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Editorial

It is a great pleasure and honour for me to be a guest editor of The Reasoner. In this issue I will interview Berit (Brit) Brogaard, Professor of Philosophy at the University of Miami and Dr Richard Dawid, Research Fellow at the Munich Center for Mathematical Philosophy.

Brit has only recently moved to Miami after working at the University of Missouri, St. Louis, for some ten years. She is one of the most versatile intellectuals I have been acquainted with in my academic career. As I hope my interview will reveal, Brit's research has become more and more interdisciplinary in time. It provides a paradigmatic example of how philosophy can fruitfully collaborate with and draw from natural sciences. I first came across Brit's work on philosophical and epistemic logic while I was doing my postdoctoral research at the Centre for Time of Sydney University in 2005-2006. When I found out that Brit had a temporary position in ANU, I invited her to give a talk at a workshop on ontological commitment that I organized at the Centre for Time. More recently, I had the chance to use for my own research other stimulating papers by Brit, this time on philosophy of mind and epistemology of perception. Because of her findings in these areas, I invited Brit, again, to contribute to a workshop on external world scepticism that I organized at the Munich Center for Mathematical Philosophy in 2013. Only very recently I discovered that Brit is also a brilliant cognitive scientist.

I first met Richard in 2011, when we were both visiting fellows at the Tilburg Center for Logic and Philosophy of Science. Since then we have been involved in a stimulating exchange of ideas about various issues of philosophy of science and epistemology. Richard's book, *String Theory and the Scientific Method* (CUP 2014) and the article 'The No Alternatives Argument', co-authored by Stephan Hartmann and Jan Sprenger and forthcoming in British Journal for Philosophy of Science, have recently captured the attention of the media. The truth is that Richard's work comes as a breath of fresh air in the current philosophical conversation on scientific methodology.

Richard started his academic career as a high-energy physicist but he converted to philosophy of science after doing his postdoctoral work in physics at the University of Berkeley. Now his research focuses on issues of scientific rationality surrounding String Theory (the controversial theory of all physical interactions). In a nutshell, Richard's goal is to provide an explanation of why string theorists have such a strong belief in their theory despite the lack of empirical confirmation. He offers an interesting answer that—as one might expect—heavily depends on non-empirical criteria of theory assessment. This answer comes together with an innovative picture of scientific methodology as a whole. According to Richard, the critics of String Theory typically rely on an outdated conception of scientific rationality and thus fail to recognize that a new framework in scientific methodology is required.

Luca Moretti
Philosophy, Aberdeen

Interview with Berit Brogaard

Luca Moretti: You have been giving significant inputs to various areas of analytic philosophy, including—I would say—philosophy of language, metaphysics, philosophy of mind, and epistemology. Furthermore, you are an active researcher in the area of cognitive neurosciences—you have done empirical research, for example, on synaesthesia and autism. You are the president of the Southern Society for Philosophy and Psychology and of the Central States Philosophical Association, the American Editor of Erkenntnis, the philosophy of language editor for PhilPapers, you serve on the editorial boards of various academic journals. You write for trade and popular magazines and are also a very active blogger. Last but not least, you are a poet in Danish language. All this is quite amazing. Could you tell us something about your neuroscientific findings?

Berit Brogaard: It's relatively recent that scientists have discovered a connection between autism and synaesthesia. Recently a family link study showed that one of the genes involved in autism is also implicated in synaesthesia, at least in that particular family. A population study furthermore showed that there is a greater number of synaesthetes in the autistic population compared to the general population.

LM: I think the meaning of autism' is sufficiently familiar. But what do you mean by synaesthetes'?

BB: Synaesthesia is an extraordinary way of perceiving the world, involving experiences of connections between seemingly unrelated sensations. For example, the number 3 may lead to a perception of copper green, the word kiss' may flood the mouth with the flavour of bread soaked in tomato soup and the key of $C^{\#}$ minor may elicit a bright purple spiral radiating from the centre of the visual field. By synaesthetes' I simply mean the subjects who have this condition, synaesthesia.

LM: It's clear now. So what has your lab proposed to explain the observed connection between autism and synaesthesia?

BB: My lab has proposed that abnormal serotonin levels may be involved in autism and synaesthesia. We know that serotonin levels tend to be abnormally high in children with autism and then they typically drop later in life. We also know that serotonin agonists—i.e., compounds that activate serotonin receptors in a manner similar to serotonin—such as LSD, psilocybin and mescaline, can trigger synaesthesia. On the model we proposed for psilocybin-induced synaesthesia, psilocybin (or, in fact, the chemical it turns into) binds to layer-V pyramidal cells in the visual cortex. This leads to hyperexcitability of the visual cortex but it also leads to an inhibition of the thalamus. The thalamus is implicated in restricting the information that enters the visual cortex. When it is inhibited, an overload of random information enters the visual cortex, yielding hallucinatory experience. This random information also gets bound together with auditory or other visual information, which triggers, for example, sound-colour synaesthesia. We suspect that the sensory hyperexcitability demonstrated in the case of

drug-induced synaesthesia is also triggered in children with autism. Over time hyper-excitability can lead to local hyper-connectivity and abnormal binding of features. This would explain why there are more synaesthetes in the autistic population compared to the general population.

LM: So this ground-breaking investigation is still going on. Has the fact that you have recently accepted the position at the University of Miami yielded any practical difficulties? For example, have you taken your research group with you to Miami?

BB: My lab is in the process of moving to University of Miami. The research facilities and support there will likely mean that my lab will expand but the core research done will be the same.

LM: In Miami you have a joint appointment at the Department of Psychology and the Department of Philosophy. So you are still a philosopher. Let's switch to philosophy. As you said, you started as a logician. But I recall that I invited you to present a paper on ontological commitment some years ago and, more recently, one on phenomenal conservatism and scepticism. Could you say a bit about your work in philosophy?

BB: After I got my degree in philosophy I was mostly doing logic-based stuff. I did quite a bit of work on the knowability paradox. In fact, the majority of my first publications were in this area. So, I continued doing mostly logic-based philosophy for a while. My work on ontological commitment was also heavily grounded in logic. But I eventually returned to the areas I had worked on previously, namely language and philosophy of mind. The majority of my current papers and books are now in those areas.

LM: When you say 'knowability paradox' do you refer to what others call 'Fitch's paradox'?

BB: Yes, the knowability paradox is also known as 'Fitch's paradox'.

LM: Did you arrive at interesting results?

BB: I have provided a number of solutions to Fitch's paradox that rely on modal logic. I have also demonstrated some limitations of the so-called restriction strategy to the paradox.

LM: Interesting. Actually, I recall that I used the Stanford Encyclopedia entry on Fitch's paradox that you have co-authored with Joe Salerno for my teaching. Actually, the first paper by you (and Joe) I read was just a logic-based paper. It was not about Fitch's paradox, but alethic antirealism and the conditional fallacy objection. You claimed you had a working formal proof that a popular antirealist definition of truth entails an absurd consequence. That paper intrigued me so much that I decided to write a reply to defend antirealism. And not just one reply: I recently co-authored another paper with Patrick Girard that could be seen as a continuation of my response. But let's go back to your story. You said that after working on logic for a while, you went back to philosophy of language and mind. I guess your first book, *Transient Truths: An Essay in the Metaphysics of Propositions* (OUP 2012), is one of your major publications in these areas. What is the central thesis of the book?

BB: Transient Truths is an extended defense of temporalism, the view that propositions can have different truth-values at different times. For some reason this thesis has been rather unpopular in the last few decades. The turning point was Mark Richard's paper—Richard (1981:Temporalism and Eternalism, *Philosophical Studies*

39(1):1–13)—arguing that temporalism has absurd consequences. I think he is wrong about that, of course.

LM: Some of the titles of your forthcoming papers in philosophy of mind have also drawn my attention. For instance: 'Color Synesthesia and its Philosophical Implications' or 'Synesthesia as a Challenge for Representationalism'. It would seem to me that you are trying to cast a bridge from your scientific investigation to your activity as a philosopher. The second paper intrigues me particularly: how can synesthesia challenge representationalism?

BB: The paper argues on the basis of research in my lab that some forms of projector synesthesia do not represent the colors projected out into the world as instantiated by mind-independent, physical objects.

LM: What do you mean by 'projector synesthesia'?

BB: Projector synesthesia is a form of synesthesia in which the synesthetic experiences are projected out into the world. It contrasts with associator synesthesia in which the synesthetic experiences are presented internally, like visual imagery, for example. In the paper I argue that since some forms of projector synesthesia do not represent the colors projected out into the world as instantiated by mind-independent, physical objects, this shows that the phenomenology of visual experience does not flow from the representational content. In other words, there are qualia, or phenomenal properties, that are not determined by a representational content of experience. So, representationalism is false. My own view is actually a more extreme version of the qualia view. I hold that experience does not have content in any meaningful sense but that it is nonetheless representational. I also point out that the qualia view doesn't entail a rejection of physicalism. That is apparently not obvious to everyone.

LM: Another interesting title of a new paper of yours is 'What Can Neuroscience Tell Us About Reference?' So what can it tell us?

BB: Well, it cannot yet tell us that much about external-world reference, but it can shed light on anaphoric reference and on how we update and revise internal semantic representation structures more generally. Electroencephalogram-based studies also seem to confirm many of basic tenets of discourse representation theory and other dynamic semantic frameworks.

LM: I would like to ask much more about this, but I have to wrap up. This is my last question. I noticed that you have a forthcoming book that is quite surprisingly titled: *On Romantic Love: Simple Truths about a Complex Emotion*. What sort of work is this? Is it cognitive neuroscience? Philosophy?

BB: This is an OUP trade book. It defends the view that love is a complex emotion that admits of degrees, can be unconscious, can be rationally justified and can be rationally controlled. It will be out in January 2015. Most of the evidence I provide is from neuroscience, psychology and philosophy.

LM: I would like to thank you, Berit, for this stimulating interview.

BB: Thanks so much, Luca!

Interview with Richard Dawid

Luca Moretti: Dear Richard, I'm delighted that you accepted to be interviewed by *The Reasoner*. Of course, I will ask you questions about your recent book, *String Theory and the Scientific Method* (CUP 2014). Before that, I would like to know about your intellectual story. I recall that you started your career as a physicist but converted to philosophy after a while. Could you tell us something about these events?

Richard Dawid: My pleasure, I'm looking forward to the interview. After my PhD, I spent two years as a high-energy physicist at Berkeley. That period, the late 1990s, was a particularly fascinating time for String Theory (ST). Some new conceptual ideas developed in those years substantially changed the understanding of ST and paved the way for its further development until today. Watching those developments, I felt that they raised novel and interesting philosophical questions at various levels. Thinking about them eventually made me switch from physics to philosophy.

LM: Did you have any background in philosophy?

RD: I was always interested in philosophy. I had read some philosophy, had joined a philosophy discussion group during my PhD in Vienna and had attended a few philosophical university seminars. But at the time I decided to enter philosophy, my knowledge was quite haphazard. Thinking back today, I'm a little stunned on what meagre basis I made that decision.

LM: I remember you told me that you emailed eminent philosophers for advice. What did you ask them? Did you manage to meet any of them?

RD: Right, once I had developed some first philosophical ideas about what I intended to do, I wanted to clarify two things before seriously moving into philosophy. First, I wanted to know whether my ideas made a little sense to genuine philosophers. Second, I wanted to know whether it was fun discussing with genuine philosophers. Since the only philosophers I knew at the time were really famous ones, whose books I had read, I just emailed three of them: Hilary Putnam, Bas van Fraassen and Hartry Field. I asked them whether they were willing to talk to me about my ideas. All three were extraordinarily kind and agreed to meet. Unfortunately, Putnam had to cancel the day before we met for urgent personal reasons, but I met van Fraassen and Field and presented a sketch of my ideas to them. Van Fraassen was very supportive and gracious and seemed genuinely interested, a real pleasure to talk to. Field told me right from the start that he wasn't interested in the subject but was ready to comment on the general soundness of my reasoning, which he did with impressive acuteness. Both meetings substantially strengthened my conviction that it made sense for me to turn towards philosophy.

LM: Were your first philosophical ideas already about ST and the no-alternatives argument?

RD: I was mainly interested in two issues that were both related to ST.

LM: Perhaps, before continuing, it would be helpful if you could shortly explain what ST is.

RD: ST aims at providing a unified theory of all physical interactions. The nuclear interactions, which are crucial for understanding microphysics, are today described by gauge field theory, which is based on the principles of quantum mechanics. Gravitation is described by general relativity. A coherent overall theory that covers both regimes faces deep conceptual problems. There are reasons to believe that ST can solve those problems. ST starts from the basic idea that elementary objects are not point-like objects, as gauge field theory assumes, but one-dimensional strings. Those strings are taken to be so small that their extendedness cannot be measured by present day experiments. But if ST is right, the movements and topological characteristics of strings can explain all observable properties of elementary particles.

LM: Thanks. Please now let's go back to my original question.

RD: Yes. First, I was interested in the phenomenon of string *dualities*. In ST, one encounters the phenomenon that seemingly very different realizations of the theory after close inspection turn out to be dual to each other. If two theories or models are dual to each other, they are related in a specific way that implies that they are empirically equivalent. Dual theories or models can be different in all respects normally taken to specify the ontology of a physical theory. They can imply different symmetry structures, different spacetime structure, different dimensionality of elementary objects, different kinds of interaction and so on. Duality relations even reach out beyond the limits of ST proper: it turns out that in specific contexts a string theoretical description is dual to a purely field theoretical one that doesn't contain any strings. Duality relations are abundant in string physics and constitute one of its core characteristics. At a philosophical level, dualities are fascinating for example because they seem to offer a straightforward argument *against* scientific realism: if I can move from a description that posits a certain set of fundamental objects to another one empirically equivalent that posits an entirely different set of fundamental objects, and if my theory suggests that such correlations are one of its core characteristics, a realist interpretation of any set of fundamental objects seems at variance with spirit and content of the theory.

LM: I can see it. I wonder why antirealists have never mentioned this intriguing argument.

RD: That's a good question. I first made this argument in a paper in 2003. The same point was emphasized later by Dean Rickles and Keizo Matsubara in their work on the philosophy of ST. But it was never picked up in the general realism debate. I think one reason is that philosophers of science mostly take ST as an unconfirmed speculation that, as such, can have no serious implication in philosophy of science. Which brings me directly to the second important philosophical issue related to ST I was and am still interested in. Despite the fact that ST hasn't found empirical confirmation, string theorists have a conspicuously high degree of trust in it. Clearly they *don't* understand ST as a mere speculation. Thus with ST, an empirically unconfirmed theory has acquired the position of a conceptually dominating force in fundamental physics. I think that this requires a substantially altered philosophical concept of scientific theory assessment and confirmation to account for this novel situation.

LM: So we have arrived at the topic of your recent book. I remember you told me that the original title was 'Delimiting the Unconceived'. Why did you choose just this title?

RD: Yes, my original title idea was *Delimiting the Unconceived*. Nick Gibbons, the CUP editor, thought that for those who haven't already read the entire book that title was overly enigmatic. Today I think he was absolutely right. Still, the phrase 'delimiting the unconceived' catches quite well the basic idea of the book. All of us, scientists as well as everyone else, deal with the world based on theories we have developed about it. We know, however, that there are many other possible and potentially important theories we haven't thought of yet.

LM: This has been forcefully argued for by Kyle Stanford (2006). His point is that we can inductively infer from examining past science that our new theories are probably underdetermined by empirical data even if we are actually unable to think of the alternative theories that engender the underdetermination.

RD: Yes. The 'canonical' understanding would be that we know nothing about this realm of 'the unconceived'. My book argues that this is *not* true. We do know something about the unconceived. We don't know what it contains, obviously, but we can understand something about its limits. From our observations about the world we can learn something about the size of the spectrum of possible scientific theories that we have not yet developed. At its core, the book is an investigation into how this can work.

LM: So your book aims to answer Stanford's new underdetermination argument from unconceived alternatives. Your point is—it seems to me—that at least in the case of ST there is probably no alternative—not even an *unconceived* alternative.

RD: The book argues that there can be a scientifically viable line of reasoning that leads to that conclusion even in the absence of empirical confirmation. To evaluate the strength of such reasoning in a specific case is up to the involved scientists. Eventually, that is the conclusion, yes. But in order to develop the philosophical point, get there, one has to take a number of intermediate steps with respect to understanding various facets of underdetermination.

LM: I guess many of the difficulties to get to that conclusion hinge on the notion of an alternative theory. Cannot one argue that there are always *sceptical* alternatives—for instance the brain-in-a-vat hypothesis—or that we can produce alternatives by *conventionalist* manoeuvres, say, by changing the value of physical constants?

RD: You're right, before assessing the number of alternative theories, it is necessary to specify what counts as an alternative. That specification crucially relies on what we want achieve by counting alternatives. Let us go back to the initial observation that string theorists trust their theory in the absence of empirical confirmation. Why do they do that? The answer in my recent book is that they do so based on their assessment of underdetermination: they believe that the chances for a viable alternative to ST are small, from which they conclude that, assuming there is a viable scientific theory of all interactions at all, ST (or whatever ST ends up being when fully developed) is likely to be that theory. Note that physicists are not interested in the realism question here. They are interested in the more modest question whether ST is consistent with the empirical data at the theory's characteristic scale. This means that, when counting possible alternatives, we should only count alternatives that can be empirically distinguished from ST at its characteristic scale. So the theories that are empirically equivalent to each other should be counted just as the same theory. Furthermore, we should only be interested in theories that pass for *scientific* in the eyes of physicists. If we have reasons to ex-

pect the number of alternatives of that kind to be very small, we have reasons to have trust in ST even in the absence of empirical confirmation. Based on similar reasoning, it doesn't make any sense to count theories with different parameter values as different theories. When a physicist assesses the viability of a theory with a free parameter whose value has not yet been fixed by empirical data, she does not insist on a specific parameter value. Therefore, her assessment of underdetermination will be based on a theory individuation that subsumes all parameter values under the same theory.

LM: I see where you're going. However, one might still doubt that if we have reasons to expect that the number of proper alternatives to ST is very small, we have evidence for ST in the absence of empirical confirmation.

RD: Well, a precondition for making this epistemic connection is to have trust in the success of the scientific method in the given context. Based on our observation that physicists have so often found viable scientific theories within the scientific contexts they were investigating, we can assume that there is some viable scientific theory for the contexts proper to ST as well. On that basis, we can say: if there are no scientific alternatives to ST, this theory must be viable. If there are very few alternatives, there should be a decent chance that, when developing ST, physicists have picked the viable theory. If there were a wide range of alternatives, however, knowing this fact wouldn't instil significant trust in ST.

LM: This is my last question. You have a paper forthcoming in the BJPS coauthored by Stephan Hartmann and Jan Sprenger titled 'The No Alternatives Argument'. Could you tell us what it is about?

RD: The topic of the paper emerges from the context we were discussing. If scientists assess the number of possible alternatives to their theory, how do they do it? In my book I identify three main argumentative strategies to that end. The most direct strategy is based on an inference from the observation that scientists haven't found any viable alternatives to the theory in question to the statement that there probably are no or few alternatives. I call this inference the 'no alternatives argument'. A second argument is based on the observation that the theory under scrutiny provides explanations of phenomena or conceptual characteristics of predecessor theories it was not developed to explain. And a third argument is based on the observation of a tendency of predictive success in the research field. Now an interesting question arises: what status can we attribute to such reasoning? Can we understand it as a form of theory confirmation? In the paper with Stephan and Jan we analyze this question for the case of the no alternatives argument in a Bayesian framework. We find that under very mild and plausible assumptions the no alternatives argument does amount to theory confirmation. This is interesting because Bayesian confirmation is normally taken to rely on empirical data predicted by the confirmed theory. But the observation that scientists haven't found alternatives to, say, ST cannot be predicted by ST itself. Still, it turns out that it confirms the theory in question. The paper also shows that the no alternatives argument on its own, though formally leading to confirmation, is ineffective because it does not allow assessing the significance of that confirmation. Thus, in order to have relevant and substantial confirmation, at least one of the other two argumentative strategies must be deployed in conjunction with the no alternatives argument.

LM: I would like to thank you, Richard, for this interesting chat.

RD: Thanks, Luca, it was a lot of fun.

Anaphoric Reference Rules!

How do we 'get in touch with' abstract objects? The answer is: by using anaphoric reference.

Readers of Prior's later work (1971: *Objects of Thought*, Clarendon, Oxford) may be surprised by his earlier admission that 'that'-clauses are like names. But there is a further insight about 'that'-clauses to be found in Prior's earlier book (1957: *Time and Modality*, O.U.P, Oxford, 55–56):

> The theory with which Frege's name is especially associated is one which is apt to strike one at first as rather fantastic, being usually expressed as a theory that sentences are names of truth values. I do not think it is watering down Frege's actual viewpoint, and it certainly makes it less puzzling, if we consider him to be discussing not sentences but the corresponding 'that'-clauses 'That Caesar conquered Gaul', 'That Pegasus is white', and so on. These already look much more like names, and they are equivalent to phrases like 'The conquest of Gaul by Caesar', 'The whiteness of Pegasus', and so on, which look more noun-like still.

Indeed, sentences are not names, or even like them, and Frege's thought has to be corrected on that score. But there is a further detail of Frege's theory, besides his confusing sentences with 'that'-clauses, which must then really be 'watered down'. For 'That Caesar conquered Gaul' does not refer to the same truth as 'That 2 + 2 = 4', for instance. Certainly each refers to a proposition that is true, but neither of them refers to the one and only truth, let alone The Truth, as the object of reference. This second error of Frege's is most clearly seen in his attempt to do away with the subject-predicate conception of sentences, in the first place with respect to the passive transform. For Frege took 'Cato killed Cato' to say the same thing as 'Cato was killed by Cato', so he invented a symbolism that could express what he took to be their common thought: 'Kcc'. But that could arise from the application of λxKxc or λxKcx to c, in Church's Lambda Calculus. So the propositional referents of the two associated 'that'-clauses, 'That Cato killed Cato', and 'That Cato was killed by Cato', are not identical, as Frege would have it. Instead they are merely logically equivalent, i.e. they must have the same truth-value. Thus if 'That Cato killed Cato' is '$\lambda\phi$' while 'That Cato was killed by Cato' is '$\lambda\psi$' then while it is necessary that $\phi \equiv \psi$, still it is not the case that $\lambda\phi = \lambda\psi$. The main problem for Frege at this point, however, is that not only 'that'-clauses can occur as complements in indirect speech. For there are also propositional proper names and definite descriptions, like 'The Riemann Hypothesis', 'Goldbach's Conjecture', 'Fermat's Last Theorem', 'Euclid's First Proposition' and 'Pythagoras' Theorem'. So, for a start, it becomes necessary to symbolise propositional identities like 'Goldbach's Conjecture is that every even integer greater than 2 can be expressed as the sum of two primes'. These have the form 'x = $\lambda\chi$'. But, on Frege's doctrine about The True and The False, as objects of reference, all the theorems in mathematics refer to the same truth. Yet

Fermat's Last Theorem is clearly distinct from Pythagoras' Theorem, which means that the referents of the associated proper names and definite descriptions (as well as of the associated 'that'-clauses) must differ not only in indirect contexts, but also in direct ones like 'x = y'. All of this defeats Prior's theory of operators in *Objects of Thought*, of course. For now, clearly, 'There is something that Peter believes' can be formalised '(∃x)(Peter believes x)', and not just, as Prior tried to have it, '(∃χ)(Peter believes that χ)'. But why did Prior, indeed the bulk of modern formal logicians, not observe this?

The needed answer takes us to a point about reference that was little recognised in twentieth century formal logic. For we can say, for example, 'Peter believes the Riemann Hypothesis. It [that] is also believed by Paul'. So referential phrases can be used in such cases, as well as the empirical ones that modern logic has more often had in mind. But it is not *deictic* reference that is involved in the cases where there are propositional subjects; it is *anaphoric reference*, i.e., reference merely to subjects within a discourse. As a result it becomes clearer that it is also such anaphoric reference to subjects of discourse that is involved quite generally in connection with locating what the quantifiers in predicate logic range over (if that logic is taken to be *a priori*). For in logical contexts, if we have as a premise '(∃x)Fx' then it is common to go on 'call that thing which is F 'a', so Fa'. But standard predicate calculus cannot formalise all that is expressed here (though its extension, the epsilon calculus, can; see, for example my 2006: 'Epsilon Calculi' *Logic Journal of the IGPL* 14.4, 535-590, especially 572f.) For what is lacking in standard predicate logic is a formalisation of the demonstrative phrase 'that thing which is F'. Yet such a formalisation enables an immediate escape from the problem of 'empty names' which arises when it is thought that deictic reference is involved in predicate logic, since the anaphoric phrase 'that thing which is F' is merely referring to the subject brought up in the given premise, not directly to anything in the empirical world outside of the discourse. Someone may say, for instance, 'There is a mouse in the room' ((∃x)(Mx∧Rx)) and we may ask 'Where is it?', to which the reply might be 'It [that mouse in the room] is on the carpet' (Cϵx(Mx∧Rx)); and the epsilon term refers whether or not the existential statement is true. The central point that justifies this is that our question is entirely appropriate whether or not the first speaker is speaking truly or falsely. So the required, necessarily existing, abstract objects are just subjects of discourse reachable through anaphoric reference.

Hartley Slater
University of Western Australia

News

Norms Actions Games, 1–2 April

Many disciplines deal with conflict resolution. Game Theory does, by definition almost, as it studies interactive decision-makers with different beliefs and objectives; Philosophy does, because it's interesting to do; Computer Science does, because machines might have to handle conflicting imperatives, tasks and information, just like humans. Yet, these disciplines do not speak the same language: models, tools, techniques, to study

conflict resolution are scattered across their many sub-areas. If we think that we can profit from interdisciplinary collaboration at all, the problem becomes to come up with ideas for closing the gap.

In this respect, the Norms Actions Games Workshop, held at King's College London on April 1st and 2nd, was a very ambitious attempt; I knew this before starting. Putting together all those different backgrounds and having them talk, not to mention building up cross-disciplinary collaborations, would be a tough, if not impossible, objective to achieve. In any case, my fellow co-organizers Jan Broersen (Utrecht University), Alex Possajennikov (University of Nottingham) and Elizabeth Black (Kings College London), and I decided to go for it.

We all agreed that a typical Computer Science type of workshop, e.g., the one with 15 pages of novel work followed by Springer proceedings, would simply miss the point. We really didn't want to give any incentive to researchers beyond a competent audience to present their work to. Also, we didn't want to have long contributed talks, with presenters overindulging in discipline-related technicalities. Rather, we wanted a focus on ideas, novel or not, and interdisciplinary discussion.

We did have our trump cards, built up after months of organisational effort and mainly thanks to our sponsor, SINTELNET: an amazing venue, the Department of Informatics of King's College London, and a truly stellar line-up of invited speakers, namely Urs Fischbacher (experimental game theorist from the University of Konstanz), Jorgen Weibull (theoretical game theorist from the Stockholm School of Economics), Cristina Bicchieri (philosopher and experimental game theorist from the Unversity of Pennsilvanya), Martin van Hees (philosopher from the Unviersity of Amsterdam), Kai Spiekermann (philosopher from the London School of Economics), Michael Wooldridge (computer scientist from the University of Oxford) and Marek Sergot (computer scientist—and, at least according to me, philosopher, as well—from Imperial College London). The response was startling. We had 110 short abstract submissions—mostly from experimental game theory and philosophy, but overall covering the areas of game theory, philosophy, logic, computer science—and about 70 participants. Besides, we got lucky with the weather, as well. London, during the workshop days, was simply stunning.

Various ideas emerged during the talks and the active discussion time. In particular it became clear how the role of communication before an interaction can be crucial in promoting norm-following (what came out from Cristina Bicchieri's presentation, "I cannot cheat on you after we talk"), but that for a norm to be effective one needs punishment mechanisms and not just rewards (Urs Fischbacher pointed this out in his "To Punish or to Reward? An Experimental Study of Behavioral Sanctioning Norms"). In general, what people should do in a certain situation is not so clear-cut and prescriptions tend to be formulated in a coarse way. Kai Spiekermann has shown in what conditions the strategic use of information can occur to lower normative demands. Other aspects play a significant role in agents' interaction, such as the distinction between witting and unwitting action. Telling apart which consequences of an action an agent is actually responsible for bringing about is fundamental for understanding normative behaviour. This was pointed out by Marek Sergot in his "Action, agency and agent interactions". Marek was also concerned with the actual specification and implementation of artificial agents, an aspect that was taken up in Michael Wooldridge's talk, "Folk Theorem for

MultiAgent Systems". Mike's talk analysed the properties of players in a game that are implemented as automata, with severe restrictions on their decision-making on the one hand, but with an appealing practical flavour on the other, easing their actual construction as machines. All these ingredients have been argued to be crucial for understanding norms, especially if one is interested in formalising complex normative concepts such as Kant's categorical imperative, as Martin van Hees is. A unified formal theory of norms is still under development. Settled equilibria, presented by Jorgen Weibull, are an elegant and mathematically deep model of emergent social norms, which can now be studied with the classical tools of theoretical game theory.

Depicting the scientific debate during the workshop as a uniform flow of ideas complementing one another would not be telling the truth. I have seen quite some distance among the areas, not only in the methodology, as is obvious, but also in the terminology used, and a difficulty in communicating models that, in the end, have a lot in common. I do think, though, that NAG was able to make people feel the need for a common vocabulary, relating ideas and findings across so many different disciplines working on the very same topic. I am confident that there will be more such attempts to come.

Paolo Turrini
Computing, Imperial College London

Recent Work on the Logic of Ground, 5–6 June

The department of Philosophy at the University of Oslo hosted a workshop on Recent Work on the Logic of Ground on June 5–6. The goal was to bring together leading contributors to this new field.

The workshop was opened by Antje Rumberg (University of Konstanz) who discussed Bolzano's theory of grounding and its relationship to natural deduction proofs in normal form. Benjamin Schnieder (Hamburg University) discussed "Aristotle's Insight"—the idea that it is because Socrates is human that it's a truth that Socrates is human and not the other way around—and argued that we can derive the Insight from a modest theory of truth and uncontentious principles in the logic of ground.

Jon Litland (University of Oslo and csmn) discussed problems that arise for the impure logic of ground when self-referential constructions are allowed. He then showed how the notion of a "completely satisfactory explanation" can be used to develop a satisfactory impure logic of ground even when self-reference is allowed. Erica Schumener (NYU) considered the question whether (say) someone's being a bachelor is grounded in his being an unmarried man. She argued that we would make progress if we took such grounding claims as claims concerning essences. She then connected grounding with questions, arguing that the relation between a question and its answer can be one of ground.

Louis deRosset (Vermont) developed an account of ground on which, while there are non fundamental *truths* that are grounded, there are no non-fundamental *facts* to be amongst the relata of the grounding relation. He then showed how to make formal sense of this, while noting that this gives rise to a non-standard treatment of the grounding of disjunctions. Francesca Poggiolesi (CNRS, Université d'Aix-Marseille) presented a

novel impure logic of ground based on the Bolzanian idea that Γ grounds ϕ iff first, ϕ is both positively and negatively derivable from Γ and, secondly, ϕ is more complex than Γ.

Kit Fine (NYU) sketched a new foundation for essence and ground. He argued that both ground and essence have to be understood as generic. Amongst other things, he applied this idea to identity-criteria, holding that an identity criterion like "two sets are identical in virtue of having the same members" should be understood as a generic claim and not as a universally quantified statement. Jeremy Goodman (NYU) developed an account of coarse-grained facts that allowed us to draw a distinction between existentially (universally) quantified statements and the distjunction (conjunction) of their instances.

Fabrice Correia (Neuchatel) developed a new impure logic of "conceptualistic" grounding. A notable feature of his account was that statements of the form $\phi \vee (\psi \vee \theta)$ and $(\phi \vee \psi) \vee \theta$ were taken to have exactly the same grounds. He proved the logic sound and complete. Shamik Dasgupta (Princeton) argued that we should distinguish between two notions of metaphysical necessity. On one notion of metaphysical necessity the grounds necessitate what they ground; on the other notion of necessity they do not necessitate what they ground. He then applied this distinction to defend various broadly Leibnizian metaphysical views against the charge that they lead to indeterminism.

The workshop was well attended—both by locals and visitors. This contributed to a lively and fruitful discussion both during the formal sessions and over dinners and drinks.

Jon Erling Litland
Philosophy, Oslo

Imprecise Probabilities in Statistics and Philosophy, 27–28 June

A workshop on 'Imprecise Probabilities in Statistics and Philosophy' took place at LMU Munich on the 27th and 28th of June. The workshop was co-organised by the Munich Center for Mathematical Philosophy and the LMU statistics department. There were speakers from four continents, and a broad range of views in philosophy and statistics were represented. The conference was a great success and we hope that this leads to closer ties between the philosophy and statistics communities.

The conference opened with the first keynote talk by Teddy Seidenfeld who discussed two criteria for coherence of personal probabilities and their extensions to Imprecise Probabilities (IP). Next, Carl Wagner discussed an extension of Jeffrey conditioning to more general kinds of evidence. Frank Coolen then discussed *non-parametric predictive inference* which naturally gives rise to sets of probabilities. Catrin Campbell-Moore showed how IP arises when attempting to give a semantics for self-referential probabilities. Brian Hill argued that the standard dynamic choice argument against non-expected utility theories is mistaken. Arthur Paul Pedersen and Gregory Wheeler characterised the conditions under which a set of probabilities is subject to dilation. Frederik Herzberg discussed aggregation of infinitely many probability judgements. The first day of the conference closed with Arthur van Camp building bridges between approaches to rational belief based on desirable sets of gambles and choice functions.

The second keynote speaker, Fabio Cozman, opened day two of IPSP. He discussed the difficulties with finding a concept of independence for IP that satisfies standard graph-theoretical assumptions. Yann Bennetreau-Dupin pointed out that the problem with 'noninformative' (precise) priors being too informative can be overcome with IP and thereby solve paradoxes like the Doomsday paradox. Jan-Willem Romeijn discussed how to develop a theory of when statistical information sanctions full belief. Anthony Peressini used interval analysis applied to imprecise chances to avoid some problems with the discontinuous evolution of chance. Marco Cattaneo used a measure based on likelihoods to give some content to the 'reliability index' in Gärdenfors and Sahlin's Unreliable Probabilities model. Seamus Bradley argued that two prima facie problems for updating IP aren't problems once the proper interpretation of IP is used. Namjoong Kim discussed another problem for IP updating. The conference closed with our final keynote speaker, James M. Joyce, who discussed using scoring rules to model an agent's epistemic values (e.g., an agent's attitude to epistemic risk).

The workshop was supported by the Alexander von Humboldt Foundation, the LMU Statistics department and the LMU Universitätsgesellschaft. The keynote talks were filmed and the videos are available online through the media page of the conference website.

Seamus Bradley
MCMP, LMU Munich

Modelling, Simulating and Experimenting, 27–28 June

The workshop 'Modelling, Simulating and Experimenting' was organized by Andrea Loettgers and Marcel Weber and took place at the University of Geneva on June 27–28. The general question was addressed of how models, simulations, and experiments are related to objects in the world as well as to each other. It is often thought that experiments allow scientists to draw inferences more directly than models, which instead intervene on the stand-ins of the phenomena or objects. This is often taken as evidence of the epistemic superiority of experiments. The participants to the workshop asked, instead, whether we need a more nuanced view of the relationship of models, simulations, and experiments to their target system. The subject admits of many possible interpretations. Not surprisingly, no consensus emerged.

Claus Beisbart (University of Bern) and Arnon Levy (Hebrew University) both defended a philosophical position which opposes the integrative view. Beisbart distinguished between models and experiments, whereas Levy drew a line between theories and experiments. In Levy's view, models and simulations are positioned on the side of theory. He asked: How to go about the task of distinguishing between models and experiments / theories, and experiments? Based on the theory of action, Beisbart developed a minimalist framework, where methods are conceptualized as different types of activities which in turn are characterized by a primary goal. He applied this view to computer simulations, and showed how they can be categorized as experiments on the hardware of the computer.

Uskali Mäki (University of Helsinki) took a very different position by focusing on models, which for him aim to tell the truth about the world. Applying his method

of functional decomposition to the von Thunen model of agricultural land use, Mäki argued that modelling involves multiple components with specific functions and that good modelling is both pragmatically and ontologically constrained.

Sara Green (Aarhus University) as well as Andrea Loettgers (University of Geneva) took up more recent developments in biology in which mathematical modelling has become an important and widely used tool of inquiry. Green proved the heuristic role of mathematical abstractions in biological research, where one aims to identify function-precluding classes of models, which aim to reduce the number of possible explanations to be addressed experimentally. Loettgers discussed the role of the materiality of the different models, for example mathematical models and model organisms. She examined hybrid models, so called synthetic models. These are engineered genetic networks which, being located in-between mathematical models and experiments, integrate features from modelling and experimenting.

Adam Toon (University of Exeter) discussed the point made by scientific realists who describe instruments, simulations, and models as "extensions" of our normal cognitive capacities. He asked whether the realists could strengthen their view by drawing on the extended mind thesis, which claims that cognitive processes may extend beyond our brains and bodies into the environment. Toon argued that this view of mind and cognition has implications for a range of issues in philosophy of science, from debates between realists and constructive empiricists to recent discussion of the nature of scientific understanding.

Eric Winsberg (University of South Florida) offered a new take on the role of analogies in science. Winsberg discussed the role of fluid dynamical 'dumb holes' as analogue simulations of gravitational black holes. He argued that analogue simulations, unlike other species of analogical reasoning, can provide conformation, when the analogue provides knowledge of inaccessible features in the target system.

Mauricio Suárez (University of Madrid) defended the notion of propensities as theoretical entities. He argued that a coherent understanding of statisticians' practice calls for a distinction between three notions that are often conflated, namely probabilistic dispositions (or single-case propensities), chance distributions (or probabilities), and experimental statistics (or frequencies).

Andrea Loettgers
Philosophy, University of Geneva

Calls for Papers

Virtues & Arguments: special issue of *Topoi*, deadline 1 September 2014.
Maximum Entropy Applied to Inductive Logic and Reasoning: special issue of *Entropy*, deadline 1 December 2014.
Combining Probability and Logic: special issue of *Journal of Applied Logic*, deadline 15 January 2015.
Causation and Mental Causation: special issue of *Humana.Mente*, deadline 15 March 2015.

What's Hot in ...

Uncertain Reasoning

The 15th edition of the 'Information Processing and Management of Uncertainty' (IPMU) conference was held in Montpellier on 15-19 July 2014. In the tradition of the series, this event was characterised by the diversity of approaches to the broad area of uncertain reasoning, from the logico-mathematical foundations to the applications in engineering and agriculture. The three-volume strong IPMU 2014 proceedings are available from Springer's *Communications in Computer and Information Science*.

Among the many noteworthy papers presented at the workshop I think that readers of The Reasoner may find Ins Couso's 'Preference Relations and Families of Probabilities: Different Sides of the Same Coin' particularly interesting. The paper surveys, from the statistician's point of view, the decision-theoretic underpinnings of classical and imprecise Bayesianism. In particular the paper reviews how *stochastic preferences* give rise to classical expectation as well as its imprecise generalisations. Despite its relatively short length it provides a rather exhaustive list of relevant references and therefore this paper can serve as a compact but very useful entry point to this literature.

The derivation of imprecise probabilities (broadly construed so as to include upper/lower previsions as well as sets of probabilities) from preference suggests quite naturally that the long-standing problem of devising norms for rational decision under imprecise probabilities could be tackled as a problem of *aggregation* or *multi-criteria* decision-making, where each probability in a given set of probability functions provides an individual criterion of rational belief. Viewing decision-making with imprecise probabilities in this way makes room for a potentially very fruitful and presently underappreciated application of a whole stock of methods, results and techniques of social choice theory—including its logical offspring, namely the theory of judgment aggregation—to the theory of imprecise Bayesianism.

Hykel Hosni
Marie Curie Fellow,
CPNSS, London School of Economics

Events

August

Reasoning Minds: Reasoning About Other Minds: Logical and Cognitive Perspectives, Groningen, 4–5 August.
AiML: Advances in Modal Logic, University of Groningen, 5–8 August.

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ICPP: 13th International Conference on Philosophical Practice, Belgrade, 15–18 August.
CLIMA: 15th International Workshop on Computational Logic in Multi-Agent Systems, Prague, Czech Republic, 18–19 August.
STAIRS: 7th Starting AI Researcher Symposium, Prague, Czech Republic, 18–19 August.
SBQ: Science and the Big Questions, VU University Amsterdam, 18–21 August.
ECAI: 21st European Conference on Artificial Intelligence, Prague, Czech Republic, 18–22 August.
KNEW: Cognitive Science of Science, Kazimierz Dolny, Poland, 18–22 August.
DARE: International Workshop on Defeasible and Ampliative Reasoning, Prague,

Czech Republic, 19 August.
ROBO-PHILOSOPHY: Aarhus University, Denmark, 20–23 August.
Knowing Minds: Conference in Honour of George Botterill, Sheffield, 21 August.
Causal Explanation: in Psychiatry, VU University Amsterdam, 22 August.
Hypo: Hypothetical Reasoning, Tübingen, Germany, 23–24 August.
SLS: 9th Scandinavian Logic Symposium, University of Tampere, Finland, 25–27 August.
ECAP: 8th European Conference of Analytic Philosophy, University of Bucharest, Romania, 28 August–2 September.
Social Mind: Origins of Collective Reasoning, University of Oslo, 29–30 August.

September

WoLLIC: 21st Workshop on Logic, Language, Information and Computation, Valparaiso, Chile, 1–4 September.
LPOSGW: Approaches Within Philosophy of Science, London, 2–3 September.
SOPhiA: Salzburg Conference for Young Analytic Philosophy, Austria, 4–6 September.
Collectivity: Bristol, 5–7 September.
DGN: Decisions, Groups, and Networks, LMU Munich, 8–9 September.
WPMSIIP: 7th Workshop on Principles and Methods of Statistical Inference with Interval Probability, Ghent, Belgium, 8–12 September.
COMMA: 5th International Conference on Computational Models of Argument, Scottish Highlands, 9–12 September.
BPPA: British Postgraduate Philosophy Association Conference, Leeds, 9–12 September.
ENPOSS: 3rd European Network for the Philosophy of the Social Sciences Conference, Madrid, 10–12 September.
GANDALF: 5th International Symposium on Games, Automata, Logics and Formal Verification, Verona, Italy, 10–12 September.
CI: Collective Intentionality, Indiana, USA, 10–13 September.
X-Phi: 5th Workshop of Experimental Philosophy Group UK, Oxford, 11–12 September.
M & I: Models and Inferences in Science, Rome, 11–13 September.
LANCOG: workshop on Modal Syllogistic, Lisbon, 11–13 September.
PAM: Predicate Approaches to Modality, MCMP, LMU Munich, 12 September.
SCLC: 10th Symposium for Cognition, Logic and Communication, University of Latvia, Riga, 12–13 September.
AICS: Artificial Intelligence and Computer Science, Bandung, Indonesia, 15–16 September.
SUM: 8th International Conference on Scalable Uncertainty Management, Oxford, UK, 15–17 September.
CCC: Continuity, Computability, Constructivity: From Logic to Algorithms, University of Ljubljana, 15–19 September.
NoR& N: Nature of Rules and Normativity, Prague, Czech Republic, 17–19 September.

IWSBP: 11th International Workshop on Boolean Problems, Freiberg, Germany, 17–19 September.
ICTCS: 15th Italian Conference on Theoretical Computer Science, Perugia, Italy, 17–19 September.
PGM: 7th European Workshop on Probabilistic Graphical Models, Utrecht, The Netherlands, 17–19 September.
ARD: Argumentation, Rationality and Decision, Imperial College London, 18–19 September.
EERG: Buffalo Annual Experimental Philosophy Conference, Buffalo, 19–20 September.
ICSS: International Conference on Social Sciences, Bucharest, Romania, 19–20 September.
FOIS: 8th International Conference on Formal Ontology in Information Systems, Rio de Janeiro, 22–25 September.
KI: 37th German Conference on Artificial Intelligence, Stuttgart, 22–26 September.
LAP: Logic and Applications, Dubrovnik, Croatia, 22–26 September.
JELIA: 14th European Conference on Logics in Artificial Intelligence, Madeira Island, Portugal, 24–26 September.
EoM: Epistemology of Modality, Aarhus University, Denmark, 24–26 September.
IEEE: Intelligent Systems, Warsaw, Poland, 24–26 September.
LANCOG: Workshop on Analyticity, Lisbon, 25–26 September.
EoP: Epistemology of Perception, KU Leuven, 25–26 September.
EFAK: Disagreements, University of Tartu, 25–27 September.
Johan van Benthem: ILLC, Amsterdam, 26–27 September.
Belief: 3rd International Conference on Belief Functions, Oxford, 26–28 September.
DoI: Dimensions of Intentionality, Ruhr-Universität Bochum, Germany, 29 September–1 October.
PMR: Proof Theory, Modal Logic and Reflection Principles, Mexico City, 29 September–2 October.

October

WCPA: Western Canadian Philosophical Association, Vancouver, BC, 3–5 October.
FPMW: 6th French Philosophy of Mathematics Workshop, Toulouse, 9–11 October.
Descartes Lecture: Leitgeb on Rational Belief, Tilburg University, Netherlands, 20–22 October.
EBC: Explanantion Beyond Causation, LMU Munich, 23–24 October.
ILCS: Inductive Logic and Confirmation in Science, University of Utah, 24–25 October.
ICSR: Knowledge Representation and Reasoning in Robotics, Sydney, Australia, 27–29 October.
MDAI: Modeling Decisions for Artificial Intelligence, Tokyo, Japan, 29–31 October.
IDA: 13th International Symposium on Intelligent Data Analysis, Leuven, Belgium, 30 October–1 November.

ECSI: European Conference on Social Intelligence, Barcelona, Spain, 3–5 November.
ACGC: 8th Arché Graduate Conference, University of St Andrews, 8–9 November.
LORENTZ: Logics for Social Behaviour, Leiden, 10–14 November.
SoPhiSci: Social Philosophy of Science, Moscow, Russia, 18–19 November.

Courses and Programmes

Courses

EASLLC: 3rd East-Asian School on Logic, Language and Computation, Tsinghua University, China, 2–8 July.
Carnegie Mellon: Summer School in Logic and Formal Epistemology, 2–20 July.
INEM / CHESS: Summer School in Philosophy and Economics, University of the Basque Country, Donostia-San Sebastian, Spain, 21–23 July.
SIPTA: 6th SIPTA School on Imprecise Probabilities, Montpellier, France, 21–25 July.
MCMP: MCMP Summer School on Mathematical Philosophy for Female Students, Munich, Germany, 27 July–2 August.
ESSLLI: 26th European Summer School in Logic, Language and Information, University of Tübingen, Germany, 18–22 August.
Epistemology & Cognition: Groningen, 25–29 August.
IJCAI: 2nd IJCAI School on Artificial Intelligence, Buenos Aires, Argentina, 1–5 September.
CLPA: Summer School on Argumentation: Computational and Linguistic Perspectives on Argumentation, University of Dundee, Scotland, 4–8 September.
CSSiP: 9th Cologne Summer School in Philosophy on Practical Reasons, Cologne, 15–19 September.
Geometry and Physics: 17th International Summer School in Philosophy of Physics, 15–19 September.
AAAI: Texas, USA, 25–29 January.
Combining Probability and Logic: University of Kent, 20–21 April.

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.

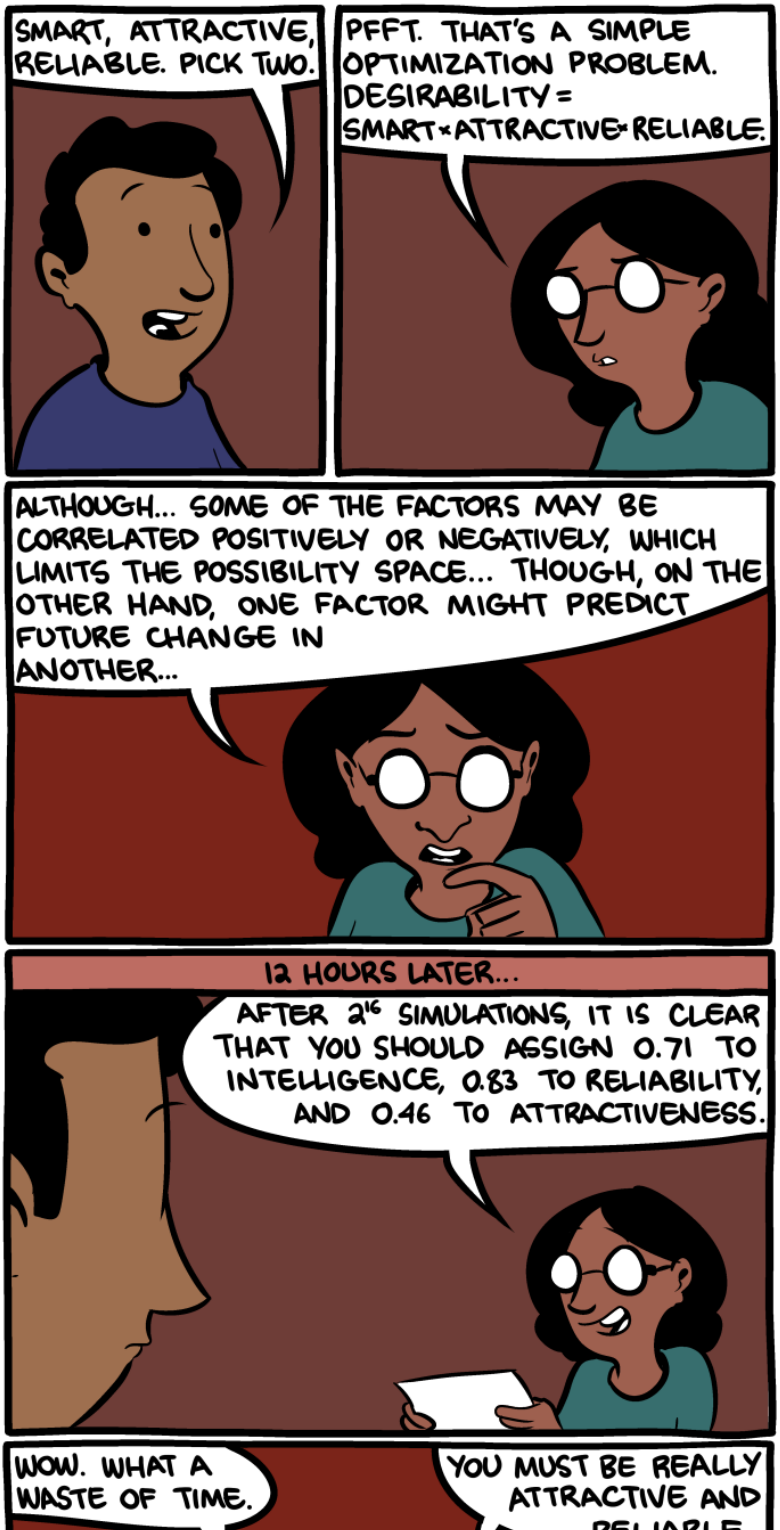
Jobs and Studentships

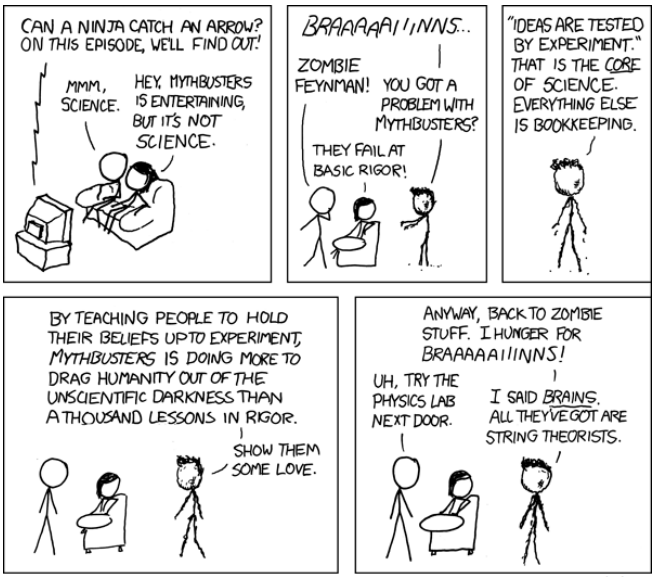
Jobs

Post-doc Position: in Set Theory, Torino University, until filled.
Professor: of Uncertainty Quantification, School of Mathematical Science, University of Nottingham, until filled.
Permanent Positions: Federal University of Bahia, Brazil, until filled.
Lecturer: in Philosophy of Science, University of Auckland, deadline 16 August.
Lecturer: in Metaphysics and Philosophy of Science, University of Bristol, deadline 20 August.
Postdoc Position: in Philosophy, University of Oslo, deadline 1 September.
Postdoc Position: on the project "Grading evidence of mechanisms in physics and biology," Philosophy, University of Kent, deadline 3 September.
Lecturer: in Theoretical Probability, School of Mathematical and Physical Sciences, University of Reading, deadline 12 September.

Studentships

PhD Position: in Philosophy, University of Oslo, deadline 1 September.
PhD Position: on the project "Grading evidence of mechanisms in physics and biology," Philosophy, University of Kent, deadline 3 September.
PhD Position: in Computational, Mathematical or Philosophical Logic, University of Pretoria, deadline 30 October.


SMART, ATTRACTIVE, RELIABLE. PICK TWO.
PFFT. THAT'S A SIMPLE OPTIMIZATION PROBLEM. DESIRABILITY = SMART×ATTRACTIVE×RELIABLE.
ALTHOUGH... SOME OF THE FACTORS MAY BE CORRELATED POSITIVELY OR NEGATIVELY, WHICH LIMITS THE POSSIBILITY SPACE... THOUGH, ON THE OTHER HAND, ONE FACTOR MIGHT PREDICT FUTURE CHANGE IN ANOTHER...
12 HOURS LATER...
AFTER 2^{16} SIMULATIONS, IT IS CLEAR THAT YOU SHOULD ASSIGN 0.71 TO INTELLIGENCE, 0.83 TO RELIABILITY, AND 0.46 TO ATTRACTIVENESS.
WOW. WHAT A WASTE OF TIME.
YOU MUST BE REALLY ATTRACTIVE AND


CAN A NINJA CATCH AN ARROW? ON THIS EPISODE, WE'LL FIND OUT!
MMM, SCIENCE.
HEY, MYTHBUSTERS IS ENTERTAINING, BUT IT'S NOT SCIENCE.
BRAAAAAI!INNS...
ZOMBIE FEYNMAN!
YOU GOT A PROBLEM WITH MYTHBUSTERS?
THEY FAIL AT BASIC RIGOR!
"IDEAS ARE TESTED BY EXPERIMENT." THAT IS THE CORE OF SCIENCE. EVERYTHING ELSE IS BOOKKEEPING.
BY TEACHING PEOPLE TO HOLD THEIR BELIEFS UPTO EXPERIMENT, MYTHBUSTERS IS DOING MORE TO DRAG HUMANITY OUT OF THE UNSCIENTIFIC DARKNESS THAN A THOUSAND LESSONS IN RIGOR.
SHOW THEM SOME LOVE.
ANYWAY, BACK TO ZOMBIE STUFF. I HUNGER FOR BRAAAAAI!INNS!
UH, TRY THE PHYSICS LAB NEXT DOOR.
I SAID BRAINS. ALL THEY'VE GOT ARE STRING THEORISTS.

xkcd.com