THE REASONER

CONTENTS

EDITORIAL

I am delighted to be the guest editor for the eighth anniversary edition of the Reasoner. I want to take this opportunity to thank the editors for helping us all keep abreast of the latest developments in the field of reasoning. It has been successful and I'm certain it will continue to be so!

It's a great pleasure to have interviewed Robbie Williams (J. Robert G. Williams) for this edition.

He has important work in many areas, often relating to indeterminacy, including working out what it means for one thing to represent another, what the metaphysics of conditionals are, and what patterns of belief are rational when agents don't believe in classical logic. I first encountered Robbie's brilliant work on non-classical probabilities, so I decided to ask him some questions about that today.

CATRIN CAMPBELL-MOORE
Munich Center for Mathematical Philosophy

FEATURES

Interview with Robbie Williams

Catrin Campbell-Moore: Hi, thanks very much for agreeing to do this interview for the Reasoner.

Robbie Williams: Hi! No problem.

CC-M: Perhaps first you could tell us a little bit about your background. What led you into studying philosophy? Was it always your plan?

RW: It wasn't always a plan. In the UK you have to choose between the arts and sciences at 16. I had chosen maths and sciences, but wasn't completely happy with that. So at university I wanted something that could also give me some scope to keep the arts side as well, so I ended up studying Maths and Philosophy. Philosophy suited me nicely because you have this freedom to think about almost anything. It also helped that it didn't require you to have studied it before.

CC-M: And how about your current research areas, how did you start to find out about them and get interested in them?

RW: I had a kind of weird route. When I started doing my masters I thought I was going to be doing Wittgenstein studies. It was a thing in Oxford at the time I was there. I ended up drifting away from Wittgenstein studies because all the metaphilosophy seemed less and less attractive. There are some articles of faith for the people who are doing that that I didn't want to buy into, like "there are no arguments in Philosophy".

When I moved to St Andrews to start my PhD, I had been interested in Philosophy of Maths for a while, and particularly reference to mathematical objects. For example there are puzzles for how you apparently manage to refer to entities where there are internal symmetries, like in the complex numbers where you've got i and $-i$, they've got all the same mathematical properties, so can we manage to refer to one of them? So I was thinking about that and that that might be a source of indeterminacy: maybe it's indeterminate which object we're picking out. Thinking about that I started thinking about other kinds of indeterminacies: indeterminacies that might be generated by vagueness, indeterminacies in the gavagai literature where it might be indeterminate whether you're referring to rabbit parts, rabbit stages, or 4D fusions of rabbit parts. So I ended up writing my PhD thesis on indeterminacy, trying to answer questions like what does it take for reference to be indeterminate, how to model indeterminacy and how to theorise about cases of indeterminacy.

CC-M: You have three research projects:

1. The nature of representation.

2. Life in a nonclassical world.

3. Conditional thinking

Do you see them as three separate projects, and work on them quite independently, or do they connect as part of a larger project?

RW: There's no overarching goal which will be revealed in 10 years or so, I find them all intrinsically interesting. It's not my methodology to run things with the idea that the ends of the puzzle should fit into some prefixed idea of where we're heading. Obviously there's a set of prejudices I bring to each, but I'm running them, at least in principle, autonomously.

However, the projects are interconnected, and the interconnections you find between them go quite deep. So, for example, one of the things that I'm working on in connection to the non-classical logics is about how you think about rational debates about logic: how you choose one logic over another, and how you represent somebody rationally being uncertain about whether classical or intuitionistic logic is the right way to go. An answer to this question will affect what doxastic possibilities and belief states are. This will then have an impact on the conception of the target you're trying to account for in the foundations of representation project.

CC-M: What's your most exciting current research project?

RW: That's like asking for a favorite child, they're all interesting!

What I find most exciting is when you start to understand a debate that was previously a mystery to you. For example, for a long time I couldn't find a way to represent in the framework I was familiar with, these traditional empiricist puzzles like what does it take to represent genuine causation in the acts in the world. But within the frameworks for theorising about foundations of representation I'm working in at the moment, there seem to be analogues of exactly those kinds of puzzles. So without endorsing the particular theory of representation the empiricists were working on, you can still formulate exactly these kinds of challenges from representation scepticism. It's

like learning a new language, you've now got access to far more than you otherwise would have had. And that's exciting.

CC-M: I'm particularly interested in your work on non-classical probabilities; perhaps you could give a brief introduction to them. How do probabilities and logic fit together?

RW: If you're thinking about probabilities as models of rational belief states, so degrees of belief that are attaching to something like Fregean thoughts, then what you seem to be committed to is that the probabilistic axioms look like rational constraints on those belief states. You then look at those axioms and see that e.g., every logical tautology has probability 1, that you're not allowed to drop your confidence over logically valid sequents, and a partitioning principle saying: if you've got a thought where the logic tells you it can only become true in two mutually exclusive and exhaustive ways, then your degree of confidence in the thought should be the sum of the beliefs you have in those two ways. So by thinking about what probability, as a model of rational belief states, is committed to, it falls out that what it's doing is taking a whole bunch of logical properties and formulating rational constraints in terms of those. That's basically a thought I'm inheriting from the way Hartry Field presents these things.

CC-M: What's different when non-classical logics are considered?

RW: In non-classical probabilities you retain the abstract principles I mentioned, such as the partition principle, but you don't have the instances of it which are the things we practically work with. For example you might have a supervaluational probability with $P(A) = 0$, $P(\neg A) = 0$ and $P(A \vee \neg A) = 1$. That's just going to be ruled out in the classical probability setting we're used to working with. This is because of the different relationships that classical logic and supervaluational logic say obtains between a disjunction and its disjuncts. In the classical setting it'll just be automatically the case that if you have incompatible disjuncts they will count as a partition of the disjunction, so you will have as an instance of the partition principle that $P(A \vee \neg A) = P(A) + P(\neg A)$, but that's a logical principle that fails in supervaluational logic. So there's a level of generality across these logics, but also differences in details.

CC-M: What will end up deciding between different logics, and which logic ends up as rationally binding? Is there a subjective element to it?

RW: My thought at the moment is that none of these probability theories that embed a particular logic really get at the rational requirements. Rational requirements are instead a relation between your degrees of belief and your doxastic space, and no logic is baked into that space. What happens with these non-classical probability theories is that they are things to which your rational degrees of belief approximate

when you invest high credence in one or another logic, so that would make the logical rationality requirements very much turn on the subjective matters of what logic you're happening to accept at that point.

CC-M How do you conceive of these rational requirements, or norms? Do you think of them as evaluative?

RW: I think the job description for rational requirements is something like articulating a bunch of preconditions, or ways that agents have to be, for us to have a distinctive collaborative relation with them. So if someone's rational they're open to persuasion by presenting them with evidence and reasons but if they're (badly) irrational we need to try to *causally* change their beliefs, perhaps by giving them therapy.

Often this question of whether the norms are evaluative is presented as if the question "why should you be rational" is being asked from a third person or a first person perspective. I think of it as essentially third personal. If we couldn't persuade an irrational agent that they'd be better being rational then that wouldn't be a surprise to me, after all they're irrational! But there is something in the substantive answers to the question which has a large dollop of first personal considerations. You don't want to rest content by deeming huge bunches of the populations as not being worthy of interaction or collaboration because they're getting some things wrong by your lights. You want rationality to depict these people as responsive to reasons, if you can.

CC-M: You have a lot of research about degrees of belief but not much about all-or-nothing belief. Is there a principled reason for that? What do you think about the question of all-or-nothing belief vs degrees of belief?

RW: I find the reasons for believing in degrees of belief, like the lottery paradoxes and preface paradoxes, more compelling than I find the reasons for theorising in terms of all-or-nothing belief, which is why I start there.

That being said, I have been thinking about the all-or-nothing case recently in my project on the metaphysics of representation. Lots of the theories there are framed in terms of all-or-nothing belief. E.g., if you look at Dretske, the way he characterises representation depends on something like toggling the belief on and off and seeing how the world toggles. It's very difficult to see how to generalise that to the degrees of belief setting.

The approach to all-or-nothing beliefs I find most attractive is the way of thinking about them that derives from Bratman. The general idea is that all-or-nothing belief is something like coping with problems of limitations. If you imagine a creature that's rich in storage but poor in processing power, then you could think of full beliefs as cutting down all the information about credences and just extracting the information that allows you to solve a particular problem. There's an awful lot to be said about how credences and beliefs fit together if you do go that way.

CC-M: Are you only interested in the application of these probabilities to an agent's degrees of belief, or also for other interpretations of probability, like objective chance?

RW: I've only worked on the degrees of belief front. Formally developing a theory of objective chance on this basis should be relatively straight-forward but I haven't found a puzzle to do with non-classicism and chance that strikes me as sufficiently distinctive from what I say on the subjective front to motivate me to explore that directly.

CC-M: In your work you have mentioned many different applications of your non-classical logics to cases of indeterminacy, e.g., vagueness, indeterminacy about the future, paradoxes of self-reference.... Which application is the most important to you?

RW: The one that I find most gripping is the personal identity one, so cases of indeterminacy across time where somebody has changed radically, psychologically or physiologically, and the question of at what stages are we the same person? One reason for that is that it's something we really care about in lots of ways. It really matters to me, as an agent making decisions about my future self, how I should factor in the interests of someone where there's no fact to the matter as to whether they're me or not.

I think I would find things like future contingents similarly interesting if I wasn't so sceptical that there are genuinely cases of this kind of indeterminacy. I'm pretty convinced there are cases of indeterminate identity, I am not convinced that there are cases of indeterminacy in future contingents.

CC-M: What's the next project?

RW: I'm currently writing a book on indeterminacy. There is also still two and a half years to run on this representation project, so there's quite a lot of stuff I'm committed to thinking through now.

One thing I'm thinking about right now is the extension of some of this stuff on non-classical probabilities, in particular decision theories that go along with them, to ethical, rather than credential, decision making. That actually relates to what you asked about chances earlier on. If you can have indeterminacies in the goals then you're going to have to allow chances to be defined over the kinds of items that take indeterminate truth values. Whether that gets into the book or not is another matter.

CC-M: Thank you very much!

RW: Thanks for asking me!

The inherent risks in using a name-forming function at object language level

The truth problem is one of the central problems of philosophy. Nowadays, every major theory of truth that applies to formal languages utilizes devices referring to formulae. Such devices include name-forming functions. The theory of truth discussed in this paper applies to strict formal logical languages, the critique of which must, therefore, also obey mathematical rigour. This is why I have used formal logical derivations below rather than the argumentation of ordinary language.

The first derivation below demonstrates that some name-forming functions produce an antinomy. However, in the first instance we can straightforwardly escape from the trap by denying the existence of that given function. In the second

derivation, however, I prove that the citation function can also produce antinomy. Furthermore, in this case we cannot easily escape from the trap, because denying the existence of the citation function is counterintuitive.

In mathematics it is often difficult to understand that two formulae are equivalent, although it is evident that a formula is always equivalent to itself. The axiom that follows (Axiom A) makes a similar claim in the domain of logical formulae.

Let L be a first-order logical language, including the one-to-one name-forming function ξ, which is a mapping from formulae to names. Let ξ^{-1} be the inverse of ξ. The iteration of the operator ξ is acceptable. If z is a formula name of L, then $\xi^{-1}(z)$ is a formula of L. There are no restrictions for ξ, and hence, Axiom A, which is a version of Tarskis β, is intuitive:

(A) $\exists \xi((\xi$ is a one-to-one function of L in the domain of L WFFs) & $\forall x \forall y$(if x and y are L sentence names and $x = y$, then $(\xi^{-1}(x) \leftrightarrow \xi^{-1}(y))))$

The following derivations are based on Quine's deduction technique outlined in his book "Methods of Logic". What follows is a proof by contradiction: the asterisk indicates the original premise which is assumed, and all subsequent consequences of that premise. The absence of an asterisk prefix to (7) indicates that that line does not depend on previous premises, but instead holds absolutely. The absence of an asterisk thus indicates a claim to validity. The argument, and specifically premise (2), is a formulation of Tarski's derivation of an antinomy from the unrestricted use of quasi-quotation. (For further clarification of this point and its relation to Tarski see the discussion that follows this derivation.)

There are no specific restrictions for ξ; so, we apply premise (2):

*(2)	$\exists x(x$ is a sentence name of L & $x = \gamma(\forall y(x = y \to \sim \gamma^{-1}(y))))$	
	γ is such a bijective function	(A)
*(3)	$z = \gamma(\forall y(z = y \to \sim \gamma^{-1}(y)))$	(2) z
*(4)	$\gamma^{-1}(z) \leftrightarrow \forall y(z = y \to \sim \gamma^{-1}(y))$	(A) (3)
*(5)	$\gamma^{-1}(z) \leftrightarrow (z = z \to \sim \gamma^{-1}(z))$	(4) $y = z$
*(6)	$\gamma^{-1}(z) \leftrightarrow \sim \gamma^{-1}(z)$	(5)
(7)	If (A) and (2) are true, then $\gamma^{-1}(z) \leftrightarrow \sim \gamma^{-1}(z)$	(A) (2)
(8)	If (A) is true, then there is no such γ function.	(7)

Since axiom (A) is a plausible assumption, it is preferable to deny the existence of function γ and preserve (A). Nonetheless, it is clear that this argument is readily transformable to another inference based on another name-forming function. What if we substitute function γ with the so-called citation function?

Tarski designated the "$(\lambda x)\ulcorner x \urcorner$" function as the "citation function" or "quasi-quotation," distinguishing the usage of the citation function from the normal usage of quotation marks. The quasi-quotation is only one possible name-forming function among many others, like Gödel numbering.

In the domain of well-formed formulas (WFFs), the quasi-quotation function is not a partial function; however, other name-forming functions can be partial functions. This means that one can form an individual constant of any WFF or term by applying quasi-quotation, and can quote quote-names; however, other name-forming functions leave one's hands tied: in other cases, it is not permitted to iterate the use of name-forming functions. It must be noted that there is no problem in using quasi-quotation at the metalanguage level in the domain of formulas of the object language. In this case, $\ulcorner p \urcorner$ is not a name of object language names, but a name of metalanguage names. In another case, if one applies a formal logical language including quasi-quotation at the object language level, then one has to handle this device very carefully. We know from Tarski that quasi-quotation itself is a very risky device which can produce antinomy. Tarski only sketched the argument:

> Let the symbol 'c' be a typographical abbreviation of the expression 'the sentence printed on this page, line 6 from the top'. We consider the following statement: for all p, if c is identical with the sentence 'p', then not p . . . We establish empirically:
> (α) the sentence 'for all p, if c is identical with the sentence 'p', then not p' is identical with c.
> In addition we make only a single supplementary assumption which concerns the quotation-function and seems to raise no doubts:
> (β) for all p and q, if sentence 'p' is identical with sentence 'q', then p if and only if q.
> By means of elementary logical laws we easily derive a contradiction from the premises (α) and (β)

> Tarski (1936: The concept of truth in formalized languages, in Logic, Semantics, Metamathematics, OUP, p.162)

Although Tarski's argument has been reconstructed before, the reconstruction below appears to be novel.

Let "$(\lambda x)\ulcorner x \urcorner^{-1}$" symbolize the denotation function: if x is a formula name of L, then $\ulcorner x \urcorner^{-1}$ is a formula of L. Let us then consider the following inference:

(B) $(((\lambda x)\ulcorner x \urcorner$ is a one-to-one citation function of L in the domain of L WFFs) & $\forall x \forall y$(if x and y are L sentence names and $x = y$, then $\ulcorner x \urcorner^{-1} \leftrightarrow \ulcorner y \urcorner^{-1}$))

There are no specific restrictions for $(\lambda x)\ulcorner x \urcorner$ name-forming function, so we apply premise (2):

*(2)	$\exists x(x$ is a sentence name of L & $x = \ulcorner \forall y(x = y \to \sim \ulcorner y \urcorner^{-1}) \urcorner)$	(B)
*(3)	$z = \ulcorner (\forall y(z = y \to \sim \ulcorner y \urcorner^{-1}) \urcorner$	(2) z
*(4)	$\ulcorner z \urcorner^{-1} \leftrightarrow \forall y(z = y \to \sim \ulcorner y \urcorner^{-1})$	(B) (3)
*(5)	$\ulcorner z \urcorner^{-1} \leftrightarrow (z = z \to \sim \ulcorner z \urcorner^{-1})$	(4) $y = z$
*(6)	$\ulcorner z \urcorner^{-1} \leftrightarrow \sim \ulcorner z \urcorner^{-1}$	(5)
(7)	If (B) and (2) are true, then $\ulcorner z \urcorner^{-1} \leftrightarrow \sim \ulcorner z \urcorner^{-1}$	(B) (2)
(8)	If (B) is true, then there is no such $(\lambda x)\ulcorner x \urcorner$ name forming function.	(7)

Thus, applying the citation function as a name-forming device at the object language level does indeed result in an antinomy. It follows from the above-mentioned inference that any theory including (B) – similar to the Revision Theory of Truth – is inconsistent. Gupta and Belnap declare in their seminal work: " . . . L contains for each sentence A a quotational

name '*A*'. The interpretation I assign to the name '*A*' the sentence *A*." Gupta and Belnap (1993: The Revision Theory of Truth, The MIT Press, p.75). Philip Kremer says: "L^- will have a quote name '*A*' for every sentence *A* of L^-." (2014: The Revision Theory of Truth, *The Stanford Encyclopedia of Philosophy*, Summer 2014 Edition).

The conclusion is that it is advisable to use name-forming devices at the metalanguage level; otherwise, at the object language level, one must carefully restrict the domain of the name-forming function to avoid its iterated usage.

I thankfully acknowledge the helpful assistance received from Peter Fekete.

FERENC ANDRÁS
Regional Environmental Center for Central and Eastern Europe

NEWS

Causation in biology, 27–28 April

A workshop 'Causation in biology—and related topics' was held at the University of Copenhagen, organized by the project 'Causation and Reduction in Systems Biology' based at the University of Oslo. The workshop brought together a group of philosophers of science working on topics related to causation and philosophy of biology. This is a brief report of the event.

First things first: the event started with a lunch at the Niels Bohr institute, the venue for the workshop. First speaker was yours truly, with a presentation comparing the epistemic characteristics of theoretical modelling and model organism research. I argued that these practices rely on analogous strategies for justifying model-to-target inferences. However, they are still epistemically distinct: theoretical models extend our inferential abilities, whereas model organism research creates new evidence and explananda that are extrapolated to other real-world systems. In the discussion a critical point was raised: theoretical modelling can also reveal phenomena that are, in practice, truly novel and surprising to the researchers.

Next, Lorenzo Casini from university of Geneva presented an account of evidence conditions of constitutive explanations. Lorenzo opened with a critique of Carl Craver's mutual manipulability thesis: there can be no interventions on the relata of a constitution relation that would satisfy the Woodwardian conditions for intervention variables that Craver needs. From this he argued for an account where constitutive relations are inferred from robust evidence showing that a correlation between two variables cannot be broken by interventions. The talk sparked a discussion on the interpretations of Craver's account, pros and cons of using directed graphs to represent mechanisms, and many spin-off topics in philosophy of explanation.

Lise Marie Andersen from Aarhus University gave the day's last talk, which was a critical examination of a proposal that classification in psychiatry should be based on brain mechanisms. Drawing from the work of Kenneth Kendler, Lise Marie argued that such a classification scheme might prevent us from identifying causes of psychiatric disease potentially more relevant as therapeutic targets than biological causes, and would thus introduce an unwanted bias to psychiatric research. After this talk the participants enjoyed a lovely workshop dinner and continued discussions on the day's topics.

Day two was started off by Gry Oftedal, one of the organizers, who gave a talk on causal strength and importance, and the attempts to interpret these notions and develop objective measures for them. Various proposals to identify these notions with the difference-making ability of a cause with respect to an effect were presented. For instance, a cause may be important in the sense that it is an actual difference-maker for variation seen in a population. On the other hand, such a cause may be weaker than some other causal factor, if its difference-making power is dependent on the particular environment in which the population is embedded. Likewise, other properties of difference-making relations such as stability and sensitivity may provide cross-cutting judgements of causal strength and importance. The prospects of developing a measure of causal strength and importance independent of particular explanatory interests was discussed.

Next, Marie Kaiser from University of Cologne explored the limits of causal modelling with directed graphs, utilizing an example of DNA-protein recognition. DNA-protein recognition is sensitive to shapes, spatial distribution and chemical structures of the components involved, thus requiring that such information is included into a causal graph proposed to explain it. One way to do this would be to include this information into the characterization of the variables of the graph. However, this strategy was found unsatisfying due to being overly complicated. Another way would be to represent all the aforementioned information by one variable. But this strategy would compromise the explanatory power of the model. Again, the prospects of modeling mechanisms with causal graphs were discussed in the Q and A.

Johannes Persson from Lund University presented with the title 'How- and why-explanation in ecology—and beyond'. Johannes presented a case study—a field experiment in aquatic ecology—that exemplified how ecologists decompose an explanatory task into proximate (how-question) and ultimate (why-question) explanations according to Ernst Mayr's famous distinction. Johannes then surveyed the 'new mechasnist's' views on mechanisms as answers to how-questions. It was argued that mechanisms do not fit the bill for answering the how-questions that ecologists are thinking of. A discussion ensued about how to distinguish ultimate/proximate and why/how-explanations, and what explananda the new mechanist's really think mechanistic explanations address.

Anders Strand from University of Oslo, the other organizer of the workshop, closed with a presentation on the metaphilosophical question of how philosophy of causation should be done. According to Anders, one could do a truth-conditional analysis of causation while remaining agnostic about fundamental metaphysics. The former analysis should be informed by investigation of scientific and everyday uses of causal concepts, and should aim at a functionalist account of the role that causal concepts play in these contexts. This analysis is primary in the sense that the role of a metaphysical account should be to explain why the causal truths identified according to the functional analysis hold, rather than epistemology of causation being constrained by prior metaphysical theory. The question of what is metaphysics, among other things, was enthusiastically debated and discussed, before the time was up and participants dispersed to get dinner, catch planes, and digest the ideas brought up in the talks and discussions.

A very pleasant and inspiring workshop—many thanks to the

participants and especially the organizers.

Veli-Pekka Parkkinen
Philosophy, Kent

Calls for Papers

Social Cognition: special issue of *Perspectives: International Postgraduate Journal of Philosophy*, deadline 30 May.

Inductive Statistical Methods: special issue of *Entropy*, deadline 31 May.

Reasoning about Preferences, Uncertainty, and Vagueness: special issue of *IfCoLog Journal of Logics and their Applications*, deadline 21 June.

Critiquing Technologies of the Mind: special issue of *Phenomenology and the Cognitive Sciences*, deadline 30 June.

Logic Theorems: special issue of *Logica Universalis*, deadline 31 July.

Probabilistic Beliefs: special issue of *Theory and Decision*, deadline 1 October.

Reasoning, Argumentation, and Critical Thinking Instruction: special issue of *Topoi*, deadline 30 October.

What's Hot in . . .

Uncertain Reasoning

The search for objectivity is one of the key challenges in the foundations of uncertain reasoning. It is perhaps no exaggeration to say that one's *philosophy* of uncertainty is described by one's own tolerance to the (inevitable) element of subjectivity which intervenes in the rational quantification of uncertainty. Yet, we all like it when rational (scientific) reasoning and decision-making lead to little or no disagreement. The idea, naively, is that at least for some questions a unique and "true" answer exists. At least in those cases disagreement among individual evaluations reveals either an imperfect assessment or a fundamental, and as such ineliminable, uncertainty. P. Walley, (1991: *Statistical Reasoning with Imprecise Probabilities*. Wiley) brought to the attention of statisticians and philosophers the need to put this question on solid foundations, and in fact insisted that coping with disagreement among experts was one of the key motivations for the development of the theory of imprecise probability.

The ensuing decades witnessed substantial formal development of the theory, for which the interested reader is referred to the SIPTA website. For rather obvious reasons though, this formal development hasn't been matched by equally solid experimental data about how individual experts rationally disagree in their judgments. It is therefore unsurprising that the results of a NIPS experiment circulated recently on the SIPTA mailing list.

The forecasting experiment was centred around the following question:

10% of the papers submitted for the 2014 Neural Information Processing Systems Conference were duplicated and reviewed by two independent groups of reviewers. What percentage of the papers reviewed will yield inconsistent decisions?

By "inconsistent decision" the experimenters (Corinna Cortes and Neil Lawrence) meant a decision of the form "accept/reject" or "reject/accept".

Among the many interesting aspects of this experiment, let me mention the fact it is a "scaled continuous" question: rather than forecasting the outcome of a well-defined event, the uncertainty to be quantified is relative to the actual mean value that the random variable will eventually take, in some unit. Moreover, given what acceptance to top conferences means for the career prospects of the researchers involved, and given that we would all like our scientific output to be evaluated *objectively*, this experiment has also extremely interesting sociological fall-outs.

I think I should not end this column by reporting the percentage of disagreement among the NIPS referees and give the interested reader a chance to make their own forecast.

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

Evidence-Based Medicine

Evidence Live is a "global community coming together for a conference and evidence-based courses where intentional thinkers share ideas to improve healthcare". It is a partnership between the University of Oxford's Centre for Evidence-Based Medicine and the BMJ. This year's conference was held on 13–14 April at the University of Oxford. There was a great line-up of speakers.

The speakers were invited to consider "dangerous ideas" that might help improve evidence-based health care by closing the gap between evidence and clinical practice. Some of the suggested dangerous ideas are listed in this blog post. There is also a YouTube channel with videos of some of the speakers presenting their dangerous ideas.

Readers may be interested in looking at these videos. For instance, I found interesting Sharon Mickan's idea that there is a need for methods to assess the quality of all the evidence that factors in the decision-making process of evidence-based medicine. Mickan argues that at present there are ways to determine the quality of only some of the relevant evidence. In particular, she points out the need to identify the quality of evidence that results from clinician experience. That short video is available here.

Michael Wilde
Philosophy, Kent

Events

May

FWPS: Mangoletsi Lectures: Freedom of the Will and the Perils of Scientism, University of Leeds, 5–13 May.

E & A: Explanation and Abduction: Logico-Philosophical Perspective, Ghent University, 7–8 May.

EoM: Epistemology of Metaphysics Workshop, University of Helsinki, 8–9 May.

DT: Decision Theory Workshop, University of Cambridge, 13–19 May.

OSM: Masterclass: Objectivity, Space and Mind, London, 14–15 May.

SLACRR: St. Louis Annual Conference on Reasons and Rationality, Moonrise Hotel / Washington University in St. Louis, MO, 17–19 May.

TIoP: The Idea of Pragmatism, University of Sheffield, 18–19 May.

TAMC: Theory and Applications of Models of Computation, School of Computing, National University of Singapore, 18–20 May.

FEW: Formal Epistemology Workshop, Washington University in St. Louis, 20–22 May.

MR: Metacognition and Reasoning, Dubrovnik, Croatia.21–23 May

TRUTH AND GROUNDS: Mount Truth, Ascona, Switzerland, 24–29 May.

CD: Compromise and Disagreement, University of Copenhagen.27–29 May

TOFB: The Odds for Bayesianism, University of Vienna, 28-30 May.

JUNE

ICCS: International Conference on Computational Science, Reykjavik, Iceland, 1–3 June.

PR& MR: Practical Reasoning and Motor Representation, University of Warwick, 1–3 June.

TTL: 4th International Congress on Tools for Teaching Logic, Rennes, France, 1–4 June.

P& E: Philosophy and Economics, Uppsala, June 8–10.

ECA: Argumentation and Reasoned Action, Lisbon, Portugal, 9–12 June.

HPTL: Hilberts Epsilon and Tau in Logic: Informatics and Linguistics, University of Montpellier, 10 June.

OIS: Objectivity in Science, Tilburg University, 10–12 June.

PLS: 10th Panhellenic Logic Symposium, Samos, Greece, 11–15 June.

SEL: Studying Evidence in the Law: Formal, Computational and Philosophical Methods, University of San Diego, 12 June.

TSC: Towards a Science of Consciousness, Helsinki, 9–13 June.

GIB: Ground in Biology, Geneva, 19–20 June.

RE: Religious Epistemology, Heythrop College, 19–20 June.

MR: Meaning& Reference, University of Bucharest, 19–21 June.
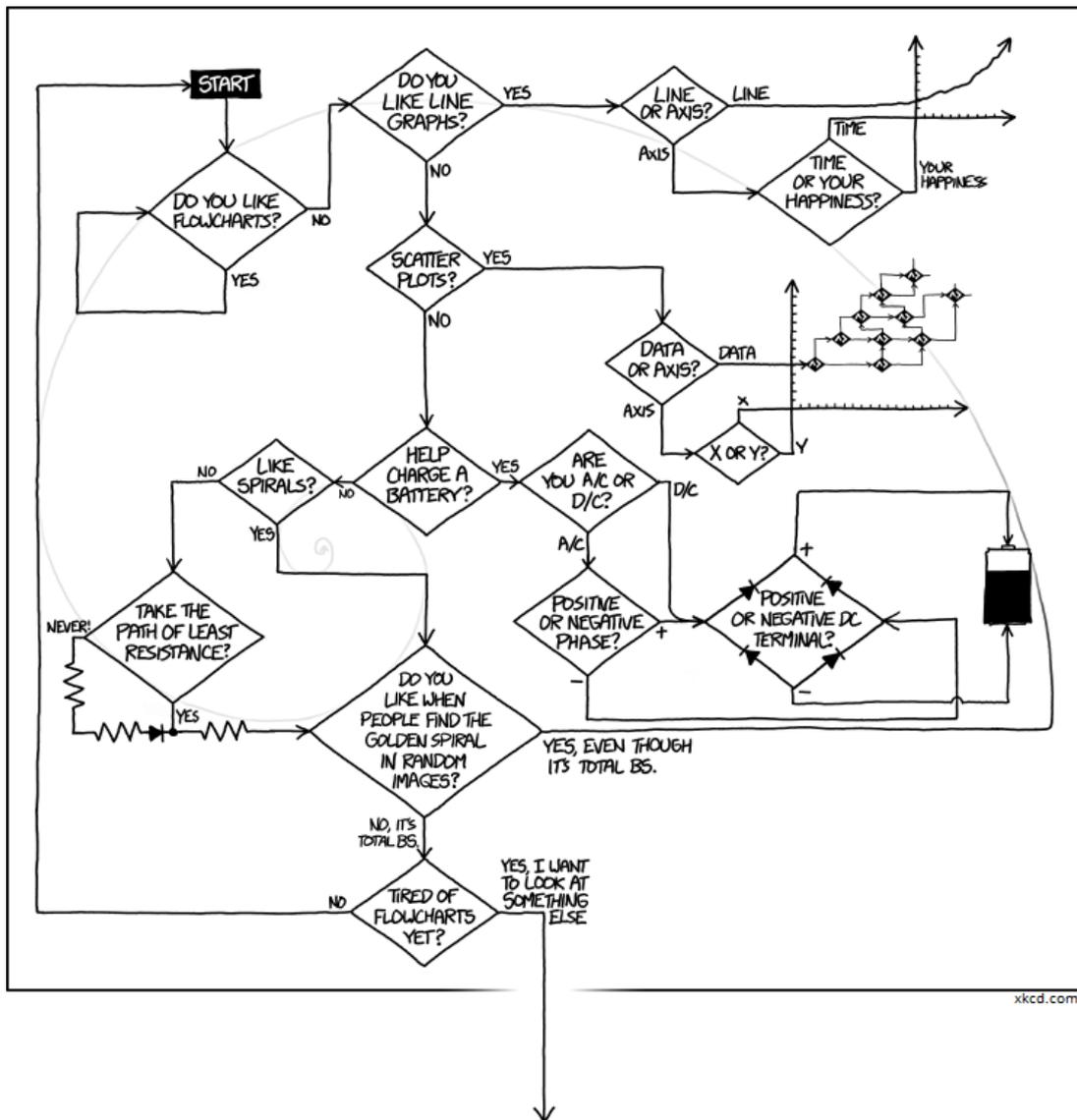
PoM: Philosophy of Mathematics Conference, University of Oxford, 22 June.

UNILOG: 5th World Conference on Universal Logic, Istanbul, 25–30 June.

LA: Legal Argumentation, Rotterdam, 26 June.

CaMitS: Causality and Modeling in the Sciences, Madrid, 29 June–1 July.

July

AAoL: Conference of the Australasian Association of Logic, Sydney, 2–3 July.

ICoML: International Conference on Machine Learning, Lille, France, 6–11 July.

AiCI: Advances in Causal Inference, Amsterdam, 16 July.

BMAW: Bayesian Modelling Applications Workshop, Amsterdam, 16 July.

CoNR: Conference on Computing Natural Reason, Indiana University, Bloomington, 19–20 July.

ISIPTA: Society for Imprecise Probability, Pescara, Italy, 20–24 July.

WLAI: Weighted Logics for Artificial Intelligence, Buenos Aires, 25–27 July.

Courses and Programmes

Courses

Combining Probability and Logic: University of Kent, 20–21 April.

EPICENTER: Spring Course in Epistemic Game Theory, Maastricht University, 8–19 June.

EPICENTER: Mini-course on Games with Unawareness, Maastricht University, 22–23 June.

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.

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 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

Research Fellow: in Health Sciences/Data Analyst, deadline 12 May.

Lecturer: in Epistemology/Metaphysics/Ethics, University of Cambridge, deadline 14 May.

Chair: in Philosophy, University of Birmingham, deadline 17 May.

TEACHING FELLOW: in Statistics, University College London, deadline 25 May.

TENURE-TRACK POSITION: in Statistical Machine Learning, Norwegian University of Science and Technology, deadline 25 May.

PROFESSORSHIP: in Metaphysical Philosophy, University of Oxford, deadline 26 May.

TEACHING FELLOW: in Formal Ethics, Case Western Reserve University, deadline 26 May.

POST-DOC: in Formal Methods of Knowledge Representation, University of Copenhagen, deadline 27 May.

ASSOCIATE PROFESSOR: in Probability Theory, University of Copenhagen, deadline 25 September.
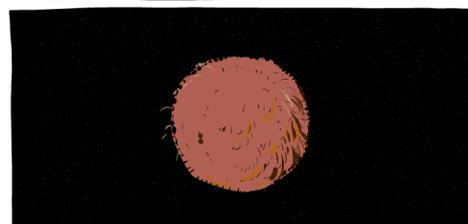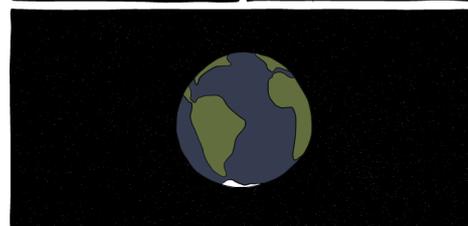
Studentships

PHD POSITION: in Modes and Contents, University of Fribourg, deadline 2 May.

PHD POSITION: in The Emergence of Relativism, University of Vienna, deadline 11 May.

PHD POSTION: in Statistical Modeling, University of Warwick, deadline 1 June.

PHD POSITION: in Cognitive Irrationality, University of Basel, deadline 30 June.