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those interested in solutions to the quantum mechanical measurement problem.

More recently, Wallace has been working on the foundations of statistical mechanics and the nature of gauge theories; as he says in the interview below, his work on quantum mechanics proves crucial here. Wallace is fond of pointing out that Everett is the only true realist interpretation of quantum mechanics; other so-called interpretations actually involve modifying the physics and therefore change the foundations of other parts of physics as well. The many-worlds interpretation, surprising as it is, may actually be just what we need to get on with the business of doing both physics, and the philosophy of physics.



ELEANOR KNOX

Philosophy, King's College London

## EDITORIAL

Hugh Everett's 'many-worlds' interpretation of quantum mechanics was first proposed 57 years ago, and has inspired science fiction ever since. Yet only recently has it gained intellectual respectability in both physics and philosophy circles. At the heart of the Everettian renaissance is [David Wallace](#), Professor in Philosophy of Physics at Balliol College Oxford. Wallace's book *The Emergent Multiverse: Quantum Theory According to the Everett Interpretation* (which was a joint winner of the 2013 Lakatos prize) is the most comprehensive defense of the Everett interpretation to date. As one might expect, or at least hope, it contains thorough responses to traditional objections to Everett along with a clear account of how the physics of decoherence renders this contemporary version of Everett much more plausible than previous versions. But along the way it covers topics that may be dear to even non-physics oriented *Reasoner*-readers hearts; most notable is the extensive discussion of probability, which I find myself recommending to students interested in the nature of probability in general, not just

## FEATURES

### Interview with David Wallace

Eleanor Knox: David, thanks for doing this interview. You're one of several examples in philosophy of physics of a physicist who became a philosopher. Can you tell us a little bit about what got a doctoral student in physics interested in philosophy? Was it important that you were in an environment (Oxford) where philosophy of physics was part of the culture, or was it just obvious to you from the get-go that foundational problems in physics needed addressing?

David Wallace: I did a physics degree in the mid-90s, and I was probably part of the first generation in physics for whom

the fact that there were philosophical problems in quantum mechanics wasn't a state secret. Tutors were likely to be relatively open about the fact that there were some quite confusing problems here. Also, I'd read popular physics before coming up to Oxford and so I was aware that there was a quantum measurement problem. What I wasn't aware of is that there were conceptual problems in physics more broadly—a real case of this is statistical mechanics. That's a case where it's *still* a state secret that there are conceptual and foundational problems, and it's still the official party line that there *were* problems, but they were solved by Gibbs and Boltzmann back in the 1900s and we're all fine now. For that kind of case, it was genuinely important to be in an environment where there were people doing philosophy of physics. If I hadn't had people to talk to, I probably wouldn't have picked on a lot of those issues.



EK: And when you went into your physics doctorate, were you aware that it was going to become as foundational as it eventually did? Was that the plan?

DW: No, not at all. I had a very open-ended PhD place and probably had more room for manoeuvre than was actually good for me. I ended up spending a lot of my time talking to philosophers of physics and thinking about conceptual problems. It's almost certainly the case that if I hadn't been in exactly that intellectual environment I would have done something more mainstream—quantum information or quantum field theory or something and I would be somewhere very different now.

EK: You ended up, after your physics DPhil, doing the Oxford philosophy BPhil and moving over to philosophy. What made you do that rather than trying to find one of the few places where you might have done foundational work in a physics department?

DW: I think the word 'few' is doing a lot of work in that sentence! I don't want to make this sound too pragmatic; of course there were substantial intellectual advantages in the move I made but there would have been intellectual advantages otherwise. To a large extent, my decision reflects the economic structure of physics and foundations of physics, at least in Britain and America. A case in point: the Perimeter Institute was advertising at the time, and if I'd gone there, I might have had a three year or a five year position in foundations of physics. But where I would have gone then is not at all clear. There are plenty of extremely talented physicists working in foundations who've faced exactly that problem; they've found it possible to do a PhD and get a post-doc or series of post-docs in foundations but have found it enormously difficult to find permanent positions anywhere, let alone anywhere they particularly want to be. If you look at people who've made functional careers in foundations, there are roughly three groups: people in philosophy of physics, people in quantum information who've finessed it into foundations, and people who've made their bones in some other more mainstream area, and have moved on to foundations after they've got tenure.

EK: Absolutely—one of my students was recently told by their theoretical physics tutor that foundational questions were

'questions for old men'! But moving on to your work—a great deal of your work has involved making the Everett interpretation a worked-out, plausible and intellectually respectable interpretation of quantum mechanics. I get the impression you've both benefited and suffered from focussing much of your work on such a controversial topic; have the benefits outweighed the disadvantages?

DW: It's hard to tell, because the work I've done on the Everett interpretation is such a necessary intellectual foundation for everything else I've done in the philosophy of physics that it's hard to think about having done it the other way round. The quantum measurement problem is such a gaping hole in contemporary physics that without some idea of what's supposed to fill it, I don't know how to ground most of the other things I'm interested in. Most of the other things I've done have the Everett interpretation tacitly or explicitly in the background. Stuff on quantum field theory relies on that fairly explicitly. A lot of what I've done in statistical mechanics presupposes unitary quantum mechanics (i.e., Everett) in the background. Even stuff on classical field theories interests me largely as low-energy limits of quantum field theories. One way to characterise the work I've done on Everett, which is really Simon Saunders' characterisation, is that it's a defence of a radical conservatism about contemporary physics. It's basically a license to get on with analysing the rest of contemporary physics as we find it.

EK: One of the ways that I was thinking that the controversial nature of the Everett interpretation had helped your work was noticeable in the book. One thing that struck me when I read it was that it's much more a work of systematic philosophy than some other philosophy of physics books. You mention frequently in your book that some of the problems you're dealing with are old philosophical problems thrown into stark relief by an unfamiliar context. But one of the things that's nice about the book is that you're forced to articulate what you think about scientific realism, probability, philosophy of mind and higher-level ontology. And that makes the book a more interesting read because it's extremely satisfying to read the views of someone who's been forced to work out their world-picture in that kind of way.

DW: You're right that you're forced towards that in defending Everett because a large number of people come to Everett thinking "well of course it's wrong, so the problem is to work out what's wrong with it". That means you have to defend against every avenue of possible attack. I find that in defending Everett I'm cut enormously less slack than in defending a view on symmetry, or thermodynamics or something.

EK: One of the nice things about the book is that you do a lot of substantive interesting work on probability that actually stands its ground quite independent of the Everett interpretation. There's a strong functionalist streak both there, and in your work on higher-level ontology. Is that something that you realised was necessary in order to defend Everett or something you were independently philosophically inclined towards?

DW: It's independent I think. A three word summary of my views on functionalism in philosophy of mind would be: "what Dennett says". I was very thoroughly persuaded by Dennett's conception of the nature of higher-level ontology in science; his "Real Patterns" is up there as one of the most influential papers feeding into the book. It's very difficult to take the practicalities of contemporary physics seriously without being driven to something very structuralist, very functionalist, even inde-

pendently of Everett and quantum mechanics. Philosophers of physics don't always see that because, while they know a lot of physics, the sort of physics they know is often a purified, axiomatic, foundational physics that's quite remote from application. The sort of messy complexity of contemporary physics, and the way it's layered together, and the promiscuity with which physicists identify different mathematical structures, and the casualness with which they move from one framework to a structurally equivalent one made it very difficult for me to see that there was any other way apart from the "Real Patterns" way of thinking about it.

EK: There is one philosophical assumption in your book that you don't put a lot of work into justifying, and that's a fairly strong scientific realism (albeit qualified by your commitment to a form of structural realism). I'm sympathetic to that, because I think some kind of realism is often a pre-requisite if one is to get interested in interpretational questions. Is that how you see it?

DW: There's certainly some of that. To borrow an example I've used elsewhere, if you thought that the point of building the large hadron collider was to clarify what devices like the large hadron collider did when you switched them on, you probably wouldn't bother building them. It's virtually impossible to make sense of scientific practice unless you attribute to scientists some level of scientific realism. Of course, that's not in itself an argument for scientific realism. I don't discuss those arguments in the book in part because I've got nothing original to say; those arguments have been well made elsewhere. In general, the case for at least a nuanced form of scientific realism is so good, that it wasn't something I wanted to dwell on. But 'nuanced' is doing some work there; I actually think that you can read my book with something like a qualified constructive empiricism and most of it goes across *mutatis mutandis*. You can't combine it with serious instrumentalism and operationalism, particularly at the semantic level; you can't think that the theory is failing at its representational role. But I don't talk about that much because that position is to a large extent defunct within philosophy of science. That said, one of the things that's become more apparent to me in the last few years is the extent to which that position is not defunct within some corners of physics, particularly in quantum information.

EK: Isn't that position just obviously there lurking in the background of the 'shut up and calculate approach'?

DW: I think it's not actually. The right way to think about the 'shut up and calculate' approach is as a sort of quietism. It really means what it says. You only have to do two things to adhere to the shut up and calculate approach. You have to calculate, and you have to shut up. If you are not shutting up, if you layer a warmed-up instrumentalism over it, you're no longer adhering to the approach. And I think there are lots of smart and informed people in quantum information theory who aren't doing shut up and calculate, and who are really pushing some kind of instrumentalism. And I think when the dust settles, those strategies don't escape the criticisms of operationalism and instrumentalism made back in the day.

EK: Let's discuss your current work. Having produced the Everett book, you've moved on to other work, particularly in statistical mechanics and the foundations of gauge theories. You mentioned earlier that it's your Everett work that you feel gives you a license to work on these issues without running into the measurement problem, but aside from that, how do you see the work as connecting up? Or are they separate projects?

DW: To a significant extent all of these things have to link up, because despite the impression you might get in the literature, everything's quantum. At a more specific level, I've been thinking about these things as rather disjoint projects. Sometimes they connect more than you might expect. The statistical mechanics material turns out to be much more intertwined with quantum mechanics than I'd originally thought. I now think that the right way to understand statistical mechanics is very bound up with the right way to think about quantum mechanics.

EK: Yes. This is the moral of some of your recent work which says that the chances in statistical mechanics are really quantum chances.

DW: Yes, and that the probability distributions in statistical mechanics are really the classical limits of the states of individual quantum systems. Those statements are sort of the same thing, but they look different. Philosophy of statistical mechanics has been advanced almost entirely classically. If you read a textbook or research monograph in the field, it will normally start with some little apologia to the effect that we should really be doing these things in terms of quantum mechanics, but that we'll do things in terms of classical mechanics because it's mathematically easier, and the conceptual problems are basically the same. That statement's more or less exactly wrong. It's actually simpler for foundational purposes to do quantum statistical mechanics; the mathematics is more under control. And I also think conceptually many things are very different in quantum statistical mechanics. The real tacit reason why an awful lot of the work doesn't engage with quantum statistical mechanics is failure to have solved the measurement problem. That leads us back to reason why, autobiographically, I needed to understand Everett before I felt I understood anything else.

### 'That's Deflatable'—Sets vs. Truth

In a recent feature, "Deflationism About Sets" (*The Reasoner* 8(5):50–51), Matthew Clemens defends an analogy—originally due to Luca Incurvati—between deflationism about truth and deflationism about sets. Clemens characterizes the analogy as follows:

Deflationism about sets is the view that there is no substantial metaphysical nature to sets in roughly the same way that deflationism about truth is the view that there is no substantial metaphysical nature to truth.

Clemens' primary aim is to show how the deflationary strategy can be extended to more than one conception of sets. He convincingly argues that both the iterative conception of sets and the graph conception of sets are amenable to such treatment. This generalization, however, highlights an important source of *disanalogy*. I will argue that Clemens-style deflationism does not accomplish for sets what traditional deflationism does for truth.

To appreciate this, we need to look more closely at the project originally described by Incurvati (2012: "How to be a minimalist about sets", *Philosophical Studies* 159:69–87). The first part of this project is a deflationary account of *iterative sets*. On the iterative conception, the set-theoretic universe is the cumulative hierarchy. Deflationism about iterative sets is the view that the full content of the iterative conception of sets lies in the structural features of iterative set formation, which

we can grasp independently of any metaphysical insight. This is deflationary because it runs counter to the prominent view that iterative set formation has substantive metaphysical content. Because iterative set formation is open-ended one might suspect that it carries an implicit commitment to the claim that sets *asymmetrically depend* for their existence on their elements. Incurvati contends otherwise. Just as we fully grasp the number series when we grasp how the ‘successor’ operation works, we fully grasp the cumulative hierarchy when we grasp how the iterative ‘set of’ operation works. Just as we can master succession without coming to think that the existence of later numbers is grounded in the existence of earlier numbers, we can master iterative set formation without coming to think that the existence of a set is grounded in the existence of its elements. As Incurvati puts it: “in order to grasp the cumulative hierarchy picture of the set-theoretic universe we need not think of sets as metaphysically dependent upon their members” (Incurvati 2012: p. 83)

Clemens offers a similar account of *graph sets*. On the graph conception, the set-theoretic universe is the collection of accessible, pointed, directed graphs (APGs). Incurvati helpfully elaborates that a graph set is simply “an object having a (hereditary) *membership structure*” (2014: “The Graph Conception of Set”, *Journal of Philosophical Logic* 43, p. 191). Such an object is depicted by an arbitrary APG. Deflationism about graph sets then follows the pattern above: just as we fully grasp the number series and the cumulative hierarchy when we master their constitutive definitions, we fully grasp the graph sets when we master the definition of an APG. Clemens characterizes his achievement as follows. The deflationist about iterative sets claims that she can fully explain *what it is to be* an iterative set by pointing out . . .

. . . the objects that arise via repeated applications of the powerset and generalized union operations begun on the empty set. What is it to be a set? Thanks to our formal picture we can answer—just to be one of *those things*.

Similarly, the deflationist about graph sets claims that she can fully explain *what it is to be* a graph set by pointing out the following:

The collection of the APGs plays the role, for the graph conception, that the cumulative hierarchy plays for the iterative conception . . . What is it to be a set? Once we’ve got the general definition of an APG, the deflationist about sets on the graph conception can answer—just to be one of *those things*.

That is, one can fully articulate the content of either conception demonstratively and—most importantly—without appeal to any substantive metaphysical theses. This is all quite plausible, but also problematic for the analogy between truth and sets.

Whereas deflationism about truth is an account of *truth*, Clemens-style deflationism about sets is a family of accounts of *conceptions of sets*. This entails that deflationists about sets can disagree amongst themselves about which conception is correct. Deflationists about truth, on the other hand, agree that the concept of truth is uniquely specified by its disquotational role. This difference exists because there is a gap between any given, precise conception of sets and the ordinary concept of a

set or collection. Incurvati is well aware of this gap: the second part of his project is an argument meant to indirectly justify the belief that the iterative conception of sets is correct, despite the fact that it has no claim to determinately capture the ordinary concept. This difference, however, weakens the analogy between sets and truth.

Some deflationists about truth, e.g., Horwich (1990: *Truth*, Oxford University Press), claim that anyone who possesses the concept of truth is in a position to know all that there is to know about truth. This intimate link is a source of skepticism about whether there is a property of truth ‘out there’ at all. Leaving aside whether the argument succeeds, what’s pertinent is that it presupposes a unique conception of truth. The deflationist about sets cannot advance such an argument because she recognizes a multiplicity of conceptions of sets.

More importantly, the Clemens-style deflationist about sets does not *want* to advance this or any similar argument. Here we encounter the most profound difference between the two projects. The deflationist about truth is a kind of eliminativist: the property of truth is not part of her ontological picture of reality. The deflationist about sets has no such view and, seemingly, no aspiration to deny the existence of sets. This observation in no way undermines deflationism about sets, it is only meant to clarify what the project is really about. But this clarity also reveals why Clemens’ characterization is misleading: deflationism about sets does not accomplish for sets what traditional deflationism does for truth.

COLIN CARET

Philosophy, Yonsei University

## NEWS

### Robo-Philosophy, 20–23 August

This conference was the first large-scale academic event on philosophical aspects of social robotics. The aim of the event was bringing together the key researchers in this new field and to show that the issues of social robotics address all systematic areas of philosophy. The conference featured two keynotes, seven plenaries, 35 session talks and two panels; attracting approx. 120 conference participants.

After a short welcome by Johanna Seibt (Aarhus), the first plenary John Sullins (Sonoma State) gave a talk on ‘Machine Morality Operationalized’. Besides elaborating on the proper role of philosophy with respect to the development of novel technologies. He argued that some level of artificial ethical agency is possible and discussed suitable applications for robotics. This was followed by 12 parallel talks with session topics ranging from the ontology of simulation to sociality, normativity, etc. The second plenary was Luciano Floridi (Oxford) on ‘Smart, Autonomous, and Social Agents’ where he discussed human exceptionalism. Floridi argued that robotics and AI confront us with technologies that might outperform humans on various levels. Nevertheless, humans might be unique when it comes to being ‘successfully dysfunctional’.

The second day was opened by the plenaries Peter Kahn (Washington) on ‘Social and Moral Relationships with Robots’ and Wendell Wallach (Yale) on ‘Machine Morality and Human Ethics’. Kahn presented empirical studies investigating whether humans can form intimate relationships with robots i.e., hold them accountable and enhance human creativity. Wal-



lach focused on how we differ from machines and challenged the applicability of ethical theories to robotics. Hereafter followed 13 parallel sessions on a variety of topics, e.g., cultural political issues, moral agency, etc. Mark Bickhard (Lehigh) headed a focal session on embodied and social agency; Charles Ess (Oslo) directed a session on communication-theoretic issues.

On the third day Kerstin Dautenhahn (Hertfordshire) and David Gunkel (Northern Illinois) gave plenaries prior to 11 parallel sessions and a focal session on military robots organized by Ezio di Nucci (Universität Duisburg-Essen). Dautenhahn presented challenges with respect to the use of companion robots and Gunkel argued why we instead of asking ‘can machines have rights?’ should ask ‘should machines have rights?’ The day concluded with a festive keynote by Hiroshi Ishiguro (Osaka) on ‘Android Philosophy’. Ishiguro explained how he investigates concepts such as mind, and consciousness by building androids.

The final day opened with the plenary by Mark Coeckelbergh (De Montford) on ‘The Automation of the Social?’ He presented his arguments for why social robots should be included in our social ontologies, and demonstrated that sociality cannot be ‘fully understood, controlled and automated’. The final highlight of the conference was the keynote by Illah Nourbakhsh (Carnegie Mellon). He outlined some of the challenges robotics is confronted with, such as the massive broadening of the social gap. Nourbakhsh stressed the importance of proper education of next generations with respect to understanding novel technologies.

The conference was organized by Johanna Seibt, Raul Hakli, and Marco Nørskov from the Department of Culture and Society (Aarhus) and funded by the VELUX Foundation and Aarhus University.

MARCO NØRSKOV  
Philosophy,

Aarhus University & Hiroshi Ishiguro Laboratories

## The Social Mind: Origins of Collective Reasoning, 29–30 August

The Centre for the Study of Mind in Nature (CSMN) at the University of Oslo hosted a workshop, “The Social Mind: Origins of Collective Reasoning,” on August 29–30. The aim of this interdisciplinary workshop was to bring together philosophers interested in team reasoning with psychologists interested in the development of cognitive and conceptual capacities thought essential to team reasoning, such as group identification, social perspective taking, and the sense of self and other.

Workshop organizers Sebastian Watzl (CSMN), Jola Feix (CSMN), and Katharine Browne (Dalhousie/CSMN) opened the workshop with an introduction to team reasoning and relevant research in developmental psychology, intended to serve as a common ground to help facilitate cross-disciplinary discussion.

The workshop was comprised of two sessions a day, each consisting of one talk by a philosopher and one talk by a psychologist. In the first session, Natalie Gold (King’s College London) explored the (minimal, she argued) commitments about mental capacities that team reasoning makes, and pointed to the need for a team goal or preference. Sebastian Grueneisen (Max Planck Institute for Evolutionary Anthropology, Leipzig)

presented results indicating children’s capacities to coordinate their actions with others by converging on a salient solution, conforming to a majority, and adjusting their decision in response to second-order false beliefs.

In the second session, Katharine Browne (Dalhousie/CSMN) argued that team reasoning was able to provide prescriptions for action to team members, but that no account could be given of why individuals should become team members. This, she argued, casts doubt on the utility of team reasoning as a normative theory. Philippe Rochat (Emory University) traced the development of human conceptions of ‘I,’ ‘we,’ and ‘they,’ beginning at birth with self-feeling awareness, to emerging co-awareness, and representational co-consciousness.

Henrike Moll (University of Southern California) opened the first session on day two by exploring young children’s mutualistic understanding of seeing persons, according to which they can ‘look at’ but cannot ‘see’ another agent whose eyes are covered. Raimo Tuomela (University of Helsinki) gave an account of team reasoning in what he called the “we-mode.” He argued that reasoning in the we-mode provides more accurate predictions about behavior in social dilemma situations, and generates better results for team members.

In the second session, Raul Hakli (Aarhus University) explored how understanding team reasoning either *as a precursor to* or *effect of* collective intentionality has consequences for the interpretation of group preferences and the role of team reasoning in the creation of the social world. Malinda Carpenter (University of St. Andrews and Max Planck Institute for Evolutionary Anthropology) gave an overview of her extensive research on the development of various ways of becoming a ‘we’ in children. She argued that her findings suggest that the origins of collective reasoning have deep roots in development, long before any real reasoning is involved.

The workshop drew attendees both locally and from abroad, from philosophy and the social sciences.

KATHARINE BROWNE  
CSMN, Dalhousie University

## London Philosophy of Science Workshop, 2–3 September

The First London Philosophy of Science Graduate Workshop took place 2–3rd September at University College London under the theme “Approaches within Philosophy of Science”. The event, generously sponsored by the BPS and BSHS, was organised through the Science & Technology Studies Department by PhD students Toby Friend and Erman Sozudogru. The aim of the workshop was to bring graduates and faculty working on philosophy of science in London together with researchers from further afield to communicate, consolidate and celebrate the diversity of the burgeoning discipline.

The workshop was a great success with an attendance of around fifty people including speakers, half of whom came from outside London and over half of those from outside the UK. The discussions were various, but with notably strong themes of methodological and epistemic pluralism.

We began with our first keynote speaker Chiara Ambrosio (UCL) giving a vibrant exposition of her recent research at the Harvard Archives into Peirce’s history of science. This was followed by Chris Campbell (UCL) who related Pierce’s and Mendelev’s conceptions of natural laws, offering a pluralist

perspective.

Julian Newman (Birkbeck) began our second session with an application of epistemic considerations to computing science. We then heard from Mario Santos-Sousa (UCL) on how psychological findings can inform the epistemology of numbers. Manuela Fernández Pinto (Helsinki) then presented us with a case for pluralism after confronting the imperialism of economics in social epistemology.

Our final session of the day began with Irene Van de Beld (Twente) who argued that the data-phenomena distinction made by Woodward and Bogen should be ridden of all connotation with the realism-antirealism debate. Our second keynote speaker Stephen Mumford (Nottingham) concluded the first day with a motivating presentation of the power in philosophy of science of accepting an irreducible modality of dispositionality.

The second day commenced with our third keynote by Mauricio Suárez (Institute of Philosophy) offering lessons from intriguing discussions in aesthetics on representation in art for our understanding of representation in science. We then heard from Michał Leśniak (KUL) about how methodological pluralism can be used to bridge the gap between feminist and mainstream philosophy of science.

In the next session, Vincenzo Politi (Bristol) discussed varieties and incompatibilities of concepts of naturalism in philosophy of science. This was followed by James Nguyen (LSE) who offered a response on behalf of the virtue pluralist to arguments from social choice theory that no principled algorithm for theory-choice can be established. Anna De Bruyckere (Durham) then motivated us to consider the phenomenology of science using the inconceivability of the 2008 financial crisis among mainstream economists as a case study.

Ruth Hibbert (Kent) began the penultimate session with some advice inspired by lessons learned from the increasing specialisation in science for a workable methodological pluralism in philosophy of science. We then heard from Liam Kofi Bright (CMU) about social choice theory's application to issues concerning judgement aggregation in science.

Our final session began with a presentation from Toby Friend (UCL) comparing the compatibility of holist and particularist attitudes in philosophy of science. The final keynote speaker Hasok Chang (Cambridge) concluded the workshop with an inspirational discussion of the light pluralist attitudes bring to discussions regarding the proper approach to philosophy of science.

During the two days which followed the workshop UCL hosted a summer school comprising four sessions run by Erman Sozudogru (UCL), Jack Wright (Cambridge), Neil Barton (Birkbeck) and Harriet Lloyd (UCL) in which we discussed works by Galison, Longino, Maddy and Cartwright.

We all made good friends over the course of the week and hope to run the second graduate workshop next summer.

TOBY FRIEND

Science and Technology Studies, UCL

## Robustness Analysis, 25–26 September

A two-day workshop on robustness analysis was held at the University of Helsinki, September 25–26. The Centre of Excellence in the Philosophy of the Social Sciences organized the event as part of a research project on Models and Simulations.

The workshop brought together ten researchers who are currently working on the subject.

Robustness analysis is a method of inquiry investigated since long in the philosophy of science. The practice was first brought to the attention of philosophers by its use in biology (Levins, 1966) and econometrics (Leamer, 1983, 1985). Recently, there has been a revival of interest in the practice, motivated by the appeal to robustness in economics, biology, climate sciences, statistics and neuroscience. Via robustness analysis, scientists try to ensure the predictions of models and experiments are robust, i.e., invariant under small changes in the theoretical or experimental setup from which they are derived.

The format of the workshop was novel in that the speakers were asked to pre-circulate their working papers in advance, two weeks before the meeting. During the workshop, each speaker presented his/her work in a very brief introduction (five to ten minutes), which mainly served to launch the discussion. The invited speakers were Lorenzo Casini (Geneva), Cedric Paternotte (Munich), Jonah Schupbach (Utah), Kent Staley (St. Louis) and Jacob Stegenga (Utah). Internal speakers were Alessandra Basso, Jaakko Kuorikoski, Aki Lehtinen, Chiara Liscandra and Caterina Marchionni. The presentations were clustered around four main thematic areas, namely derivational robustness, robustness and the variety of evidence problem, measurement robustness and robustness reasoning. In what follows, a brief description of the talks will be given.

Jaakko Kuorikoski and Caterina Marchionni's presentation was on the epistemic rationale of triangulation as a form of robustness analysis. In their paper, triangulation was defined as the use of multiple and independent sources of evidence to check whether a phenomenon is an artefact of a particular method. Chiara Liscandra presented a paper on robustness analysis and mathematical tractability. In the paper, she investigated whether robustness analysis is an effective strategy to assess the impact of the mathematical framework in which a certain model is formulated. Jacob Stegenga introduced a distinction between two kinds of independence, i.e., conditional independence and ontic independence. He claimed that ontic independence is mistakenly considered to be a necessary and sufficient condition for robustness arguments to be warranted. In his paper, Aki Lehtinen argued that in the case of derivational robustness some lack of independence is necessary for robustness to be confirmatory, whereas experimental robustness is different in this respect.

Cedric Paternotte presented a review of the uses of robustness in evolutionary biology. In his paper, he argued that when the phenomena to be explained are stable under perturbations or multiply instantiated, the appeal to within-model and across-model robustness can be justified. In the measurement robustness camp, first Kent Staley's presentation was on the distinction between statistical uncertainty and systematic uncertainty. He argued that the estimation of the latter is to be understood as a form of robustness analysis. Next, Alessandra Basso argued that measurement robustness is incompatible with the miracle argument, and that it is not exposed to the same kinds of objection.

Lorenzo Casini's presentation was on robustness explanation and how it squares with available causal and non-causal accounts of explanation. In his paper, he drew on the economic literature on agent-based models of asset pricing. Finally, Jonah Schupbach's work explored a variety of types of robustness analysis from a formal epistemological perspective.

He claimed that many cases of robustness analysis follow a pattern of explanatory reasoning, and that this may even be a common feature that unifies the seeming diversity of types of robustness analysis.

The discussion was lively and the criticisms productive. The format has proved to be particularly successful. Finnish cuisine was served at a social dinner and apparently very much appreciated!

CHIARA LISCIANDRA  
Philosophy, Helsinki

## Calls for Papers

**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 *Hu-mana.Mente*, deadline 15 March 2015.

## WHAT'S HOT IN ...

### Uncertain Reasoning

Uncertain reasoning is one of those fields in which technical advances cannot be disentangled from philosophical reflection. As a particularly striking case in point, take de Finetti's 300-plus page treatise on the economics of insurance (published in 1967, in Italian). Right in the first chapter he quotes Harold Jeffreys's remark from his *Theory of Probability* to the effect that "language has been created by realists, and mostly very naive ones at that". Not exactly the kind of remark one would expect from a reference work on the quantification of uncertainty for actuarial purposes.

Within uncertain reasoning, the technical question which involves perhaps the greatest degree of philosophical sophistication concerns the choice of what are usually called *prior* probabilities. Put crudely, the issue arises when one grants that uncertainty should be quantified probabilistically even in the absence of an objective method to do so. This problem is particularly pressing for those (like de Finetti and Savage) who believe that the only normative requirement on rational degrees of belief is their coherence. Except in very specific cases, this leaves the decision-maker with a number of equally rational probability distributions and no formal criterion for selecting one for decision-making purposes. Hence the problem of choosing initial probability distributions is both practically and theoretically very challenging.

R. Kass and L. Wasserman (1996: [The Selection of Prior Distributions by Formal Rules](#), *Journal of the American Statistical Association* 91 (435) pp. 1344–1370) provides a manageable yet detailed review with an annotated bibliography on the problem. One reason I particularly enjoy this somewhat dated review is the emphasis it puts on the development of Harold Jeffreys' ideas on the matter. As Kass and Wasserman argue,



Jeffreys can be seen as the first to reason explicitly about methods for selecting priors. In his contributions to the field he held rather distinct positions culminating with the view that priors are largely a matter of convention. A view that results from his realist philosophy which lead him to write (in the second and third editions of his *Theory of Probability*) that the choice of "reference probabilities" is akin to the conventions which determine the international standards of measurement and that "in a different world the matter would be one for decision by the International Research Council". Thus the standard view which takes Jeffreys to be a logicist about rational degrees of belief (i.e., the idea that logical, objective, impersonal rules determine uniquely the degree to which a proposition is rationally believed) is shown by Kass and Wasserman to be quite inaccurate.

This in my view opens up a very interesting perspective on the role of *intersubjectivity* in the long-standing contrast between the "subjective" and the "objective" in the foundations of probability.

HYKEL HOSNI  
Marie Curie Fellow,  
CPNSS, London School of Economics

### Evidence-Based Medicine

In a [recent paper](#), Trisha Greenhalgh, Jeremy Howick, and Neal Maskrey ask whether evidence-based medicine is a movement in crisis. They suggest that EBM has become distorted. In its current form EBM is having a hard time dealing with some of the problems it set out to address. So Greenhalgh et al propose a return to *real* EBM. They argue that one advantage of real EBM is that it '[i]s characterised by expert judgment rather than mechanical rule following'. They go so far as to say that 'students should be encouraged to try intuitive reasoning in the clinic and at the bedside'.

This might seem to be a step in the wrong direction, since EBM is often praised for overturning the unhealthy reliance on intuitive reasoning that was prominent in traditional medicine. One story goes as follows. Intuitive reasoning suggested that in order to prevent sudden infant death syndrome, babies should be put to bed on their tummies rather than their backs because this will lower the chance of them choking on their vomit. But then EBM came along, and comparative studies suggested the exact opposite. Babies should be put to bed on their backs, since this significantly reduces mortality from sudden infant death syndrome.

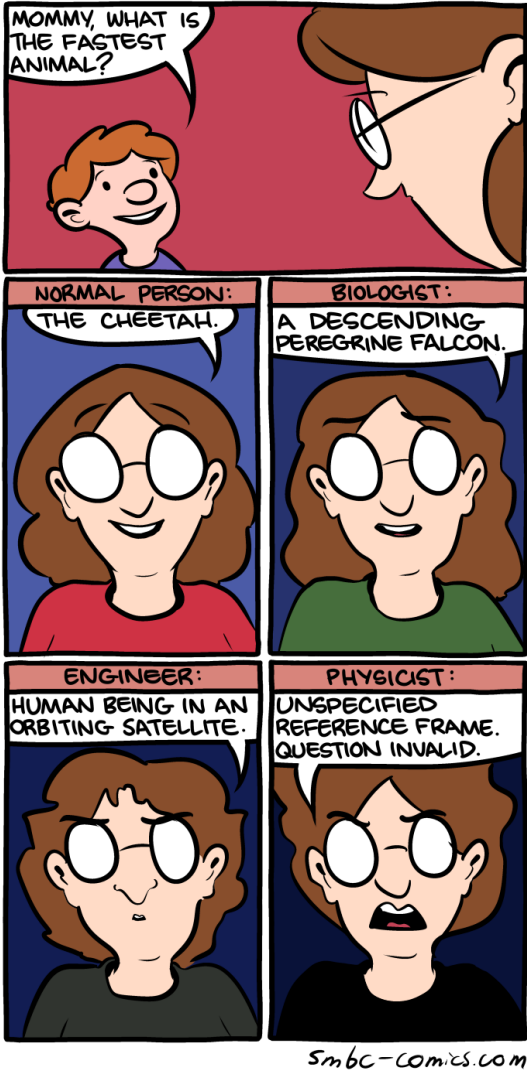
Of course, Greenhalgh et al. are not arguing that evidence from comparative studies should be ignored in favour of intuitive reasoning. They are suggesting only that informed intuitive reasoning should be allowed to play a more prominent role in clinical decision making. They accept that current EBM is great at giving rules and guidelines for clinicians to follow, but argue that clinicians can rely on these rules at the expense of other evidence, such as evidence based on intuitive reasoning. And this seems to them a distortion of evidence-based medicine: '[r]eal evidence based medicine is not bound by rules'. They recommend informed intuitive reasoning to redress this imbalance.

Their recommendation is bound to stir up some debate. But this can only be a good thing: an unexamined evidence-based medicine is hardly an evidence-based medicine at all.

Meanwhile, over at the [EBM+ blog](#), Jon Williamson has written about [some epistemological challenges for Systems Medicine](#). EBM+ are also planning a [workshop](#) titled “EBM+: Evidence of mechanisms in evidence-based medicine”. The workshop will take place at the Canterbury campus of the University of Kent, 8–9 January 2015. Please contact [Michael Wilde](#) if you would like to give a talk at this event, or even if you’d just like to attend.

MICHAEL WILDE  
Philosophy, Kent

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## EVENTS

### NOVEMBER

[ERNEST SOSA](#): Judgment and Knowledge as Forms of Action, University of Muenster, 3 November.  
[ECSI](#): European Conference on Social Intelligence, Barcelona, Spain, 3–5 November.  
[OBLLM](#): Object and Property in Logic, Language, and Metaphysics, University of Birmingham, 5 November.  
[PoCE](#): Phenomenology of Cognitive Experiences, University College Dublin, 5–7 November.  
[EPISTEMIC REASONS](#): University of Sherbrooke, Canada, 7–8 November.  
[GROUNDED COGNITION](#): Düsseldorf, 7–8 November.  
[ACGC](#): 8th Arché Graduate Conference, University of St Andrews, 8–9 November.  
[BOTB](#): Bayes on the Beach, Queensland, Australia, 10–12 November.  
[LORENTZ](#): Logics for Social Behaviour, Leiden, 10–14 November.  
[SoPhiSci](#): Social Philosophy of Science, Moscow, Russia, 18–19 November.  
[MENTAL CAUSATION](#): University of Leuven, 20–21 November.  
[EPISTEMIC CONSEQUENTIALISM](#): London School of Economics, 21 November.  
[COMPUTERS AND MINDS](#): University of Edinburgh, 21 November.

[ARE& W](#): Analogical Reasoning East and West, Heidelberg, 24–25 November.  
[AIC](#): 2nd International Workshop on Artificial Intelligence and Cognition, Turin, Italy, 26–27 November.  
[SKEPTICISM](#): Bonn, 26–28 November.  
[E& PoL](#): Epistemology and the Philosophy of Logic Workshop, University of Graz, 28 November.  
[AAL](#): Australasian Association for Logic Annual Meeting, University of Canterbury, Christchurch, New Zealand, 29–30 November.

### DECEMBER

[NZAP](#): University of Canterbury, New Zealand, 1–5 December.  
[FREGE](#): University of Bergen, Norway, 5–6 December.  
[FE & RE](#): Formal Epistemology and Religious Epistemology, Oxford University, 8–9 December.  
[ASCS](#): Australasian Society for Cognitive Science, Monash University, 8–10 December.  
[CMNA](#): Computational Models and Natural Argument, Krakow, Poland, 10 December.  
[LPMP](#): Logic and Philosophy of Mathematical Practices, Brussels, 11–12 December.



**ABM:** Agent-Based Modeling in Philosophy, LMU Munich, 11–13 December.

**SERP:** Workshop on Statistical Evidence in Epistemology and the Law, University of Glasgow, 12–13 December.

## JANUARY

**ICLA:** 6th Indian Conference on Logic and Its Applications, Bombay, 5–8 January.

**DATA:** Workshop on the Theory of Big Data Science, University College London, 7–9 January.

**ICAART:** 7th International Conference on Agents and Artificial Intelligence, Lisbon, Portugal, 10–12 January.

**SoTFoM:** Competing Foundations, London, 12–13 January.

**WHAT IS EXPERTISE?:** Münster, Germany, 12–13 January.

**SAPS:** 4th South African Philosophy of Science Colloquium, Pretoria, 15–16 January.

**CGCPML:** 8h Annual Cambridge Graduate Conference on the Philosophy of Mathematics and Logic, St John's College, Cambridge, 17–18 January.

**DIAGRAMS:** 1st Indian Winter School on Diagrams, Jadavpur University, Kolkata, 27–31 January.

**SDSS:** Scientific Discovery in the Social Sciences, London School of Economics, 30–31 January.

## COURSES AND PROGRAMMES

### Courses

**AAAI:** Texas, USA, 25–29 January.

**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.

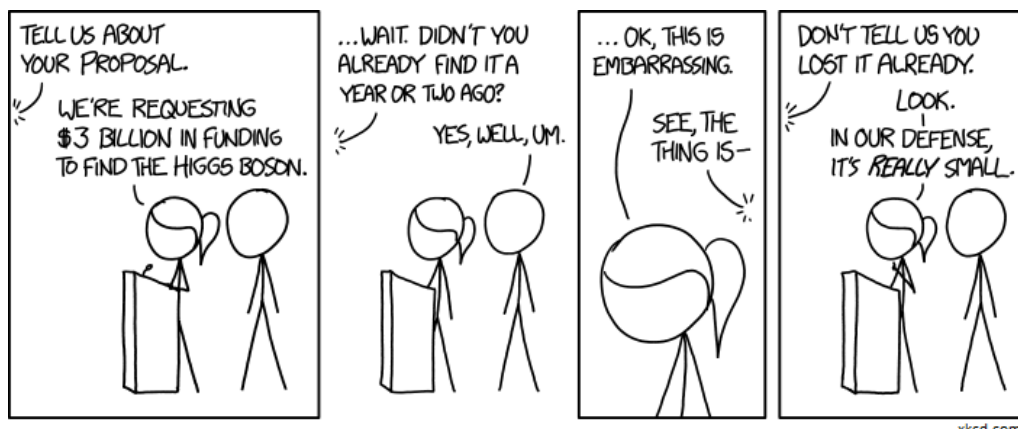
**MA PROGRAMMES:** in Philosophy of Language and Linguistics, and Philosophy of Mind and Psychology, University of Birmingham.

**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

**ASSISTANT PROFESSOR:** in Philosophy of Mind, University of Toronto, deadline 13 November.

**POSTDOCTORAL FELLOW:** Tilburg Center for Logic, General Ethics, and Philosophy of Science, deadline 20 December.

### Studentships

**PHD POSITION:** in epistemology and philosophy of science, University of Kent, until filled.

**PHD POSITIONS:** in “Scientific Realism and the Quantum”, Philosophy, Leeds, until filled.

**PHD POSITION:** in Spatial Cognition and Reasoning, Psychology, Giessen, until filled.

**PHD POSITION:** Philosophy of Cognitive Science, University of Cologne, deadline 1 November.

**PHD POSITION:** on the project “Recognizing Trust in Natural Language,” Computer Science, Philosophy and Linguistics, University of Dundee, deadline 30 November.