-career Researchers in Cognitive Science, University of Sheffield, 29 November–1 December.

DECEMBER

PRIMA: 16th International Conference on Principles and Practice of Multi-Agent Systems, Dunedin, New Zealand, 1–6 De-



xkcd.com

ember.

AIC: International Workshop on Artificial Intelligence and Cognition, Turin, Italy, 3 December.

TPNC: 2nd International Conference on the Theory and Practice of Natural Computing, Cáceres, Spain, 3–5 December.

AJCAI: 26th Australasian Joint Conference on Artificial Intelligence, Dunedin, New Zealand, 3–6 December.

PHILOSci21: Challenges and Tasks, Lisbon, Portugal, 4–6 December.

ICDM: International Conference on Data Mining, Dallas, Texas, 8–11 December.

LPAR: Logic for Programming, Artificial Intelligence and Reasoning, Stellenbosch, South Africa, 14–19 December.

OBAYES: International Workshop on Objective Bayes Methodology, Duke University, Durham, NC USA, 15–19 December.

VAGUENESS: University of Navarra, Pamplona, Spain, 16–17 December.

DIALDAM: 17th Workshop on the Semantics and Pragmatics of Dialogue, ILLC, University of Amsterdam, 16–18 December.

IICAI: 6th Indian International Conference on Artificial Intelligence, Tumkur, India, 18–20 December.

ISHPS: 14th Annual Conference of the Israeli Society for History & Philosophy of Science, Bloomfield Science Museum, Jerusalem, 22 December.

JANUARY

ISAIM: International Symposium on Artificial Intelligence and Mathematics, Fort Lauderdale, Florida, 6–8 January.

CONDITIONAL THINKING: Leeds, 14–15 January.

COURSES AND PROGRAMMES

Courses

MODES OF TECHNOLOGICAL KNOWLEDGE: Chalet Giersch, Manigod, France, 19–25 January.

GRONINGEN WINTER SCHOOL: Faculty of Philosophy, University of Groningen, 27–28 January.

EPISTEMIC GAME THEORY: EPICENTER, Maastricht University, 12–23 May.

Programmes

APHIL: MA/PhD in Analytic Philosophy, 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.

MASTER PROGRAMME: Philosophy of Science, Technology and Society, Enschede, the Netherlands.

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 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 COGNITIVE SCIENCE AND HUMANITIES: LANGUAGE, COMMUNICATION AND ORGANIZATION: Institute for Logic, Cognition, Language, and Information, University of the Basque Country, Donostia, San Sebastian.

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.

MA IN REASONING

A programme at the University of Kent, Canterbury, UK. Gain the philosophical background required for a PhD in this area.

Optional modules available from Psychology, Computing, Statistics, Social Policy, Law, Biosciences and History.

MSc IN COGNITIVE & DECISION SCIENCES: Psychology, University College London.

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 MATHEMATICAL LOGIC AND THE THEORY OF COMPUTATION: Mathematics, University of Manchester.

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 Sebastian).

OPEN MIND: International School of Advanced Studies in Cognitive Sciences, University of Bucharest.

PHD SCHOOL: in Statistics, Padua University.

JOBS AND STUDENTSHIPS

Jobs

ASSOCIATE PROFESSOR: In Philosophy of Science, University of Geneva, until filled.

POST-DOC POSITION: in Set Theory, Torino University, until filled.

POST-DOC POSITION: on the project “Rational reasoning with conditionals and probabilities”, MCMP, LMU Munich, until filled.

PROFESSOR: in Philosophy of Science, New York University Shanghai, deadline 1 October.

POST-DOC POSITION: on the topic “The Ties Between Logic and Argumentation”, University of Coimbra, Portugal, deadline 11 October.

POST-DOC POSITION: within the project “Grounding—Metaphysics, Science, and Logic”, Institute of Philosophy, University of Neuchâtel, deadline 15 October.

ASSISTANT PROFESSOR: in Philosophical Logic, ILLC, University of Amsterdam, deadline 15 October.

JUNIOR FACULTY POSITION: in Analytic Philosophy, Stanford University, deadline 1 November.

SENIOR FACULTY POSITION: in Analytic Philosophy, Stanford University, deadline 1 November.

ASSISTANT PROFESSOR: in Philosophy of Social Science, MCMP, LMU Munich, deadline 20 November.

Studentships

STUDENT ASSISTANT: on the project “Rational reasoning with conditionals and probabilities”, MCMP, LMU Munich, until filled.

PHD POSITION: in Theoretical Philosophy within the project “Knowledge in a Digital World: Trust, Credibility and Relevance on the Web”, Lund University, deadline 14 October.

PHD POSITION: in Causal Discovery with Applications in Biology, Faculty of Science, University of Amsterdam, deadline 16 October.