THE REASONER

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EDITORIAL

I am particularly grateful to the editors of *The Reasoner* for their invitation to present the December 2011 issue as it gave me the opportunity to interview Stephen Read, Professor of History and Philosophy of Logic at the University of St Andrews. Steve and I first met eighteen years ago when I arrived at St Andrews to study for an M.Litt. at the Department of Logic and Metaphysics (as it then was), where he has taught since 1972. He went on to supervise both my Masters and Doctoral research, and our collaboration eventually resulted in a coauthored survey, 'The philosophy of alternative logics', which was published in Leila Haaparanta's *The Development of Modern Logic* (Oxford University Press, 2009). This work looked at the emergence of nonclassical logics from the perspective of philosophy of science. Steve was invaluable as a supervisor, not least because he had made his own significant contributions to relevance logic, one of the four logics I used as case studies. His role in relevance (or relevant) logic, and how it has developed since, is one of the topics we discuss in the interview.

However, Stephen Read's work in reasoning has been far more wide-ranging. In particular, he has worked throughout his career in the history of logic, primar-

ily the logic of the middle ages. His work in this field was a revelation to me. Old school historians of logic, from Prantl all the way up to Bocheński and the Kneales, have an antiquarian focus: their chief concern is to get the historical record straight. This is a praiseworthy endeavour, but tends not to hold the interest of contemporary logicians or philosophers of logic, who may quite understandably ask what's in it for them. By contrast, Steve's historical work has always emphasized the exploitation of neglected material from the past in furtherance of current debates. As I suggest in the interview, this approach extends to logic what Hasok Chang has called the 'complementary function' of philosophy of science: 'a reclamation of past science, a renewed judgment on past and present science, and an exploration of alternatives' ('Philosophy as complementary science', *TPM* **40**).

But Stephen Read may be most familiar to readers of *The Reasoner* as the principal investigator of a major AHRC grant on the Foundations of Logical Consequence. Since its inception in January 2009, this project has produced a string of fascinating conferences and workshops and a growing body of published research. As we discuss in the interview, it has now entered its final phase, on the epistemology of logic, and a call for papers will soon appear for its last conference, to be held in St Andrews in June 2012.

Our conversation took place in Nancy in July, where Steve and I were both attending the Congress of Logic, Methodology and Philosophy of Science. We had a long and enjoyable talk in a small café in the old town, from which the interview below is derived. I am grateful to him for agreeing to be interviewed, for his help in reducing the transcript to reasonable length, and for settling the bill in the café!

Andrew Aberdein
Humanities and Communication, Florida Institute of Technology

Features

Interview with Stephen Read

Andrew Aberdein: What led you into logic in the first place?

Stephen Read: I actually wanted to do maths and English at university, but I went to Keele, because it was famous for cross-culturalization: there was a foundation year in which you studied every subject. That's when I discovered philosophy, and at the same time I discovered that English wasn't the right subject for me.

So I concentrated on maths and philosophy. At Keele, there really wasn't any logic: they taught us the syllogism and not much more. In my last year Alan Treherne was appointed, and he ran a course on philosophy of mathematics. I took that, and a small part of that was interesting logic. So I decided to apply to do a masters in logic, and I went to Bristol maths department. Bristol was extraordinary at that time for they actually had nine mathematical logicians: eight full-time and Rod Laver, who was visiting for a year, so I was able to learn an awful lot of logic in that year at Bristol.

AJA: So when did you become interested in the history of logic?

SLR: When I went to Oxford, which is where one thought if you're going to do philosophy at graduate level you should go. Actually, there were two things that I was interested in when I was at Oxford, and Oxford had little of either.

One was the Chomsky boom. It was all the rage at the end of the sixties: 'Goodness me, there's all this stuff in transformational grammar and this is going to solve all our problems in philosophy of language!' I turned up at Oxford and said this is what I wanted to work on and they said, 'Oh, whoops. We don't know anything about that.' But then they appointed Pieter Seuren at Magdalen in January 1971 and I transferred to him as supervisor. He knew all the latest stuff about transformational grammar, and Chomsky, and so on, and tried to teach me some linguistics, and I think something rubbed off. The other thing that started to interest me then was history of logic. I had two years in Oxford, so I had one summer vacation, when I read the Kneales' *Development of Logic*. And, again, I turned up in Oxford, 'I'm interested in mediaeval logic, what is there in the way of mediaeval logic, or maybe even mediaeval philosophy?' And again, they said, 'Oh! We're not sure we do that. Well,' they said, 'there's a man called Lorenzo Minio-Paluello. But we don't really, sort of, recognize him. He's the only person whose office is in the Phi-

losophy Subfaculty.' Having an office there meant you were a non-person, because you weren't part of the college system. The Philosophy Subfaculty building is just kind of a...

AJA: A nice library?

SLR: A *small* library, a place where the Jowett Society could meet every week, and maybe a place for graduate students to mingle. *Plus* an office for Lorenzo Minio-Paluello, who had nowhere else to go, and no one else was interested in what he did: he was unappreciated. I mean here is a scholar of the first magnitude, the *Aristoteles Latinus* project that he developed is just extraordinary. So poor old Lorenzo had no one to talk to, apart from six graduate students, most of whom knew little or no Latin. But we had a fascinating reading group using Boehner's translations from Ockham. So he was the man who introduced me to mediaeval logic. Then I was appointed to a lectureship at St Andrews in 1972, and the next inspiration was when Graham Priest arrived in St Andrews in 1974.

Not many people know this, but Graham Priest was appointed to a temporary lectureship at St Andrews in the summer of 1974 and spent eighteen months there, and we ran some seminars together on medieval logic. Which led to one of my first two publications: a joint paper with Graham called 'The Formalization of Ockham's Theory of the Proposition' in *Mind*. We wrote two papers together, one of which, that was never published, arose out of working through Desmond Henry's *Mediaeval Logic and Metaphysics*, a very strange book that says the only way really to get an understanding of mediaeval logic is to formalize it using Leśniewski's Ontology.

But the two major things I have worked on in history of logic have both been on fourteenth century logic. All through the eighties I got into a big dispute with Geach, out of that stuff with Graham. Graham and I came up with the idea that there are only three modes of common personal supposition. There's the one that matches disjunctive descent, there's the one that matches conjunctive descent, and then there's a third one which is descent to disjunct terms. Geach said, 'Only logical idiots would think that!' Because of course, considerations of symmetry tell you that there's a descent to a conjunct term. Now Geach was very careful. If you actually look, he says you can descend to a conjunct term, he doesn't actually claim that there's a mode of supposition corresponding to it. So, over many years I was puzzled by this. I discovered a comment in a man called Eckius, Johann von Eck, whom the Catholic Church signed up to refute Luther, because he was such a clever man. But before that, he wrote some works on logic, and in one of those works on logic he says 'Thomas of Cleves, as I remember, was the first person to identify this fourth mode of supposition'.

AJA: Ah ha.

SLR: So I turned up at a conference in 1985, and asked a number of people including Professor de Rijk, who was the omniscient scholar of mediaeval logic, 'Who was Thomas of Cleves?' And it was when I discovered that even de Rijk had not heard of Thomas of Cleves, that I realised that there was a promising avenue of research to pursue. And so I rounded off my supposition theory time researching Thomas of Cleves.

In the meantime, George Hughes had published his translation of chapter eight of Buridan's *Sophismata* in '82, and I reviewed it in '84. I realised at the time that Buridan's theory was unsatisfactory, but I was dimly aware that there was a man called Thomas Bradwardine in the background, but very few people knew anything about Bradwardine then. It lay dormant with me for a long time, and it wasn't until about ten years ago that I started looking seriously at Bradwardine, and then I thought that Bradwardine had everything that was right about Buridan and answers to all the things that weren't satisfactory. So from thinking that this was a very interesting but *mistaken* account of paradoxes I had a road to Damascus moment when I suddenly thought, 'Hang on! This seems to work.'

What's interesting I think about this Bradwardine stuff, in the present context of what people are doing with the paradoxes, is that you make a very well-motivated revision of the truth principles, a revision which is not obviously inconsistent with what Tarski actually said, though it's inconsistent with how people now state what Tarski said, and you can solve the paradoxes without revising your logic. So, if you want to be a classical logician, you can actually do it in classical logic; if you want to revise your logic, as I do, it would be for entirely different reasons.

Very briefly, think of what Tarski actually says, rather than the way people remember him, about the so-called Convention T. For a theory of T to be a theory of truth every instance of 'S is true if and only if p' must be derivable, where what replaces p is a translation into the metalanguage of the sentence a structural description of which replaces S. And the point about Bradwardine is that he points out that some sentences say more than at first appears. So to properly translate, for example the liar sentence, which you designate on the left hand

side, so you've got L is true, where L is the liar sentence 'L is not true'. When you now replace p, you have to translate L into the metalanguage. Now if L is actually saying more than 'L is not true', you've got to say more than 'L is not true' on the right hand side, and Bradwardine has a proof that 'L is not true' not only says that 'L is not true' but also says that 'L is true'. So, the proper Tarskian instance of the T-schema is "'L is true' if and only if L is not true and L is true." So there's no real disagreement with Tarski, but there's a new appreciation of what the T-schema is requiring, which then is a solution, because now the liar is simply a contradictory sentence which is just false.

AJA: I see, yes. Nice.

SLR: Paradox solved. The principle it all hinges on is a closure principle that says that signification, or meaning, or saying that, or whatever you call this semantic notion, is closed under consequence, so that a sentence signifies everything that follows from anything it signifies. I think that's exciting.

AJA: Um hm. Hmmm. Yes, there's a question I've been meaning to ask about that closure principle. Would it require, for example, that the Peano axioms 'mean' Goldbach's Conjecture (assuming it's true)? That struck me as rather startling. But have I got the wrong end of the stick?

SLR: Well, Gödel told us that the Peano axioms are incomplete, so perhaps Goldbach's Conjecture doesn't follow from them even if it's true (and remember that the Gödel sentence has the same form as Goldbach's Conjecture). But yes, Bradwardine does claim that a sentence means everything that follows from it, or from what it means. So if I say that the Peano axioms are true, and Goldbach's Conjecture follows from those axioms, then part of what I've said, what I've meant, is that Goldbach's Conjecture is true.

AJA: Golly. So logically equivalent claims, like, say, Zorn's Lemma and the Axiom of Choice, mean the same thing? (Well, strictly speaking Zorn's Lemma plus axioms of ZF means the same as the Axiom of Choice plus axioms of ZF.) And that seems odd—for instance, I am confident I could explain the Axiom of Choice to my students, but not confident I could explain Zorn's Lemma.

SLR: That logically equivalent sentences mean the same is not such an unusual claim. Anyone who identifies propositions with sets of possible worlds will have to accept that.

AJA: That's true.

SLR: Well, Zorn's lemma and AC are true in the same possible worlds. So on many other accounts, they express the same proposition, so have the same 'meaning' according to those accounts.

AJA: Indeed: that would be even worse. Which is why I wouldn't want to think about mathematical propositions that way—it's clearly at odds with mathematical practice.

But what's so extraordinary about all this is that it comes from the fourteenth century. Hasok Chang has this idea of philosophy of science as 'complementary science' (a rather unhappy term, as it sounds like complementary medicine). It's the idea that philosophers of science should go back to old, discarded paradigms and excavate them for bits that were missed. Your sort of history of logic strikes me as similar: you're going back and digging up things that were thrown away unnecessarily.

SLR: Yes, I think so. That was true of the supposition theory: it possibly has a solution to certain problems about intensionality, which don't have an easy solution given the Frege-Russell account of logic, and that seemed exciting. And Bradwardine was obviously an unbelievably brilliant mathematician, who is pretty famous in the history of science. He's famous in the history of theology for he became Archbishop of Canterbury, and then when he was a very, very young man, possibly in his early twenties, he has this brilliant idea about the semantic paradoxes. There is a sea change in reactions to the paradoxes in the 1320s: the rest of the fourteenth century is spent developing Bradwardine's ideas.

AJA: I also wanted to ask about the early years of relevance logic. You were not quite in the first generation of relevance logicians, but you overlapped with the second generation?

SLR: The first generation is Anderson and Belnap, the second generation is Meyer and Dunn and others, so I would be the third generation. I learnt from Dunn in particular about intensional bunching and suchlike. I've got a paper on 'Validity and the intensional sense of "and"' in 1981 and a paper about disjunction about the same time. So I'm really the eighties generation.

AJA: The impression I have is that at that point, in the seventies and early eighties, this was a philosophical topic that subsequently shifted away from philosophy. That interest in non-classical logics, interest in logic as a whole, became more emphasized in computer science.

SLR: It was philosophical right from the start, but then a lot of technical questions came up during the sixties, including in particular the one that's always talked

about which is the Kripke revolution in modal logic. Modal logic had been a formal theory in search of a semantics in the fifties. People really seriously criticized modal logic. There might have been pretend semantics of various sorts, but there was no real semantics, and Kripke answered that. That's what everyone was waiting for: the world semantics suddenly became *intuitive* in a way that the algebra was completely unintuitive.

In the sixties, they then turned their guns on relevant logic, and said relevant logic is not really a logic because it doesn't have a proper semantics. And so lots of people, the second generation, come in: Dunn and Meyer, the Routleys, both Routleys of course, Maksimova and Urquhart. And Fine. And all of them produced, in various slightly different ways, semantics for relevant logic. But once again it was the Routley and Meyer semantics that took off.

AJA: The worlds again, yes... It's interesting, because it's the worlds that some people find most hard to stomach. But it's a very successful communication tool.

SLR: Right. Then we were left with two worries which have only recently been finally addressed, I think. There's this new paper with eleven authors...

AJA: Yes, yes, I saw that.

SLR: ...in the *Journal of Philosophical Logic*. Well, that is finally answering a challenge which ought to be made, about what the semantics really means. Because relevant logics are paraconsistent, that means theories in them can be simply inconsistent without being trivial. That really was the original motivation, going right back to Ackermann, and the tweak that Anderson and Belnap made, dropping the rule gamma, was all to do with allowing theories to be inconsistent without being trivial. People often characterize paraconsistent logic as not containing the rule of explosion: From *A* and not *A* infer *B*. Now that is a consequence of allowing simply inconsistent theories without trivializing, it means you mustn't contain that rule. But, for example, you might not contain that rule, but the rule might nonetheless be admissible. In which case you wouldn't contain the triviality. So I think it's a bad definition. I think we ought to stick to 'simply inconsistent without trivialization' as the better account of what paraconsistent really means. The other thing that 'paraconsistency' is badly used to talk about is what we should call dialethic theories.

AJA: Yes. Things that are both true and false.

SLR: People often think that if one's a relevant logician, one is therefore . . .

AJA: A dialetheist?

SLR: . . . and therefore that there are true contradictions, yes. I am a relevant logician, I believe in paraconsistent logic, but I do not believe in the existence of true contradictions! It's actually an understandable consequence of the model theory, of the Routley-Meyer semantics, or the Routley star, more to the point, because they need a world semantics which invalidates explosion. In the standard way of taking consequence, if we want to invalidate an inference, we have to find a world where the premises are true and the conclusion false: a world where *A* and not *A* are true and *B* is false. That means we have to find a world where *A* and not *A* are true. And that seems to mean that we in some sense believe in the existence of worlds where *A* and not *A* can both be true, so we're dialetheists. So that's a bad inference, but I think an understandable one. And that's what my very first paper on the intensional sense of 'and' was really all about. It was saying that it all hangs on a bad account of consequence.

AJA: Is that where your version of relevance logic differs vis-à-vis its competitors?

SLR: I think that's right. My distinctive take was published in this book called *Relevant Logic*. The title's no longer an issue, but there was a time when half the relevant logic community, or half the *relevance* logic community called it relevance logic—they tended to be in America—and the other half called it relevant logic, and they lived in Australia. What was the issue all about? Why did we get so agitated about such things in those days? I think really it was to do with the American Plan and the Australian Plan, wasn't it?

AJA: I always assumed that it was a question of whether or not you wanted to help yourself to a pun, about whether this is the stuff that's *relevant*, it speaks to having applications to natural discourse, rather than being the logic of relevance.

SLR: I think that's right too. We'd had *Entailment*, volume one, subtitled *The Logic of Relevance and Necessity*, that claimed that we avoided both fallacies of relevance and fallacies of necessity, which made a lot of people think that they could look at the turn to relevance logic, and they could find some account of relevance. I think it slowly dawned on people that that wasn't forthcoming. There is actually a paper of Bob Meyer's that sets out to establish that relevance logic is not the intersection of the analysis of relevance and the analysis of necessity. And I think that was quite a big thing, that may be the bigger thing that lay behind that change of name, as opposed to logic on the American Plan versus logic on the Australian Plan.

AJA: Yes, I remember you had a paper that was forthcoming for a while, 'The irrelevance of the concept of relevance to the concept of relevant consequence'. I thought that was a hell of title, but when it forthcame it was called something more mundane.

SLR: Much more mundane. It was 'Logical consequence as truth preservation'. I wish it had come out with the original title. I'm not sure I remember quite why it lost that title. It was a bit long, but it had a certain *zing*.

For its first thirty years, the relevance logic programme didn't really have its own conception of consequence. Things were mostly done in terms of theses. This is not unusual. This is how logic was from Frege until the rediscovery of Gentzen in the eighties. We *eventually* realized—I'm sure that there were glimmerings of this before—that logic was really an analysis of logical consequence and not an analysis of logical truth. But for most of the twentieth century the concentration seems to have been on logical truth, axiomatic systems, deriving theorems, not on the consequence relation. And that was true of relevance logic: mostly to do with theorems, very occasionally you would have single premiss consequence. So if you look at the semantics papers of Routley and Meyer, they often include a single premiss consequence relation, but they don't have a multiple premiss consequence relation. And if you look in Anderson and Belnap's book, they have a lot of discussions about things called the official deduction theorem, and so on, but don't really get clear in their own minds what's going on with a collection of premisses and don't clearly distinguish between a collection of premisses some of which could be irrelevant and a collection of premisses all of which are relevant. They talk about it, but they don't get very clear about it. So one of the main driving points about my book was to set relevance logic on a proper philosophical foundation. And that meant trying to be coherent: if one's talking about entailment or consequence, then set up a theory which has at its heart a consequence relation. Now the best way that I found to deal with that was to have a single conclusion consequence relation, with a special new object called a bunch of premisses.

AJA: Right, yes.

SLR: I owe that ultimately to Dunn, who developed this distinction between intensional bunching and extensional bunching, I think even back in his thesis, in the late sixties.

AJA: Do you have newer work I've missed on relevance logic?

SLR: I've moved on to talk about logical consequence in different guises. I can remember being required to say what my research interests were in a short paragraph for the department website, and saying something like, 'My central concern remains the notion of logical consequence in both modern and mediaeval logic'. Quite when that moves away from simply relevant logic I'm not sure. That's a continuity that goes right back to that paper in '81 on the intensional sense of 'and', which was saying that there must be an ambiguity in 'and' in order to diagnose the so-called Lewis argument. The short version of the Lewis argument is to say that it's impossible for *A* and not-*A* to be true and *B* false, therefore *B* follows from *A* and not-*A*. So that second 'and' in there—'*and B* false', right—if that's extensional 'and' and that argument's valid, then forget relevant logic; we have explosion straight away.

So what's going on here? Then you develop relevance logic, or you develop linear logic, and what happens is you move to substructural logics, which get identified by Schroeder-Heister and Došen in the early '90s, partly with the impact of linear logic. Once you restrict the use of structural rules, you appreciate that the connectives start to bifurcate. So that 'and' becomes additive and multiplicative conjunction, which alternatively gets called intensional and extensional conjunction. Similarly with disjunction; similarly with conditional. And, thought the other way, if you start off with these distinct connectives, you realise that by adding sufficiently many structural rules you can conflate them. So the particular ones are W and K, contraction and weakening, and they would work to distinguish or conflate these connectives.

What I was saying is that having discovered that there's a *theory* of two conjunctions, can we put it to work in diagnosing problems? My idea in that paper in '81 was that having discovered that there's this distinct conjunction called 'fusion', then consequence is 'It's impossible for the premisses to be true *fuse* the conclusion false'. And that's what led to that paper in 2003, 'Logical consequence as truth preservation'. Logical consequence really *is* truth preservation, but when truth preservation is properly understood using fusion.

Because if you don't do that, you're forced to say that logical consequence is not just truth preservation, but requires relevance preservation as well, and that in itself leads to paradox as I try to explain in that paper. The simple form of that paradox is to say, 'We know that it's impossible for the premisses to be true and the conclusion false. So we know the argument is truth pre-

serving. We know the premisses are true. But you're not allowed to infer the conclusion because it's *not relevant*'! Even though you have to admit the conclusion is true because the argument is truth-preserving. And I think anyone who's saying that hasn't listened to what they've just said! That's the way most people I think had actually gone. So that's why I'm still interested in the subject.

AJA: I can see how that led to your current AHRC project on the Foundations of Logical Consequence. How did that get started?

SLR: It's to do with the history of Arché. Arché had built up a reputation in logic over its first seven or eight years. You had the maths project, the *Grundgesetze* project, the modality project, the vagueness project. They all came to an end, and there were people still wanting to come to St Andrews to study logic, to visit, to talk about logic, and so on, and there were no logic projects.

Ole Hjortland and Walter Pedriali heard about this. They said, 'Well, would it be a good idea if Steve Read headed up this project?' And we also had Graham Priest and Stewart Shapiro as visiting professors and they're here 25%, so couldn't they be investigators on the project? And then Ole, Crispin Wright and I put a *huge* amount of time, and I suppose thereby effort, into identifying a series of questions and issues and telling a coherent story about how we could, you know, spend a three/four year project working on the foundations of logical consequence. And we put an application together in 2007, it was approved, and the project eventually started in January 2009. And we've had a very interesting, and I think successful project, which has a year to go, on the Foundations of Logical Consequence. Funded very generously by the AHRC, I should record.

AJA: Yes. I keep seeing these very exciting conference announcements for times when I'm several thousand miles away!

SLR: Oh yes. Well, there is one next June. So you're O.K. for that one. In January 2012 we have JC Beall coming to give some daily seminars. The first weekend of December 2011 we have a joint thing with Øystein Linnebo's Plurals, Predicates, and Paradox project at Birkbeck: a workshop on Indefinite Extensibility and Paradox. And then next June we have the conference to end all conferences, or at least to end this project, on the Foundations of Logical Consequence. So watch this space. The invited speakers are up on the website already, and there'll be a call for papers later on.

AJA: Epistemology of logic was something that caught my attention, because much of what I do is epistemology of mathematics.

SLR: I was wondering whether we could actually learn something from the philosophy of mathematics. Because normally I go to conferences, on the philosophy of logic and maths. People think they're *mates*, much like some people think philosophy and theology are natural mates.

AJA: Those guys both use symbols so let's throw them together?

SLR: Yeah. I mean philosophy of maths is epistemology or metaphysics, and the philosophy of logic is normally neither of those. But maybe we can learn something from philosophy of maths.

AJA: That's what Catarina Dutilh Novaes is doing, isn't it, with her idea of logical practice? The sort of stuff which my friends who work on mathematical practice do to mathematics, she wants to do to logic.

SLR: Right. She's keen for us to extend that to the practice-based side of epistemology, but there is a whole epistemology which isn't particularly interested in practice. There are two big names here: Peacocke and Williamson. Peacocke has this thing called the integration challenge—for any subject, but in particular there is an integration challenge for logic, which is, assuming that in the first three phases of the project we've worked out what logical consequence is, how does that square with its epistemology? How would one get a handle on the metaphysics given one's epistemological limitations? That's the integration challenge.

AJA: Right.

SLR: There's another interesting point which actually might interest your readers. There was a question that came up about all the Arché projects just about the time the FLC project started. That was how, or to what extent, or whether the principal investigator should direct the research of the other members of the project. So does the principal investigator say, '*Right*, colleagues! You must now write a paper on...'

AJA: If you're doing physics or chemistry or the like this is part of the way the job works, but it's a novel problem for philosophers.

SLR: And there were people who thought that was the way it should work. Now, with the research students, there's a good chance that you actually can select your students to work on the published research. And to some extent that's true of the research fellows too, because you're picking people who have worked on this, but of course you can't ensure that their minds will continue to work on it. It's something which I think the

humanities, or at least philosophy, hasn't really got to grips with.

You say that it's pretty much de rigueur in the sciences that you recruit people to a research project and tell them, 'Go and work on a particular project. Come up with answers to particular questions.' How does one do that in philosophy? My ideas and my experience were that this was a mistake. And to the small extent that I've actually practiced it, despite thinking it was a mistake, it's been a disaster! My answer to this challenge when it was first brought up, and I think it's the answer that other people in Arché wanted to give as well, was that you can only encourage people, you can't direct them. And one way of encouraging them is to organize events where you bring in outside researchers to, say, workshops, to give you talks, or maybe reading seminars, on this theme, and hope that by being exposed to enough of this, your colleagues on the project will suddenly start to have ideas about it, and then produce answers to these questions.

And the other side of it is that we end up producing things not directly within the anticipated phases, but nonetheless useful material, which would not have existed without this funding. And I think the Research Council is perfectly happy to hear that, provided that you have got worthwhile research.

AJA: Well, they certainly ought to be.

SLR: That's the way I've treated that particular issue. Given that there have been lots of workshops on the foundations of logical consequence, maybe, indirectly, other people, particularly graduate students, will have discovered, 'Oh! There's this interesting subject called philosophy of logic, and I can work on that.'

Frege on / Frege as History

Exchanges between logicians and historians of logic are not always congenial. But with each side accusing the other of misunderstandings and misrepresentations, who's to blame? Is it the historians, who (their critics claim) fail to fully appreciate contemporary developments in logic and who are therefore unable to recognize what is truly valuable in the history of logic from what is peripheral? Or is it the logicians, who (so their critics claim) are inclined to engage in historical investigation only to the extent that it advances their own contemporary projects and who therefore reject the rest without a full appreciation of its significance?

We find a noteworthy criticism of the second variety in Hans Sluga's *Gottlob Frege*, where he writes:

The complimentary tendency, that of underestimating the distance that separates the later tradition [of analytic philosophy] from its beginnings, can equally be illustrated in the case of Frege. Its effect is also that of blocking real historical understanding. Thus, it is taken for granted that Frege was concerned with ontological questions just as the subsequent analytic tradition has been. It is assumed that he was interested in setting up a semantic theory just as logicians have done since Tarski, that, indeed, model-theoretical semantics begins with Frege. His considerations about truth as an object are dismissed as mere scholasticism. His rejection of logicism after the discovery of Russell's paradoxes is considered an overreation; his objections to Cantorian sets are explained as a result of personal hostility. *Wherever Frege's views can be made to fit the current discussion, they are simply identified with it; where they cannot be made to fit, they are either ignored or explained away in psychological terms.* (H. Sluga, *Gottlob Frege*. Routledge: London and New York, 1980, p. 6. [my emphasis])

Such criticisms are not unique to Sluga. They have been made before, and perhaps with good reason on occasion. However, this particular passage should give us pause for thought. For we must not forget that Frege had a fairly sophisticated theory of history, and that the logical fallibility implicit in his understanding of historical progress greatly problematizes criticisms of this sort.

We see Frege's approach to history most clearly in his *Grundlagen*, where he investigates the history of logico-mathematical practice and the supposed a priori principles that "formerly passed as self-evident" for logicians (GL §1). Frege's treatment of history is noteworthy not only because he acknowledges the possibility of error (the "taking-for-true" of mere psychology) while preserving the objective status of genuine a priori truth, but also because he recognizes that, insofar as philosophy is concerned with achieving a priori truth, this distinction is *essential* to its success. For a thinker who many consider to be the paradigm of an a-historical philosopher, what is perhaps most surprising here is that Frege thus situates himself—at first implicitly, but then explicitly after Russell's discovery of the paradox at the

heart of Axiom V—among such a cultural-historic community of fallible logicians.

This needn't come as a surprise. Frege, who describes his own methodology as "Euclidean", notes for example that even Euclid's standards of rigour were not always satisfying to geometers, and out of this critical treatment new developments in modern geometry arose (GL §2). It is in this sense that Frege is obliged to give an account of logico-mathematical fallibility, for in the end it is this that allows him to separate good logic from bad. Good logic, like good geography, clearly delimits the concepts required for establishing deductive truths that were not previously known. Driven by the very nature of the case, good logic carves the conceptual world up at its joints so to speak. It is thus that logic—contra the psychological logicians Frege attacks throughout—can achieve progressive a priori knowledge that is nonetheless objective, *like any other science*. And just as any other science admits approximation and error without compromising its objectivity, so too does logic.

This does not mean that there is nothing interesting to learn from the history of logic, as the *Grundlagen* demonstrates. The practice of logic has a history and it is historical still. However, it can only be so because there is an objective truth to which it aspires. The flux of history—like the changing patterns of individuals' personal psychology—must not be confused with the stable universality of genuine a priori truths and "should not usurp their place" in our investigations (GL, preface).

If the laws of thought are "in essentials the same everywhere" (GL, preface), they also hold every-*when*. There are not different laws of thoughts to suit different times. To claim otherwise would be to fall victim to the genetic fallacy Frege worked so hard to refute. In other words, there is no contradiction in something's being true which one takes to be false; however, there is a contradiction in something's *formerly being true*. In other words, it is one thing to claim that one's past assertions do not accord with the truth, but it is quite another to claim that *the truth does not accord with itself*. And this is in fact what historicism, of the psychologistic kind that Frege identifies it with via the genetic fallacy, would espouse.

Concerning H. Sluga's criticism of analytic philosophy's a-historicism—that it fails to adequately take account of its own historical development, therefore blocking itself off from "real historical understanding"—it must be admitted that insofar as analytic philosophy *is a Fregean philosophy* this a-historicism is not a failing but rather a condition of its success in terms of achieving genuine a priori truth. The history of logic, *insofar as it is history*, can only ever be the history of error. What is true a priori is eternally so and can therefore have no history. To identify Frege's views with the current discussion "where they can be made to fit", and to explain them away psychologically where they cannot, is in fact to be true to the spirit—if not the letter—of his philosophy. Frege would have been the first to approve.

JAMES M. FIELDING
Université Paris I, Panthéon-Sorbonne

Holy Megabucks, Batman! The Astounding Popularity of Superhero Movies

The first person to live to 1,000 is already 60 years old—Aubrey de Grey, Chief Science Officer of the Strategies for Engineered Negligible Senescence (SENS) Foundation

Has anyone noticed a pattern in recent summer film seasons? Maybe it's just me, but lately it seems like one out of every three blockbusters is about a superhero. Spiderman, Batman, Iron Man, the Hulk, Wolverine, Captain America. . . need I go on? A review of any one of these films would be very brief: mind-numbing eye candy. If you like big muscles and even bigger explosions, then superhero films are for you. If, on the other hand, you prefer films that are slightly more sophisticated than WWF throw-downs, then you might find the burgeoning popularity of superdude sagas a wee bit puzzling.

Apart from costume changes most superhero films are pretty much identical. The stories revolve around a central character, who is often a loser, and who experiences some sort of tragedy. The plot thickens when the luckless everyman gets juiced up with some sort of superpower. The story builds to a climax when the supercharged hero dashes off to vanquish a bad guy who really has it coming. OK, so if that about sums it up, then why do superhero movies smash box office records year after year?

Hollywood producers have long known that a film's success depends on forging a powerful personal connection with its audience. But what connection could there possibly be between real people and comic book fantasies?

Ever since people invented anthropomorphic gods—such as Thor, the star of a recent superhero

blockbuster—it's fair to say that humans have been fascinated with superhumans. The coolest thing about superhumans is that they are sublimely untroubled by the mundane problems that plague mere mortals. Compared to the gods, humans are puny, weak, and insignificant. However, humans are also similar enough to the gods that, if we permit our imaginations to run wild, we can privately entertain fantasies about wielding their superpowers: "There, but for the grace of a thunderbolt from heaven, go I."

This leads us back to Aubrey de Grey, the Chief Science Officer of the Strategies for Engineered Negligible Senescence (SENS) Foundation. Unlikely as it may seem, the SENS foundation is an institution that is devoted to finding a cure for death. Yes, you read those words correctly. De Grey is convinced that mortality is a remediable health problem. In part, de Grey is inspired by the fact that the human body contains its own fountain of youth. That is, for the first eighteen years of the human life span, the body endlessly refreshes itself. Teenagers eat junk food, drink soda pop, fry their brains on TV and, instead of withering under such lethal influences, youthful bodies thrive. Truly, for those in the pre-twenty age bracket, whatever does not kill them makes them stronger. Aubrey de Grey's goal is to make the same principle apply to the rest of the human life span.

The problem with such a mission is that it is counterintuitive. Everyone knows that there are only two certainties in life: death and taxes. What on earth would inspire an otherwise level-headed scientist to claim that humans might be on the verge of achieving immortality? By embracing such a rationality-defying goal, de Grey is pursuing what I have described as a "problematic." A problematic can be understood as an imagination-stretching objective that requires the invention of new facts in order to transform the fantasy into a reality. The conventional view of scientific progress is that it depends upon a gradual, systematic accumulation of discrete facts like bricks in a wall. Scientists assume that facts, such as stars in the heavens, already exist (i.e., they aren't a bunch of imaginary inventions like the cockamamie stuff that science fiction writers dream up) and scientists achieve breakthroughs when they assemble a sufficient number of "fact bricks" to construct a new tower of knowledge, e.g., Copernican astronomy, Newtonian mechanics, etc.

However, the process of problematic reasoning works differently. Instead of starting with facts, a problematic begins with a dream and then backtracks by inventing the facts that transform the dream into a reality. For example, Wilbur and Orville Wright dreamed of flying like birds and then invented the facts (e.g., the Wright Flyer) that that transform humans from groundlings into aviators. John F. Kennedy experienced a similar process in the space race: JFK dreamed of landing on the moon and then tasked NASA with the challenge of creating the necessary facts to make his space odyssey a reality.

In other words, problematics often lead where science fears to tread. Crazy as it may seem, today's fantasies are often tomorrow's realities. In other words, fantasies represent a navigational star upon which to focus aspirations, and human reason—via the magic of problematic—often invents the necessary facts to transform fantasies into redefined realities [By the way, I work out the details of these intellectual processes in much greater detail in recently-published book, *Good Science*].

This was certainly true for aeronautics and astronautics. For those who are not convinced that zany pop culture fantasies can have any real impact on the production of scientific facts, then recall that the first nuclear submarine in the US fleet was named for Jules Verne's Nautilus. Or, if that's not wacky enough, note that Martin Cooper claims that he was inspired to invent the cell phone after watching Captain Kirk use his communicator on *Star Trek*.

Albert Einstein once said, "Imagination is more important than knowledge." For his part, McGettigan has said that the future is a process, and the pathway to the future lies through the human imagination. Consequently, if you want a glimpse of what the future might hold for super-humans and immortality, then I recommend that you catch the inevitable sequel to *Thor* or *Captain America*. And don't be surprised if you spot Aubrey de Grey in the front row of the theater.

TIMOTHY MCGETTIGAN
Sociology, Colorado State University

NEWS

Methods for Modalities, 9–12 November

The seventh M4M conference was held from November 9 to November 12 in Osuna, Andalucia, Spain; including a one-day PhD school in Malaga on November 9, and including the fourth LAMAS workshop on the morning of November 11. The main thing about Osuna

is that the venue for M4M was the old university building. Originally, the University of Osuna was established in 1548, but it was disbanded in 1820. It currently is a university college that forms part of the University of Sevilla.

The workshop series Methods for Modalities (M4M) aims to bring together researchers interested in developing algorithms, verification methods and tools based on modal logic. Here the term modal logic is conceived broadly, including description logic, guarded fragments, conditional logic, temporal and hybrid logic, etc. The First Method for Modalities Workshop took place in Amsterdam on May, 1999. An overview of all M4M workshops is found here.

To stimulate interaction and transfer of expertise, M4M features invited talks by leading scientists. This M4M the invited speakers and their presentations were:

- Marcelo Finger (University of São Paulo, Brazil), *A Modal view of Probabilistic Logic*

- Melvin Fitting (City University of New York), *Nested Sequents for Intuitionistic Logics*

- David Gabelaia (Academy of Sciences, Georgia), *The d-semantics of Modal Logic*

- Andreas Herzig (IRIT, Toulouse), *Propositional assignments, announcements, and their applications to logics of action and agency*

- Yde Venema (University of Amsterdam), *Coalgebra Automata*

The Program Committee of M4M accepted 15 papers from 26 submissions. The committee for M4M Osuna consisted of some 30 academics active in the area of computational modal logics. The lecturers at the PhD school in Malaga on November 9 were Alexandru Baltag, Sonja Smets, and Yde Venema.

The 4th Workshop on Logical Aspects of Multi-Agent Systems LAMAS'2011 took place on 11th of November 2011, as part of the M4M7 workshop. The LAMAS workshops started as an ad hoc event in Otago, New Zealand, in 2002, followed by a second one in Dunedin, New Zealand in 2007. In 2010 the LAMAS series was resumed in Toronto, Canada, as an AAMAS'2010 workshop, and is intended to continue as a regular event. For further information on the LAMAS initiative see here. There were 8 submissions to LAMAS of which 5 were accepted for presentation and inclusion in the proceedings.

It is hard to give an overview of events at M4M. Hans van Ditmarsch particularly liked Marcelo Finger's presentation of modal logics for probability, and Andreas Herzig' presentation featuring the various advantages of modal logics with so-called ontic actions: assignments of new values to propositional variables. Although he worked with Andreas on such matters, to see them presented in this overview manner certainly added more value to these quite technical results, including back-references to goals and dreams originally formulated by Raymond Reiter on propositional quantification in AI.

The venue certainly appealed to the participants, in our opinion. First of all, three days of uninterrupted sunshine and sipping coffee and having lunch in the open air is not at all bad for mid November in Europe. Except for the Andalucian locals, we were all very much aware of that. The invited speakers and some organizers were lodged in the fabulous Marques de la Gomera hotel. Hans had a tower room with windows on four sides surrounded by a balcony whereon one could do rounds of jogging in the morning, so to speak. The night of the workshop dinner was enlivened by Manolo junior's magician's tricks, that were very much appreciated and marvelled at by the audience of smart but hopelessly credulous academics. And on the way back in the train from Osuna to Sevilla, the 10 or so people in the same carriage as Hans indeed saw pink flamingoes wading in the waters next to the railway tracks, as if this was the most normal thing in the world. In Andalucia, miracles like that are the most normal thing in the world.

HANS VAN DITMARSCH
University of Seville
DAVID FERNÁNDEZ DUQUE
University of Seville
MANUEL OJEDA ACIEGO
University of Málaga

Reasoning with Cases in the Social Sciences, 11–12 November

Philosophers met social scientists in the inspiring Cathedral of Learning in Pittsburgh on November 11 and 12 for a joint reflection on the use of cases and case studies in the social sciences. The workshop offered outstanding scholars the chance to share experiences and insights from various fields, with each session followed by an hour-long roundtable discussion. A few graduate students were generously supported by

the Center to attend the event. The cozy atmosphere and high-level contributions rendered the workshop an exciting event. This initiative was also remarkable as it called attention to a subject so far poorly discussed and quite disregarded among philosophers. Evidence from the sciences showed instead that the analysis of cases and case studies contribute in an original and substantial manner to our understanding of the social domain. On their part, philosophers and historians of science explicitly acknowledged the importance of this research strategy.

The first pair of talks focused on the relevance of context in the social domain. Sandra Mitchell, who was with Mary Morgan the organizer of the event, opened the workshop. She pointed out that there are circumstances in which contextual features strongly influence phenomena that are characterized by a high degree of instability. There are no universal laws that govern these domains, where the limited invariance of structures rather demands a case-based approach. Denise Rousseau, for several years editor-in-chief of the Journal of Organizational Behavior, offered guidelines to researchers who engage with descriptive studies, for taking into account the contextual features that matter. Concerns about the context should translate into conceptual comparability and attention to the frame of reference, the temporal dimension, the representativeness of the case, potential range restrictions, and the level of analysis.

In the second session the speakers discussed what it means to reason *in* cases and reason *from* cases. Kathleen Blee, who is a sociologist at the University of Pittsburgh, used studies on racism and group formation to illustrate the importance of small-scale analyses. She argued that by "zooming-in", one detects mechanisms that help shed light on society at large, since the complexity of social life becomes more manifest at the micro level. Mary Morgan discussed the *general* validity of case studies. Case studies not only offer heterogeneous material to be variously re-used in other circumstances. The general, she argues, can also be found within the case, for instance in the form of constructs as the cluster of community studies back in the Fifties suggests.

The third session centered on the case study as a research strategy to formulate and test causal hypotheses. Sharon Crasnow, upon examination of evidence from the political science, hinted at the distinction between causes-of-effects and effect-of causes; and argued that case-studies tend to focus on the former. Political sci-

entist Andrew Bennett emphasized advantages and disadvantages of process-tracing as a form of within-case analysis. In particular, he argued that process-tracing partly solves the underdetermination problem that comparative methods inherently face, and in this way helps reach causal conclusions with higher confidence.

Jim Woodward and Rachel Ankeny led the wrap-up session. Woodward focused on the metaphysical and theoretical aspects of the case-centered worldview. Rachel Ankeny recollected the main definitional and methodological issues surrounding the choice, use and re-use of cases in the social domain. The concluding session was as insightful and inspiring as the whole workshop, and gave us reason to hope that philosophers' interest on case studies will soon bloom as this method indeed deserves.

Attilia Ruzzene
EIPE, Erasmus Universiteit Rotterdam

Calls for Papers

Psychological Models of (Ir)rationality and Decision Making: special issue of *Synthese*, deadline 1 December.

Scope of Logic Theorems: special issue of *Logica Universalis*, deadline 24 December.

Preference Learning and Ranking: special issue of *Machine Learning*, deadline 31 December.

Structure of Scientific Revolutions: 50 Years On: special issue of *Topoi*, deadline 15 January.

Imprecision in Statistical Data Analysis: special issue of *Computational Statistics & Data Analysis*, deadline 30 January.

Input & Output Analysis for Simulation: special issue of the *Journal of Simulation*, deadline 1 March.

Formal and Intentional Semantics: special issue of *The Monist*, deadline 30 April.

The Mind-Body Problem in Cognitive Neuroscience: special issue of *Philosophia Scientiæ*, deadline 1 May.

The Aim of Belief: special issue of *Teorema*, deadline 15 September.

What's Hot in . . .

. . . Logic and Rational Interaction

Besides the usual mix of announcements and conference reports (many of which also appear in the pages of the Reasoner), a number of new publications were

highlighted on *LORIWEB* in the past month. The recent book "Computational Aspects of Cooperative Game Theory" by Georgios Chalkiadakis, Edith Elkind, and Michael Wooldridge presents a survey of recent work in the field. Frederik Van De Putte studies "Prime Implicates and Relevant Belief Revision" in an article in the Journal of Logic and Computation. In the same journal, Leila Amgoud and Srdjan Vesic give an account of the role of argumentation in negotiation dialogues. And Hans van Ditmarsch, Wiebe van der Hoek and Ji Ruan establish a connection between dynamic epistemic and epistemic temporal logics in a paper in the Logic Journal of the IGPL.

LORIWEB is a platform for sharing news related to the emerging field of Logic and Rational Interaction. If you have content to share, please contact Rasmus Rendsvig, our web manager or write to the loriweb address.

BEN RODENHÄUSER
Artificial Intelligence, Groningen

. . . Uncertain Reasoning

Uncertainty and rationality play two fundamental, yet conceptually distinct, roles in classical economic theory. Uncertainty is a *fact*, an all pervasive feature of the kinds of problems the theory intends to model. Rationality, on the other hand, is an *assumption* which classical economic models make about individual agents. The approach to decision theory which culminated in Savage (1954: *The foundations of statistics*, Wiley) is also based on the key concepts of uncertainty and rationality, but with an important change in perspective. Whilst the standard assumption of economic models is that rational agents will behave as if they were maximising their expected utility, the focus of bayesian theory is that they will not make blatantly irrational choices. A fundamental epistemological achievement of de Finetti, Ramsey and Savage consisted precisely in putting forward the specific choice contexts in which "blatant irrationality" is formally defined as *incoherence*. Using coherence as a formal wedge (see my October 2011 column for a snapshot of the current debate on this topic), de Finetti was able to justify the use of probability as a measure of uncertainty. Building on this (and on the von Neumann-Morgernstern axiomatisation of expected utility), Savage was then in a position to show how coherence leads to justifying the maximisation of subjective expected utility (the most widely endorsed

rationality assumption in economic theory!) as a normative prescription of rational choice behaviour under uncertainty.

Not everyone agrees with this analysis of uncertainty and rationality, as economists Itzhak Gilboa, Andrew Postlewaite and David Schmeidler illustrate in their unambiguously titled paper *Rationality of belief or: Why Savage's axioms are neither necessary nor sufficient for rationality*, which has recently become available on Synthese Online First. Whilst the authors insist that their goal is to question the suitability of bayesianism as it is currently understood within the limited scope of economic theory ("Our critique is only directed at economic versions of Bayesianism"), they explicitly take issue against "the existence of [probabilistic] beliefs and their justification". Thus, by attacking the probability norm, their criticism bears directly on bayesian epistemology *tout court*. Yet, dissatisfaction with the applicability of bayesian theory to (real life) economic decision modelling hardly provides grounds for rejecting the bayesian justifications for taking belief as probability. All the more so, if the arguments used to make the point rest on the descriptive failures of the theory, as often happens in this paper. Bayesian epistemology and the theory of decision Savage built on top of it, are uncompromisingly normative.

Gilboa, Postlewaite and Schmeidler are well aware of this, and so devote a substantial part of their paper to articulate various claims to the effect that bayesian epistemology falls short of being necessary for the normative, formal characterisation of rational belief under uncertainty. As they concisely put it, "It is sometimes *more* rational to admit that one does not have sufficient information for probabilistic beliefs than to pretend that one does". This idea is articulated at various levels and from distinct angles across the paper. I will limit myself to mentioning two of its particular instantiations.

The first points out that the bayesian norm of probability is silent on the belief formation process. As a consequence, it can only reflect the information possessed by an agent—not the information which the agent does not possess. This generates "cognitive unease", because what we do not know influences directly the rationality of our belief. The main problem with this line of argument, I think, is that it disregards a fundamental feature of bayesian theory, namely the interpretation of probability as the expression of one's beliefs *relative to their state of information* and their personal experience. This is all and only the information which is relevant to the definition of coherent belief and choice behaviour.
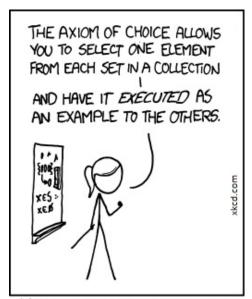
The second, closely related, line of argument takes issue with the bayesian requirement that rational agents should *always* be in a position to come up with a real valued probability for the events at hand. Since, the authors argue, we seldom have enough information to sensibly come up with a probability assessment, the requirement of completeness (of revealed belief and preference) falls short of being normatively necessary. The problem I see with this, is that bayesian theory has normative force precisely because—in their current state of information—individuals are *forced* to quantify their probability for the events at hand. De Finetti clearly insisted on this, especially when analysing the role of uncertainty in economics, as in de Finetti (1969: L'incertezza, in *Un matematico e l'economia*, Giuffrè 2005, p.69-70.). Were a bookmaker allowed to "refuse to sell a bet", we would not be in a position to tell apart the bookmaker's reasoned belief from her "whim". Pushing this argument in another direction, there is no a priori bound to refinements which we can imagine for our current state of information. So there is a clear sense in which if "refusing to choose" appears among the feasible alternatives, it can always be thought as the (only!) rational choice.

I have only briefly mentioned the author's arguments in support of the claim that bayesian epistemology is not necessary for the formal characterisation of rational belief. Arguing against its sufficiency is not particularly troublesome, as the claim probably finds no supporters at all today. To the contrary, the view that "strict" bayesianism should give way to suitable refinements and extensions is currently shared in many areas of uncertain reasoning.

Finally, I think that the bulk of Gilboa, Postlewaite and Schmeidler's criticisms would rest on much firmer epistemological grounds if they were directed towards refining classical bayesianism, rather than rejecting it as fatally flawed. Consider, for example, the reference to "unknown probabilities" which is often in the background, and sometimes well in the foreground of this paper. The very idea of "unknown probabilities" is clearly quite slippery from the bayesian point of view. However, it is apparent that on a number of occasions the authors seem to have in mind probabilities which are not (or perhaps cannot be) *calibrated*, in the Objective bayesian sense (see, e.g. Williamson 2010: *In defence of objective Bayesianism*, Oxford University Press). Similarly, their criticism of "Laplace's principle of indifference" overlooks completely the Objective bayesian discussion on the *equivocation* norm. This fits well with Williamson's observation to the effect that one can hardly accept the calibration or the equivocation norms while rejecting the probability norm. And the latter is precisely what Gilboa, Postlewaite and Schmeidler are unsatisfied with.

Hykel Hosni
Scuola Normale Superiore, Pisa

THE AXIOM OF CHOICE ALLOWS YOU TO SELECT ONE ELEMENT FROM EACH SET IN A COLLECTION AND HAVE IT *EXECUTED* AS AN EXAMPLE TO THE OTHERS.

MY MATH TEACHER WAS A BIG BELIEVER IN PROOF BY INTIMIDATION.

Events

December

CT&IT: International Workshop on Computation Theory and Information Technology, Macau, China, 1–2 December.

LENLS: Logic and Engineering of Natural Language Semantics, Takamatsu-shi, Kagawa-ken, Japan, 1–2 December.

Natural Roots of Human Cognition and Communication: Sensory-motor Concepts in Language and Science: University of Düsseldorf, Germany, 1–3 December.

ICCCI: International Conference on Computer and Computational Intelligence, Bangkok, Thailand, 2–4 December.

INDEFINITE EXTENSIBILITY AND LOGICAL PARADOXES: Arché Research Centre, St Andrews, 2–4 December.

MINDGRAD: University of Warwick, UK, 3–4 December.

PT-AI: Philosophy and Theory of Artificial Intelligence, Thessaloniki, Anatolia College/ACT, 3–4 October.

NCMPL: International Conference on Non-classical Modal and Predicate Logics, Guangzhou (Canton), China, 5–9 December.

ACAL: 5th Australian Conference on Artificial Life, Perth, Murdoch, Australia, 6–8 December.

ICIRA: 4th International Conference on Intelligent Robotics and Applications, Aachen, Germany, 6–9 December.

MIWAI: 5th Multi-Disciplinary International Workshop on Artificial Intelligence, Hyderabad, Andhra Pradesh, India, 7–9 December.

THE COLLECTIVE DIMENSION OF SCIENCE: Nancy, France, 8–10 December.

COPENHAGEN LUND WORKSHOP IN SOCIAL EPISTEMOLOGY: University of Lund, Sweden, 9 December.

ICACM: 1st International Conference on Advanced Computing Methodologies, Hyderabad, Andhra Pradesh, India, 9–10 December.

ICDM: 11th IEEE International Conference on Data Mining, Vancouver, Canada, 11–14 December.

MIND NETWORK WORKSHOP: University of Birmingham, 13 December.

NIPS: 25th Annual Conference on Neural Information Processing Systems, Granada, Spain, 13–15 December.

IICAI: 5th Indian International Conference on Artificial Intelligence, Tumkur (near Bangalore), India, 14 December.

AAL: Australasian Association of Logic, Wellington, New Zealand, 14–15 December.

COCONAT: TiLPS, Tilburg, The Netherlands, 15–16 December.

STATISTICS AND SCIENTIFIC METHOD I: THE CONTROVERSY ABOUT HYPOTHESIS TESTING: Universidad Nacional de Educación a Distancia (UNED), Madrid, 15–16 December.

ALC: Asian Logic Colloquium, Wellington, New Zealand, 15–20 December.

COMPUTING FOR GRAPHICAL MODELS: London, 16 December.

INTERNALISM VERSUS EXTERNALISM: Universiteit van Amsterdam, 16–17 December.

INTERNALISM VERSUS EXTERNALISM: Institute for Logic, Language and Computation, Department of Philosophy, Universiteit van Amsterdam, 16–17 December.

ICISME: International Conference on Information Management and Systems Engineering, Nanjing, China, 16–18 December.

COMPUTING & STATISTICS: Senate House, University of London, UK, 17–19 December.

AMSTERDAM COLLOQUIUM: ILLC, Department of Philosophy, University of Amsterdam, 19–21 December.

CAR: 3rd International Asia Conference on Informatics in Control, Automation and Robotics, Shenzhen, China, 24–25 December.

JANUARY

ISAIM: 12th International Symposium on Artificial Intelligence and Mathematics, Fort Lauderdale, Florida, 9–11 January.

UNIVERSITY OF MIAMI GRADUATE STUDENT CONFERENCE IN EPISTEMOLOGY: Miami, FL, 12–14 January.

MAMLS: Mid-Atlantic Mathematical Logic Seminar, Florida, 13–15 January.

PERSPECTIVALISM WORKSHOP: Ghent, 19–20 January.

MATHLOG: 5th Annual Cambridge Graduate Conference on the Philosophy of Logic and Mathematics, Cambridge, 21–22 January.

VAGUENESS IN LANGUAGE, REASONING AND COGNITION: Amsterdam, 27–28 January.

FEBRUARY

COLOMBIAN CONFERENCE ON LOGIC, EPISTEMOLOGY, AND PHILOSOPHY OF SCIENCE: Bogota, Colombia, 8–10 February.

CONFERENCE ON COMPUTER SCIENCE & COMPUTATIONAL MATHEMATICS: Melaka, Malaysia, 9–10 February.

PERSPECTIVES ON STRUCTURALISM: Center for Advanced Studies (CAS) and Munich Center for Mathematical Philosophy (MCMP), LMU Munich, Germany, 16–18 February.

ICIIN: International Conference on Intelligent Information and Networks, Hong Kong, 17–18 February.

ICICA: International Conference on Information and Computer Applications, Hong Kong, 17–18 February.

ICCMS: 4th International Conference on Computer Modeling and Simulation, Hong Kong, 17–18 February.

ICDC: International Conference on Digital Convergence, India, 18–19 February.

THEORETICAL COMPUTER SCIENCE: Auckland, New Zealand, 21–24 February.

THE EPISTEMOLOGY OF MODALITY WORKSHOP: Cologne, 23–24 February.

ICICN: International Conference on Information and Computer Networks, Singapore, 26 February.

MARCH

FoIKS: 7th International Symposium on Foundations of Information and Knowledge Systems, Kiel, Germany, 5–9 March.

LATA: 6th International Conference on Language and Automata Theory and Applications, La Coruña, Spain, 5–9 March.

DISPOSITIONS, CAUSES, MODALITY WORKSHOP: Cologne, 7–9 March.

GRADUATE CONFERENCE IN PHILOSOPHY OF SCIENCE: Erasmus University Rotterdam, 8–9 March.

NOTHING BUT THE TRUTH: Vienna Forum for Analytic Philosophy, University of Vienna, 9–11 March.

ICMLC: 4th International Conference on Machine Learning and Computing, Hong Kong, 10–12 March.

LPAR: 18th International Conference on Logic for Programming, Artificial Intelligence and Reasoning, Merida, Venezuela, 11–15 March.

AXIOMATIC VS SEMANTIC TRUTH: Munich, 14–16 March.

&HPS4: Integrated History and Philosophy of Science, Department of Philosophy and History of Science, University of Athens, 15–18 March.

EMPIRICAL PHILOSOPHY OF SCIENCE. QUALITATIVE METHODS: Sandbjerg, Denmark, 21–23 March.

WORKSHOP ON PHILOSOPHICAL AND FORMAL THEORIES OF TRUTH: Amsterdam, 23–25 March.

PRAGMATISM, LAW, AND LANGUAGE: University of Idaho, 23–25 March.

CIFEr: Computational Intelligence for Financial Engineering and Economics, New York, 29–30 March.

NEW SCIENCE, NEW RISKS: University of Pittsburgh, 30–31 March.

DICE: 3rd Workshop on Developments in Implicit Complexity, Tallinn, Estonia, 31 March–1 April.

APRIL

YSM: Young Statisticians' Meeting, Cambridge, 2–3 April.

SBP: International Conference on Social Computing, Behavioral-Cultural Modeling, & Prediction, University of Maryland, 3–5 April.

MIND, METHOD AND MORALITY: Pittsburgh, 6–7 April.

TIME FOR CAUSALITY: Workshop on Causal Inference and Dynamic Decisions in Longitudinal Studies, Bristol, 10–13 April.

evoSTOC: Evolutionary Algorithms in Stochastic and Dynamic Environments, Malaga, Spain, 11-13 April.

PHDS IN LOGIC IV: Ghent, 12–13 April.

NORTHWESTERN/NOTRE DAME GRADUATE EPISTEMOLOGY CONFERENCE: Northwestern University, Evanston, IL, 13–14 April.

CONFRONTING INTRACTABILITY IN STATISTICAL INFERENCE: University of Bristol, 16–19 April.

COLLECTIVE INTELLIGENCE: MIT, Cambridge, MA, 18–20 April.

BEING FREE, DOING FREE: Freedom Between Theoretical and Practical Philosophy, University of Freiburg, Germany, 19–21 April.

PSYCHOLOGY, EMOTION, AND THE HUMAN SCIENCES: University of Windsor, Windsor, Ontario Canada, 20–21 April.

MAICS: 23rd Midwest Artificial Intelligence and Cognitive Science Conference, Ohio, 21–22 April.

AISTATS: 15th International Conference on Artificial Intelligence and Statistics, La Palma, Canary Islands, 21–23 April.

THE PROGRESS OF SCIENCE: Tilburg Center for Logic and Philosophy of Science, 25–27 April.

SDM: 12th SIAM International Conference on Data Mining, Anaheim, California, USA, 26–28 April.

MAY

SOPHA: Société de philosophie analytique, Paris, 4–6 May.

BELIEF FUNCTIONS: Compiégne, France, 9–11 May.

GAMES, GAME THEORY AND GAME SEMANTICS: 8th International Symposium of Cognition, Logic and Communication, Riga, Latvia, 18–20 May.

SLACRR: St. Louis Annual Conference on Reasons and Rationality, 20–22 May.

IPDPS: 26th IEEE International Parallel and Distributed Processing Symposium, Shanghai, China, 21–25 May.

JdS: 44th Journées de Statistique, Brussels, 21–25 May.

UR: Uncertain Reasoning, Special Track at FLAIRS-25, Marco Island, Florida, USA, 23–25 May.

SSHAP: Mind, Language and Cognition, McMaster University, Canada, 24–26 May.

THE AIMS OF INQUIRY AND COGNITION: Edinburgh Epistemology Research Group, University of Edinburgh, 25–26 May.

AI2012: Canadian Conference on Artificial Intelligence, 28–30 May.

FEW: 9th Annual Formal Epistemology Workshop, Munich, 29 May–1 June.

ICCC12: Third International Conference on Computational Creativity, Dublin, 30 May–1 June.

HUMAN COMPLEXITY: The University of North Carolina, Charlotte, 30 May–1 June.

JUNE

ADVANCES IN PHILOSOPHICAL LOGIC: Ruhr University Bochum, 3–5 June.

FEW: Formal Epistemology Week, Konstanz, 4–6 June.

AAMAS: 11th International Conference on Autonomous Agents and Multiagent Systems, Valencia, Spain, 4–8 June.

MINDS, BODIES, AND PROBLEMS: Bilkent University, Ankara, 7–8 June.

EDINBURGH EPISTEMOLOGY GRADUATE CONFERENCE: University of Edinburgh, 8–9 June.

MS5: Conference on Models and Simulations, Helsinki, 14–16 June.

BASIC KNOWLEDGE: Conference on the A Priori, Aberdeen, 16–17 June.

SAT: International Conference on Theory and Applications of Satisfiability Testing, Trento, Italy, 17–20 June.

LOFT: Tenth Conference on Logic and the Foundations of Game and Decision Theory, Sevilla, Spain, 18–20 June.

DM: Discrete Mathematics, Dalhousie University, Halifax, Nova Scotia, Canada, 18–21 June.

LOGICA: Hejnice, northern Bohemia, 18–22 June.

CIE: Computability in Europe, University of Cambridge, Cambridge, 18–23 June.

PHILOSOPHICAL INSIGHTS: Senate House, University of London, 21–23 June.

MBR12: Model-Based Reasoning in Science and Technology, Sestri Levante, Italy, 21–23 June.

HOPOS: Halifax, Nova Scotia, Canada, 21–24 June.

COLT: 25th Annual Conference on Learning Theory, Edinburgh, 25–27 June.

ARTIFICIAL INTELLIGENCE AND SOFT COMPUTING: Naples, Italy, 25–27 June.

VANIM: Values and Norms in Modeling, Eindhoven, The Netherlands, 25–27 June.

SQUARE OF OPPOSITION: American University of Beirut, 26–29 June.

ICML: 29th International Conference on Machine Learning, University of Edinburgh, 26 June–1 July.

DGL12: Sixth Workshop in Decisions, Games & Logic, LMU Munich, 28–30 June.

COURSES AND PROGRAMMES

Courses

LI: Logic and Interactions, Winter School and Workshops, CIRM, Luminy, Marseille, France, 30 January–2 March.

ESSLLI: 24th European Summer School in Logic, Language and Information, Opole, Poland, 6–17 August.

Programmes

APHIL: MA/PhD in Analytic Philosophy, University of Barcelona.

DOCTORAL PROGRAMME IN PHILOSOPHY: Language, Mind and Practice, Department of Philosophy, University of Zurich, Switzerland.

HPSM: MA in the History and Philosophy of Science and Medicine, Durham University.

LOPHISC: Master in Logic, Philosophy of Science & Epistemology, Pantheon-Sorbonne University (Paris 1) and Paris-Sorbonne University (Paris 4).

MASTER PROGRAMME: in Artificial Intelligence, Radboud University Nijmegen, the Netherlands.

MASTER PROGRAMME: Philosophy and Economics, Institute of Philosophy, University of Bayreuth.

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

MA IN COGNITIVE SCIENCE: School of Politics, International Studies and Philosophy, Queen's University Belfast.

MA IN LOGIC AND THE PHILOSOPHY OF MATHEMATICS: Department of Philosophy, University of Bristol.

MA IN LOGIC AND THEORY OF SCIENCE: Department of Logic of the Eotvos Lorand University, Budapest, Hungary.

MA IN METAPHYSICS, LANGUAGE, AND MIND: Department of Philosophy, University of Liverpool.

MA IN MIND, BRAIN AND LEARNING: Westminster Institute of Education, Oxford Brookes University.

MA IN PHILOSOPHY: by research, Tilburg University.

MA IN PHILOSOPHY OF BIOLOGICAL AND COGNITIVE SCIENCES: Department of Philosophy, University of Bristol.

MA IN RHETORIC: School of Journalism, Media and Communication, University of Central Lancashire.

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

MRes in Cognitive Science and Humanities: Language, Communication and Organization: Institute for Logic, Cognition, Language, and Information, University of the Basque Country, Donostia, San Sebastian.

MRes in Methods and Practices of Philosophical Research: Northern Institute of Philosophy, University of Aberdeen.

MSc in Applied Statistics: Department of Economics, Mathematics and Statistics, Birkbeck, University of London.

MSc in Applied Statistics and Datamining: School of Mathematics and Statistics, University of St Andrews.

MSc in Artificial Intelligence: Faculty of Engineering, University of Leeds.

MA in Reasoning

An interdisciplinary programme at the
University of Kent, Canterbury, UK.
Core modules provided by Philosophy and further
modules from Psychology, Computing, Statistics,
Social Policy, Law, Biosciences and History.

MSc in Cognitive & Decision Sciences: Psychology, University College London.

MSc in Cognitive Science: University of Osnabrück, Germany.

MSc in Cognitive Psychology/Neuropsychology: School of Psychology, University of Kent.

MSc in Logic: Institute for Logic, Language and Computation, University of Amsterdam.

MSc in Mathematical Logic and the Theory of Computation: Mathematics, University of Manchester.

MSc in Mind, Language & Embodied Cognition: School of Philosophy, Psychology and Language Sciences, University of Edinburgh.

MSc in Philosophy of Science, Technology and Society: University of Twente, The Netherlands.

MRes in Cognitive Science and Humanities: Language, Communication and Organization: Institute for Logic, Cognition, Language, and Information, University of the Basque Country (Donostia San Sebastian).

Open Mind: International School of Advanced Studies in Cognitive Sciences, University of Bucharest.

PhD School: in Statistics, Padua University.

Jobs and Studentships

Jobs

Post-doc positions: in Robot Learning and Reinforcement Learning, Intelligent Autonomous Systems Group, Darmstadt University of Technology / Technische Universitaet Darmstadt, Germany, to be filled asap.

Post-doc position: in the area of developmental robotics and robot learning, INRIA, Bordeaux, until filled.

Two Post-doc positions: in Machine Learning, in the project "Composing Learning for Artificial Cognitive Systems", INRIA Lille, until filled.

Post-doc position: in Machine Learning, University of Massachusetts, until filled.

Post-doc position: in Machine Learning, SUNY at Buffalo, until filled.

Post-doc position: in Philosophy of Mind, psychology, Neuroscience, and Computing, Department of Philosophy, University of Missouri-St. Louis, until filled.

Professor: of Logic, Department of Philosophy, Linguistics, and Theory of Science, University of Gothenburg, deadline 1 December.

Lecturer: in Mathematical and Philosophical Logic, University of Bristol, deadline 5 December.

Lecturer: in Philosophy. AOS: Philosophical Logic, University of the Witwatersrand, Johannesburg, deadline 15 December.

Lecturer: in Logic and Philosophy of Science, Faculty of Philosophy, Louvain University, deadline 15 December.

Eight 3-year Research Fellowships: within the project "The Turing Centenary Research Project: Mind, Mechanism and Mathematics", John Templeton Foundation, deadline 16 December.

Research Fellowship: in Statistics, University of Warwick, deadline 20 December.

Full Professor: in High-Dimensional Data Analysis, Department of Statistics, University of South Carolina, deadline 31 December.

Professor and Tier I Canada Research Chair: in Epistemology and Metaphysics, Department of Philosophy, University of Alberta, deadline 15 January.

3-year Post-Doc

To work on the relationship between Bayesian epistemology and inductive logic. Philosophy, University of Kent, deadline 15 February

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Post-Doc Position: in the History and Philosophy of Science, University of Pittsburgh, deadline 15 February.

PROFESSOR: of Statistics, Queen Mary, University of London, deadline 25 February.

Studentships

PHD POSITIONS: in Robot Learning and Reinforcement Learning, Intelligent Autonomous Systems Group, Darmstadt University of Technology / Technische Universitaet Darmstadt, Germany, to be filled asap.

THREE DOCTORAL TRAINING GRANTS: School of Computing, Faculty of Engineering, University of Leeds, until filled.

ONE DOCTORAL RESEARCHER position and one STUDENT RESEARCH ASSISTANT: in uncertain reasoning in the intersection of philosophy, psychology and cognitive science, Munich Center for Mathematical Philosophy, LMU Munich, until filled.

PHD POSITION: in the area of developmental robotics and robot learning, INRIA, Bordeaux, until filled.

PHD POSITION: in Bayesian Decision Theory, School of Computer Science and Statistics, Trinity College Dublin, until filled.

PHD POSITION: at the Institute for Logic, Language and Computation (ILLC), deadline 1 February.

PHD STUDENTSHIP

To work on the relationship between Bayesian epistemology and inductive logic. Philosophy, University of Kent, deadline 15 February

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University of Kent

CENTRE FOR REASONING

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