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§1 Editorial

In my work as a critically thinking scientist, I usually try to respect the following simple principles: (1) Don't believe in contradictions; (2) Don't let others tell you what to believe; (3) Don't believe anything without dependable evidence. Translated into the daily-life problem of assessing subjective degrees of belief in a proposition with an unknown outcome, e.g., whether it will rain today or not, these principles have some noteworthy consequences. Most importantly, it seems that the Bayesian approach of representing epistemic degrees of belief as subjective probabilities reaches its limits. While it is true that the degrees of belief of two complementary propositions should not sum up to a value greater than one (to avoid contradictions), it appears that they should be allowed to sum up to something smaller than one (to avoid being told what to believe). Another incompatibility with the above principles results from the basic mechanism of Bayesian inference, which requires initial beliefs that one holds before any evidence is ever collected.

In the early days of probability, the idea of non-additive probabilities and the avoidance of prior probabilities were very present. The most explicit exhibition of such ideas can be found in the literature of the late 17th and early 18 century, most notably in the works of Jacob Bernoulli and Heinrich Lambert, who tried to establish a connection between the additive theory of chance or randomness (a feature of the world) and the non-additive theory of probability (a feature of our mind). According to Heinrich Lambert, a syllogism has three parts, the armative, the negative, and the indeterminate. This implies that the sum of probability values assigned to the affirmative and the negative part of a syllogism is generally smaller than one.

Bernoulli's and Lambert's ideas of non-additive probabilities were completely eliminated from mainstream probability for almost three full centuries. In a remarkable series of pioneering papers in the late 1960s, Arthur P. Dempster proposed a surprisingly simple model for probabilistic inference, which reinterprets some of these ancient ideas from a modern perspective.

The separation between aleatory and epistemic uncertainty is achieved by considering two separate sample spaces, the available evidence is encoded as a multi-valued mapping between these spaces, and the non-additivity is reflected in what Dempster called lower and upper probabilities. One of the key features of this model is the fact that it includes, rather than denies, classical Bayesian inference. Dempster's original papers are simple, ground-breaking, and mathematically very elegant, which is why I would certainly mention them first when asked about my favorite research papers.

It's a great pleasure and honor to start this month's issue of The Reasoner with an interview with Arthur P. Dempster. His ideas have influenced my own intellectual interests like no one else's, and his work has always been one of the key reference points in my own research. Arthur, thanks for agreeing to be this month's interviewee.

Rolf Haenni
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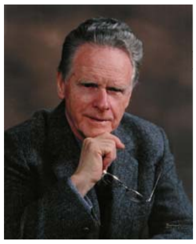
§2
Features

Interview with Arthur P. Dempster

Arthur P. Dempster is Research Professor of Theoretical Statistics in the Harvard University Department of Statistics. He joined the fledgling department in 1958, and remained active in teaching and research until 2005, including supervision of about 50 doctoral theses. His 1956 Ph.D. in Mathematical Statistics from Princeton University was preceded by a 1952 B.A. in Mathematics and Physics, and a 1953 M.A. in Mathematics, both from the University of Toronto. He is known outside his primary discipline of statistics as a co-founder of the Dempster-Shafer (DS) theory of belief functions, and inside statistics for early work on multivariate analysis in the 1960s and for recognition in the 1970s of the wide usefulness and importance of what he called the expectation-maximization (or EM) algorithm for maximum likelihood parameter estimation.

Rolf Haenni: Your original 1966, 1967, and 1968 papers (Annals of Mathematical Statistics, 37:355–374 and 38:325–339, and Journal of the Royal Statistical Society B30:205–247) laid down foundations for a generalization of Bayesian inference. Can you explain in a few words the main underlying ideas and claims?



Arthur P. Dempster: Not in a few words, but I will attempt in the course of this interview to convey how I understand the theory and its potential role in scientific uncertainty assessment. Readers seeking a glimpse of my theoretical orientation as of two or three years ago might look at my contribution to the 2008 IJAR issue in memory of Philippe Smets (International Journal of Approximate Reasoning, 48:365–377). DS for me remains very much a work in progress.

RH: What was your scientific inspiration for developing this theory? How much were you influenced by Fisher's theory of fiducial inference?

APD: The small statistics program in the celebrated Princeton Department of Mathematics of the mid 1950s faced as much east to the active statistical scene in the UK as it did west to the frequentist bastions of Berkeley and Stanford. John Tukey in particular was intrigued and inspired by Ronald Fisher's fundamental mathematical and conceptual contributions, then ongoing after more than forty years. In my earliest years of teaching I made it a priority to penetrate and reflect on Fisher's deep understanding of how mathematical probability connects to scientific uncertainty, including his introduction of many significance testing tools that remain a bedrock of statistical practice, and his attempts to come to terms with the role of the likelihood function and its relation to his sketchy notion of fiducial inference. The fiducial concept from 1930 was much disliked by resurgent Bayesians circa 1960, since it claimed to provide posterior inferences without the contentious Bayesian priors to which they were committed. It had also been severely criticized much earlier by the anti-Bayes frequentist school developing around Jerzy Neyman in the 1930s and 1940s. In response Fisher was not shy about attacking Neyman's behavioral theories as largely misdirected. The motivation on my part for what became DS was like Fisher's, hence my first two papers can be correctly viewed as aimed at putting fiducial inference on a secure foundation.

RH: I assume your original idea as a statistician was to develop a generalized theory of statistical inference.

APD: I had no such ambition. My goal was problem-solving: why did Fisher restrict fiducial inference to cases of continuous observables, declaring the method inappropriate for discrete observables such as outcomes of coin tosses? His concept of pivotal quantity carries over easily. The problem-resolution was recognition of the possibility of relaxing Fisher's implicit assumption that posterior probabilities of the truth or falsity of any uncertain assertion had to be p and q with $p + q = 1$. In the semantics I use today, the alternative is p, q, and r with $p + q + r = 1$, where p is probability "for" truth, q is probability "against" truth, and r is probability of "don't know". From this relaxation of "something big", to quote a much later personal comment from John Tukey, many things quickly followed. I recall a "Eureka" moment while walking down a street, when the rule of combination clicked into place, bringing in its train the central DS concept of independence. For statisticians, the "product-intersection" rule does away with Fisher's reliance on an ad hoc likelihood-based principle of sufficiency for sample data reduction, since the result of each observed sample draw, as from a single coin toss, is a DS inference, whence the combination rule is exactly the new sufficiency principle for combining independent sample draws, with no need for preliminary data reduction to total counts. This and many other basic ideas are buried in the 1966 paper, the best of my career.

RH: How do you explain that since then it has attracted much more attention outside of statistics, in areas such as Artificial Intelligence and Information Fusion?

APD: Inside statistics, the idea failed to take hold in part because it seemed too taxing computationally and in part because the frequentists and Bayesians were then—as they remain to this day—too consumed by arguing with each other to pay attention to a radical new proposal, despite its fundamental simplicity and power, and despite even the ways in which it can bridge important aspects of what they were arguing about. To tell the truth, I despair of the future intellectual health of the statistical professions given their inability to come to terms with (p, q, r) and the elegant technologies of DS inference. Outside statistics, the language that Glenn Shafer introduced in his 1976 book was accessible to specialists with a computer science orientation, resulting in widespread but often confusing attempts over 30 years to apply the theory. While AI and Data Fusion communities may be better known to your readers, there is an obvious unfilled hunger for probabilistic assessments of uncertain past, present, and future in a huge range of quantitative sciences and professions. In my opinion, computed model-based DS inferences have the potential to contribute to satisfying important parts of this hunger in ways that neither of the popular statistical schools can match.

RH: How do you judge Glenn Shafer's contribution to your theory?

APD: His 1976 book contains beautiful mathematics made easily accessible through the assumption of finite state spaces. His highly original research and writing over the subsequent decade made certain that the theory would not disappear from view.

RH: Glenn Shafer interpreted your original notions of "lower and upper probabilities" as non-additive degrees of belief. From a philosophical point of view, do you think this is correct?

APD: Absolutely. Regarding "non-additive", since $p + q + r = 1$, it is typically the case that $p + q < 1$, in place of the Bayesian "additive" $p + q = 1$. Also, while scientists often recoil in horror at the use of the word "belief", the everyday work of scientists is laden with assumed beliefs. In effect, the term "degree of belief" is little more than another technical name for probability, perhaps emphasizing tentative commitment when applied. A "lower probability" (or "belief" in Shafer's technical language) is what I now prefer to call "probability for the truth" in relation to a defined assertion and defined inputs of information. These probabilities are both formal and subjective, or "personal" to use the term adopted by Jimmie Savage over the decade of the 1960s that preceded his unfortunate early death.

RH: Over the years, your theory has constantly been adapted and reinterpreted. What were the most important developments in your eyes?

APD: What I call DS hews closely to the original formulation of my early papers and Glenn's book. The two most important subsequent technical developments are, first, the recognition in the 1980s that models with what we call join tree structure permit localization of inferential operations and thus computational speed, and, second, a concept of "weak belief" that Chuanhai Liu and I have yet to commit to print.

RH: Your theory has always been controversial, especially the combination rule for two independent pieces of information from your original paper. Why is this?

APD: DS methodology is essentially a bridge between Boolean logic and Bayesian logic. Neither end of the bridge is adequate in isolation. At the Bayesian end, the concepts of independence and multiplication of probabilities have been successfully linked for centuries. At the other end, assumptions of independence implicit in the Boolean intersection rule are rarely recognized and questioned (as they could be in each specific application). I do not find that competing approaches have anywhere close to comparable ancestry. For me, there is no controversy.

RH: Another trend in the more recent literature is to apply your theory in various areas. Which do you think are the most important ones?

APD: Formal representations of objective reality together with formal representations of subjective reasoning allow personal choices of mathematical models in support of uncertainty assessment in a wide swath of sciences and professions. It is not easy to predict where DS will break through.

RH: It looks like your theory will keep you busy for the rest of your academic career. Will it?

APD: Right.

RH: What exactly are you working on now and what are your next plans?

APD: Right now I am working at understanding how scientists currently formulate and report the uncertainties about many aspects of climate change. How should they? You can guess how I approach the matter. On the theory side, there is much to do developing models and computational procedures that may have applicability across many applied fields.

A note on tokenism and self-reference

Let me explain some terms. A *sentence* is an abstract object of which a *sentence-token* is a concrete realization. *Indexical* sentences are the ones that contain context-dependent terms such as *here, now, me,* etc. A *linguistic code* is a system fixing the syntax, the semantics and maybe, to a certain extent, also the pragmatics of a language. The *Liar* is any sentence (seemingly) saying of itself that it is *false*, while the *Strengthened Liar* is any sentence (seemingly) saying of itself that it is *not true.*

I give the name of "tokenism" to the claim that there are sentence-tokens of a same *non indexical* sentence-type that have different logical values *under a same semantic code.* Tokenism entails that linguistic codes do not correlate propositions and sentences but propositions and sentence-tokens.

I'll argue for the following two claims:

1. The existence of the Strengthened Liar and related cases strongly supports tokenism.

2. Those cases show that tokenism ultimately relies on the impossibility of any semantic object to refer to itself, as is expressed in the formula:

(Q) $j^{M}(\phi) \notin U^{M}$

where ϕ is a syntactic object, $j^{M}(\phi)$ is the semantic object expressed by ϕ according to some model M, and U^{M} is the universe of discourse of $j^{M}(\phi)$ Thus (Q) says that no semantic object quantifies over itself (cf. Luna, L. "Can We Consistently Say That We Cannot Speak About Everything?" *The Reasoner* 2(9)).

We are given a sentence:

'L expresses no true proposition'

and we assume a code where 'L' is a name for that very sentence-type.

We assume for *reductio* that sentences, and not only sentence-tokens, express propositions or, what can be taken as equivalent, that all tokens of a same non indexical sentence possess the same logical value. Call it assumption 1.

We try to assess L and get through the usual reasoning to the conclusion that L has no truth value, hence that L expresses no proposition, hence that *L expresses no true proposition.* So we are led to use a token of L. This forces us to reject assumption 1. We become aware that, by means of the token of L we have used, we have referred to the expressive power of a previous token, which failed to refer to its own expressive power. If the token referred to were able to refer to its own expressive power nothing could prohibit it from expressing the same proposition we expressed about it by means of another token of the same sentence-type, for then both tokens would be referring to the same referent and attributing the same predicate to it. But, of course, this would cause contradiction. Thus we need the limit imposed on self-reference by (Q) to block the paradox. Consequently, it's not the case that both tokens have the same referent: the first has no referent at all, not just because it refers to an object that happens not to exist (like the king of France) but because it is essentially unable to perform the sole reference it could in accordance with the linguistic code here assumed.

Let's now confront a more explicit case. Let 'K' be a name for this sentence:

'No token of sentence K expresses a true proposition'

In fact, when facing K for the first time we cannot avoid facing a *first token* K1 of K. We know that the capacity of reference of K1 is restricted: it cannot at the same time express a semantical object and quantify over it. Since there is no previous token of K to which K1's subject could refer, K1's subject fails to refer and K1 expresses no proposition. So we assert by means of a *second token* K2:

'No token of sentence K expresses a true proposition'

And we do so without contradiction because the quantifier in K2 does not refer to K2's expressive power but only to K1's since K1 is the only token of K *so far* available for K2 to refer to: K2 itself is *not yet* available for that purpose.

Instead of saying "no token of K" we can just name the token we met 'K1' (as we have done in fact) and utter:

'K1 expresses no true proposition'

so getting around all tokens of K. *But we need not do so.*

In the meantime, I suggest, we have learned something: *no semantical object refers to itself and this is why the paradoxical sentence-tokens fail to express propositions.* We should assume this not only because the device has worked in solving the paradox but also because it has an intuitive *phenomenological* ground: *no intentional act is its own intentional object.* Like defining and reasoning, referring cannot be circular. We can only refer to (or quantify over) what is *previously* given to us as a possible intentional object. And no thought, no intentional act, no semantic object is so given to itself because nothing is previous to itself. That's what (Q) asserts.

The learned lesson, I claim, tells us something about the *Sainsbury-type* cases studied by Gregor Damschen in "This Is Nonsense", *The Reasoner* 2(10). Consider for instance sentence N:

'N expresses no proposition'

Again when we face N for the first time, we face a first token N1 of N. Contrary to what Damschen contends, the fact that N1 can express a false proposition without contradiction while it cannot express a true one, is *not* decisive now that we know of the existence of sentence-tokens devoid of any truth value and which share a most significant feature with N1: *in order to express any proposition at all they would have to accomplish the impossible feat of expressing a self-referential proposition.*

If N1 is not true, that should not serve as a *reductio* to infer its falsity since a third possibility, namely that N1 *expresses no proposition,* is now available and strongly suggested.

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A Sci-Fi Scenario Refuting Rast on Essential Indexicals?

There is a wonderful Sci-Fi short story—Impostor by Philip K. Dick—which bears on Erich Rast's argument against the "implausibly strong" version of the irreducibility of "I" thesis in *The Reasoner* 2(10). (Read the story anyway, even if you don't like what follows!) Rast initially formulates this argument as

> (IRI) I is irreducible in thinking, because for any kind of condition ϕ that is supposed to uniquely identify a person P thinking an I-thought, P might not realize that she herself is the ϕ-er.

He then provides a further analysis of this argument framed in a formalism based on Kaplan's logic of demonstratives, because this framework is neutral with respect to IRI. Four versions emerge, two of which are not contenders, since they are framed in 3rd person terms, and two, which are framed in 1st person terms, which compete as the best way to render IRI.

Rast rejects the stronger version which

> roughly says that for any property A in any context the agent (viz. the speaker or thinker) of that context might believe that the individual object that actually exists and actually satisfies A is not identical to himself.

Rast argues that, because we should reject this implausibly strong formulation we should accept the far weaker alternate which

> roughly says that for any property A in any context it is possible that the agent of the context believes that he himself is not identical with the object that exists and uniquely satisfies A *according to his beliefs*.

I don't see why we should. I won't repeat Rast's formalism, since his argument against the stronger claim is informal in character. The stronger claim he seeks to reject looks like a denial of necessary connection: that where a property is satisfied by an existing object and *does* uniquely characterize a speaker/thinker, there is no necessity that s/he should believe that it does. Rast claims that this does not fit Perry's examples of irreducible indexicals, and then caps this claim with the following:

> After all, if John Perry doesn't recognize himself as the only person with such-and-such properties (say, in the supermarket), only whatever he believes to have these properties can be relevant for his behavior and not whatever is *actually* uniquely determined by them or not.

Philip K. Dick's impostor (I am going to spoil the story for you) is a robot simulacrum of a human being, who is, at the same time, a bomb of stupendous destructive power which has been secreted on the Earth by hostile aliens from Alpha Centauri. This bomb will be detonated by the utterance of a set of code words.

The impostor has been arrested by security forces and whisked away to the relative safety of the Moon before these words can be spoken. The impostor does not know that he is the robot bomb, he thinks he is the man he impersonates, and does everything to vindicate the man he imagines himself to be, who he takes to have been grossly wronged, falsely arrested and accused. The final showdown confronts the impostor with incontrovertible evidence that he is the robot bomb, and the words he utters when he is faced with this realization are the code that detonates the bomb . . . "The blast was visible all the way to Alpha Centauri."

Thus, for the impostor, "only whatever he believes to have these properties can be relevant for his behavior and not whatever is actually uniquely determined by them or not." I haven't gone back to check Perry's examples, but Philip K. Dick's will do. If you don't like Sci Fi, then consider examples of amnesiacs regaining memories—'Am I the husband of this woman?' It is the lack of a necessary connection here which makes possible every kind of derangement, and makes the achievement of a rational outlook possible, but, sadly, contingent. This point would be lost were we restricted to Rast's weaker formulation, which is relativised to the sphere of the individual's beliefs, and their purely internal consistency, rather than counter posing who I know myself to be to what I am actually like.

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Non-Factivity About Knowledge: A Defensive Move

The view that somebody can know a proposition p when p is false is almost universally rejected. Many philosophers are sure that knowledge entails truth—that knowledge is "factive". Furthermore, most take it to be obvious: those who think there are analytic truths are likely to think it a paradigm of analytic truth, those who trust the weight of history can find endorsements of factivity in most of the great thinkers about knowledge since Plato. There is a very strong consensus about the factivity of knowledge, and many of us have an instinctive, and strong, inclination to believe it and support it. It is instructive that a lot more attention has been devoted, historically and in the recent philosophical literature, to whether we should accept counterexamples to the law of non-contradiction than to denying the factivity of knowledge.

Indeed, there are only two recent defences of the failure of factivity that I know of in the literature. Allan Hazlett's "The Myth of Factivity" (*Philosophy and Phenomenological Research*, forthcoming), which defends non-factivity for "the concept of knowledge that serves as the meaning of 'knows' in ordinary talk" and Cristoph Kelp and Duncan Pritchard's "Anti-Realism, Factivity, and Fitch" (in *New Essays on the Paradox of Knowability*, ed. J. Salerno, Oxford UP, forthcoming). Hazlett defends denying factivity on the basis of ordinary uses that appear to not be factive such as Hazlett's example: "Everyone knew that stress caused ulcers, before two Australian doctors in the early 80s proved that ulcers are actually caused by bacterial infection". Kelp and Pritchard argue for non-factivity as part of their response to Fitch's paradox.

This paper is not intended as an argument for this sort of non-factive position, and does not depend on any of the positive arguments Hazlett or Kelp and Pritchard have offered for their non-factive positions. I rather wish to argue that non-factivity has a powerful and unexpected resource to deal with the very serious challenge it immediately faces of explaining how the belief in factivity could seem so secure and obvious to so many.

It seems that non-factivity about knowledge, if it is to be viable, would be best if it could concede something important to the widespread belief/intuition/commonplace that knowledge is factive. There are the usual moves to concede something to this widespread belief, of course, of pointing out that knowledge is often of truths, that perhaps the exceptions are rare, unusual, or little thought of, that perhaps the generic claim that knowledge is factive is true even if the universal claim that *all* knowledge is factive is not. A non-factivist can also cite the weight of tradition and unquestioning acceptance of factivity as a dead hand that hinders proper critical scrutiny. But there is something much more satisfying, and distinctive, that the position can concede to its critics.

It is open for a proponent of non-factivity about knowledge to concede that their opponents *know* that knowledge entails truth. This would concede that their opponents are in an excellent epistemic position vis a vis their opinion that knowledge is factive, and might help explain why it seems so epistemically secure, why their opponents might feel entitled to be sure of it, and so on. Of course, the proponent of non-factivity could continue, that is not to concede the argument—factivity may be known, but that's compatible with it being false, according to non-factivity. A defender of non-factivity who concedes her opponent knows that knowledge is factive has done a great deal to explain why her opponents feel they are in such a good position to reject factivity, and arguably to do a lot of justice to some of the intuitions we share about factivity's appeal.

Suppose a defender of non-factivity conceded that many of her opponents know knowledge is factive. (Perhaps because her opponents are in possession of a strong inductive argument that knowledge is factive, drawn from the many typical cases where it is true things that are known; or perhaps her opponents have followed some other generally virtuous method to reach their conclusion.) What epistemic attitude should the defender take *herself* to have to the proposition that knowledge is not factive? Well, I expect that would depend. If she thinks that she also knows that knowledge is factive, then she should either say that mutually inconsistent things can be known by the same person (not so bizarre if knowing inconsistent things does not entail that inconsistent things are true), or she should take her attitude towards non-factivity to be something less than knowledge: perhaps she only believes it with what she takes is some, but not conclusive reason. Or perhaps there is some Moore-paradox-like instability in defending non-factivity while taking oneself to know the truth of factivity, though this would need to be further investigated, preferably in a way that does not beg the question against non-factivity.

Alternatively she could maintain that while her opponents know that factivity is true, she does not—perhaps even that she knows the opposite. That different people could know inconsistent things is much more plausible once non-factivity is granted. She has a range of options here, then. And of course a view does not need actual defenders to be worth taking seriously, so even if there was some difficulty in oneself holding that non-factivity were true and also that factivity is known to be true, that would not obviously refute the position, but rather only make ad hominem trouble for its defenders.

Sometimes when people talk about a position and "a defender" or "the proponent" of it sympathetically, then they are employing the philosopher's assertion sign. Do not take it that way here. I, like most other people, know that factivity about knowledge is true.

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Armchair versus Questionnaire Polled Intuitions: Intuitions nevertheless!

Philosophers often appeal to their intuitions when putting forward theories about what counts as *knowledge*, what it means to *act freely*, what is the *moral* thing to do and so on. Among those philosophers, *conceptual analysts* are the ones who have mostly defended reasoning by appeal to intuition. According to them, philosophy is a conceptual investigation. Language imposes conceptual rules on all competent speakers. Philosophers are entitled, as competent speakers themselves, to invoke their intuitions concerning some particular target concept and analyse them. Thus they can bring to light the necessary and sufficient conditions for attributing *knowledge*, *morality*, *intellectual behaviour* or whatever other concept relates to the problem they aim to clarify.

Conceptual analysts hold that we all share the same concepts; hence we have the same intuitions. Intuitions are generated by conceptual rules and can thus provide us with *epistemic standards*: Analysing our intuitions on *knowledge*, for example, can help us lay out the *principles* that a certain belief should meet in order to count as knowledge. The clarification of the term will offer a better understanding of all issues related to this concept.

Over the past years, however, more and more experiments have investigated our intuitions: this began with psychology, but recently a new movement under the name of *experimental philosophy* has emerged. Experimental philosophers design questionnaires and record subjects' intuitive responses when confronted with philosophical thought experiments. Those studies provide strong evidence that age, background knowledge or beliefs, culture and the narrative of a philosophers' thought experiment influence our intuitive responses.

For example, J. Weinberg, S. Nichols & J. Stich (2001, "Normativity and Epistemic Intuitions", *Philosophical Topics* 29: 429–460) examine the reaction of subjects coming from different cultural environments to counter-examples of the kind Gettier suggested in order to confront the claim that *knowledge is justified true belief* (E.L. Gettier 1963, "Is Justified True Belief Knowledge?", *Analysis* 23: 121-23). Subjects were requested to consider the following story:

> Bob has a friend Jill, who has driven a Buick for many years. Bob therefore thinks that Jill drives an American car. He is not aware, however, that her Buick has recently been stolen, and he is also not aware that Jill has replaced it with a Pontiac, which is a different kind of American car. Does Bob really know that Jill drives an American car, or does he only believe it?

In this study, subjects were asked to say whether Bob (a) really knows or (b) only believes. It turned out that 74% of western subjects would agree with Gettier that (b) Bob "only believes" that Jill drives an American car, while 56% of Asians and 61% of Indians think (a) he "really knows".

In the same spirit, more and more studies are conducted suggesting, for example, that intuitions differ depending on how many philosophy courses one has attended; that responses on thought experiments vary according to whether one has considered other thought experiments first; that the rhetoric of the thought experiment also influences our intuitions. Those experiments show that background knowledge and intellectual habits, affective content and probably lots of other factors too influence our intuitions. (See Alexander &

Weinberg 2007, "Analytic Epistemology and Experimental Philosophy"; Knobe 2007, "Experimental Philosophy", both in *Philosophy Compass* 2.1: 56-92. Also Knobe & Nichols 2008, *Experimental Philosophy*, Oxford University Press).

Experimental philosophers criticise the conceptual analyst's *armchair* appeal to intuition: Philosophers cannot just sit on their armchair and reflect on their own intuitions; one needs to go out and investigate which intuitions actual folks have.

Armchair conceptual analysts, on the other hand, reject the studies conducted by experimental philosophers: Some of them suggest that those studies involve questioning subjects about very short stories. The subjects have to fill in those narratives in order to make sense of them but usually different people fill in the stories differently. This explains why the subjects provide different answers and suggests that the methodology of experimental philosophy is inadequate for philosophical investigation (Sosa 2008, "Defense of the Use of Intuitions in Philosophy", in M. Bishop & D. Murphy, eds, *Stich and His Critics*, Blackwell). Others suggest that these studies collect *surface* intuitions rather than the reflective or *robust* ones philosophy needs (A. Kauppinen 2007, "The Rise and Fall of Experimental Philosophy", *Philosophical Explorations* 10: 97-118). For philosophers are more trained in introspection and reflection than an average person (Bealer, G. 2004, "The Origins of Modal Error", *Dialectica* 58: 11-42), etc. To make a long story short, there is an on-going dispute between armchair conceptual analysts and experimental philosophers.

However, what some experimental philosophers share with the armchair conceptual analysts is the underlying assumption that philosophizing should start with an appeal to intuition: folk intuitions work as a guide to commonsensical concepts and, in their view, our common conceptual background is related to many philosophical problems. This is the reason why many philosophers today come out of their studies and conduct experiments: they want to find out what intuitions actual folks have concerning some target concept and analyse them. Intuitions again, are supposed to provide a clear starting point when reasoning about a philosophical problem (see Knobe & Nichols 2008, "An Experimental Philosophy Manifesto", ibid: 8-9; T. Nadelhoffer & E. Nahmias 2007, "The Past and Future of Experimental Philosophy", *Philosophical Explorations*, 10.2: 126).

Yet, if intuitions rely on cultural and/or socio-economic background, previous knowledge, the classes one has attended, the narrative of the questionnaire-story etc., this only suggests that intuitions might be a tricky starting point for philosophy altogether. Depending on a person's background then, tacit theories come in and the intuitions reported are only as good as the tacit theories that generate them. Moreover, new knowledge or a different narrative can change one's intuitions. If intuitions are theoretically constructed, if they are flexible and depend on background-variables, then this is a problem not only for the philosopher and his intuitions but also *for intuitions in general*.

Experimental philosophers track folk intuitions better than armchair reflection. Yet, many of them are stuck in a controversy: on the one hand they have vividly shown how untrustworthy intuition is. On the other, they depend all their theorising on the intuitions recorded. If intuition is unreliable, though, why does it make it better to rely on the intuitions of the many? A mistake is not less a mistake if made by many.

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The Indispensability Argument and Set Theory

The Quinean indispensability argument, as put by Mark Colyvan (2001: *The Indispensability of Mathematics*, Oxford University Press, p.1): "... mathematical entities are indispensable to our best physical theories and therefore share the ontological status of scientific entities."

Of course, one may take several different positions with respect to the ontological status of scientific entities such as, for example, quarks (quarks can't be observed even in principle). Do quarks "really exist", or are they only a (currently successful) theoretical construct used by physicists in their models? Perhaps, the "least committed" position could be the formalist one: let us define the "real existence" of some scientific entity as its invariance in future scientific theories. If quarks will be retained as a construct in our best future physical theories, then one may think of quarks as "really existing". Even from such a very formalistic point of view, the Quinean argument seems quite reasonable. Indeed, if some mathematical entity is indispensable to our best physical theories, then shouldn't we believe that this entity "exists" in the same sense as quarks are believed to exist?

However, imagine two mathematical entities E1 and E2, such that the existence of E1 contradicts the existence of E2. Can both such entities be indispensable to our best physical theories? As an example, let us consider two well-known versions of set theory:

ZFC i.e., ZF+AC, where ZF stands for Zermelo-Fraenkel axioms, and AC is the Axiom of Choice (see Thomas Jech 2006: *Set Theory*, Springer, Chapter 1);

ZF+AD where AD is the so-called Axiom of Determinacy (see Akihiro Kanamori 2003: *The Higher Infinite: Large Cardinals in Set Theory from Their Beginnings*, Springer, Chapter 6).

AD contradicts AC, hence, these theories cannot be used together. Currently, ZFC is almost generally acknowledged as the formal basis for theoretical mathematics. If ZF+AD would be used instead of ZFC, then we would have a slightly different theoretical mathematics. Worse, or better than the actual one? Who knows ... But: as a basis for the applied mathematics, ZFC and ZF+AD can be used equally well! All the mathematical inferences, currently necessary for physical theories, can be performed in ZF, i.e. in ZFC and in ZF+AD as well. Then, which of both set theories is indispensable to our best physical theories—ZFC, or ZF+AD?

May one believe that some of the proper ZFC inferences (i.e., inferences involving AC that can't be performed in ZF alone) could, some time in the future, be applied in physical theories? But so could proper ZF+AD inferences as well!

Would you say now that this is nothing new? That with the non-Euclidean geometries we have exactly the same situation: there are several geometries contradicting each other, but all of them are indispensable to our best physical theories? Indeed, the Euclidean geometry and non-Euclidean geometries are now become special cases of a more general theory that inspired Einstein's general relativity theory—the so-called Riemannian geometry (see Peter Petersen 2006: *Riemannian Geometry*, Springer).

And with set theories we have the same situation! As a set theory, ZF+AD is much more powerful than ZFC. According to a theorem proved by W. Hugh Woodin, ZF+AD can be "embedded" into a powerful extension of ZFC, obtained by adding one of the so-called large cardinal axioms ("There are infinitely many Woodin cardinals"), and conversely, this powerful extension of ZFC can be "embedded" into ZF+AD (see Kanamori 2003: Theorem 32.16). Should this mean, as in the case of non-Euclidean geometries, that set theories ZFC and ZF+AD are both indispensable to our best physical theories?

But then, how about the most fundamental mathematical entity—the famous unique "world of sets" to which we ought to have ontological commitment and that must be studied in set theory as the only structure worth of consideration? And in which the famous Continuum Hypothesis must be either true, or false, independently of the ability of human mathematicians to decide this? Which of the axioms—AC, or AD is true in this "world"? Most set theorists accept AC, and reject AD, i.e., for them, AC is true in the "world of sets", and AD is false. Applying to set theory the above-mentioned formalistic explanation of the existence of quarks, we could say: if, for a long time in the future, set theorists will continue their believing in AC, then one may think of a unique "world of sets" as existing in the same sense as quarks are believed to exist.

But, as we see, this is only a "light-weight" opinion that can't be justified by the Quinean indispensability argument! And, when it can't, then "what is the fuss about?" (as put by a prominent logician).

Karlis Podnieks
Computer Science, University of Latvia

§3
News

Amsterdam Graduate Philosophy Conference on Normativity, 29–30 August

In Amsterdam, the place for formal logic, Dora Achourioti, Edgar Andrade, and Marc Staudacher organised a Graduate Philosophy Conference on the theme of Normativity. The event took place on the 29th and 30th of August; it was the first of its kind in Amsterdam and turned out to be a success. The programme was dense: it consisted of twelve talks followed by prepared short commentaries, two keynote speeches, and a book launch. James O'Shea (University College Dublin) kindly offered to give both keynote talks, as the other keynote speaker, Allan Gibbard (University of Michigan), had to cancel his trip at short notice. O'Shea's very interesting talks related to his recent work on the philosophy of Wilfrid Sellars: "The Space of Reason is Janus-Faced: Sellars on Naturalism and Normativity" and "Normativity and Nonconceptual Representation: A Sellarsian Approach to Perceptual Experience".

The conference theme attracted young researchers from many countries around the world such as US, UK, Germany, and Spain. The talks related to various aspects of normativity, including normativity in metaethics, semantic normativity, and epistemic normativity. To give an idea of the variety, the topics ranged from 'Moral and rational "oughts": the distinction between "demanding" and "recommending" normativity' and "The Role of Normativity in Explaining Norm-conformity" to "Adequate Explanations and the Disjunction Problem" and "Assessment-Sensitivity and the Naturalistic Fallacy".

The conference was opened by Frank Veltman, currently the head of the Institute for Logic, Language, and Computation, and ended with a sparkling book launch. Martin Stokhof launched the book by Keith Stenning and Michiel van Lambalgen *Human Reasoning and Cognitive Science* (MIT Press, 2008). Van Lambalgen gave a short presentation of the book, and this together with champagne concluded the official part of the conference.

It was not only the scientific programme that was intense. On the social side, participants enjoyed group dinners at the end of each day and went on a boat trip through Amsterdam's canals. After the usual first day conference delays, the participants literally had to take a run and jump to catch the ILLC boat while on the move around the canals of the city. We regret not having pictures documenting this part of the conference.

With all that has happened, you may wonder what is still to come. Later this year, the online proceedings with the revised contributions will be made available here. Furthermore, a follow up conference for 2009 is in planning ...

Theodora Achourioti
ILLC, Amsterdam

Edgar Andrade
ILLC, Amsterdam

Marc Staudacher
ILLC, Amsterdam

European Conference on Machine Learning & Principles and Practice of Knowledge Discovery in Databases, 15–19 September

It is a long way from given masses of data to knowledge. For getting at least an overview of what is hidden in the terrabytes of a data base, Knowledge Discovery develops algorithms which detect frequent subsets of items that might be of interest for a user. For a certain measure of interestingness, the set of all item subsets that are interesting can be represented in a condensed form. At ECML/PKDD 2008, held in Antwerp, a generalization of interestingness measures with respect to condensed representations was presented by Arnaud Soulet and Bruno Crémilleux. While this works fine for transaction databases, there are other types of data asking for other algorithms, e.g., string databases used in genetic research, data streams stemming from measured dynamic processes like mobile devices, or sequences of events. The PKDD best paper award went to Apostolos Papadopoulos, Apostolos Lyritsis, and Yannis Manolopoulos for their mining frequent graphs, that are not dominated by other subgraphs thus forming a "skyline" of the database.

Machine Learning covers a variety of learning tasks which algorithms are to fulfill, the most popular being that of classifier learning: given a set of training instances, where the class label is known, learn a function that classifies new instances correctly. One optimality condition for the decision function is that it separates the instances of two classes by a maximal margin between them (introduced with the Support Vector Machine, SVM). Markus Weimer, Alexandros Karatzoglo, and Alex Smola proposed a new factorization method in order to solve the optimization problem for recommender systems and won the ECML best paper award for it. Recommender systems attracted quite some attention at the conference. Amazon's "people who bought X also bought Y" was extended in several ways.

The invited talks of Anil Jain about clustering, Ray Mooney about learning language from perception, Francoise Fogelman-Souli about industrialisation of data mining, Raghu Ramakrishnan about exploratory data mining, and Yoav Freund about machine learning supporting biology have been recorded. In addition to the regular papers, which were presented in the auditorium and also at poster sessions in order to allow for indepth discussions, workshops and tutorials completed the program.

These few instances of Knowledge Discovery and Machine Learning results might shed a light on what is driving researchers who have submitted their papers to ECML/PKDD (521 papers were reviewed) and researchers who came to listen to the talks (more than 450 participants). For the first time, the best papers of the conference were published in the Data Mining and Knowledge Discovery Journal and the Machine Learning Journal, right on time for the conference. These special issues offer an opportunity to those who cannot find the time to participate in the conference or read the overall proceedings with 61 papers.

Walter Daelemans
Linguistics, Antwerp

Bart Goethals
Mathematics and Computer Science, Antwerp

Katharina Morik
Computer Science, Dortmund

European Workshop on Probabilistic Graphical Models, 17–19 September

The fourth European Workshop on Probabilistic Graphical Models (PGM) has recently taken place in Hirtshals, Denmark, on 17–19 September.

The workshop series started in 2002, and continues to attract most of the European research groups on probabilistic graphical models as well as other non-European researchers in this area. A distinguishing characteristic of the PGM workshops is the large number of regular participants, which contributes to an informal atmosphere, almost with a touch of a "family reunion".

Like the previous years, the aim of this years workshop was to bring together people interested in probabilistic graphical models and provide a forum for discussion of the latest research developments in this field. The workshop was organized so as to facilitate discussions and collaboration among the participants also outside the workshop sessions.

There were 39 papers presented at the workshop, out of which 20 papers appeared as plenary presentations and 19 papers appeared as posters. Following the PGM tradition, the majority of the papers dealt with core aspects of Bayesian networks and decision graphs, with learning and inference being the most prevalent topics. This year there were 14 papers devoted to aspects of Bayesian network learning, ranging from a geometric perspective on structure learning to efficient score function calculation and parameter estimation. The 10 papers on inference primarily concerned (approximate) inference in influence diagrams (and variants hereof) as well as belief propagation algorithms, in particular loopy belief propagation and junction tree propagation.

In addition to the more established model types, we also saw several papers focusing on other (more recently proposed) probabilistic frameworks. For these model classes, learning was also a focus area and covered both probabilistic decision graphs as well as dependency networks (the latter with emphasis on classification). Other types of modeling frameworks were also considered. For example arithmetic circuits for encoding noisy-or models, dedicated languages for modeling and solving multi-agent systems, and loopy belief propagation algorithms for credal networks.

As a followup on the workshop, there will be a special PGM issue of the International Journal of Approximate Reasoning, where authors of selected papers have been invited to submit extended versions of their workshop papers.

On the social side of the program, the participants got acquainted with the sea-life in the North Sea (through a visit to the North Sea Aquarium) as well as the nightlife in Hirtshals, which was found to consist of three bars and two pool tables.

The next PGM workshop will take place in Helsinki, Finland, in 2010.

Manfred Jaeger
Computer science, Aalborg

Thomas D. Nielsen
Computer science, Aalborg

New Directions in Philosophy of Mathematics, 4 October

To celebrate the founding of Manchester Institute for Mathematical Sciences (MIMS), the mathematics department of the recently unified Manchester University, it was proposed that various workshops named 'New Directions in ...' be run. They kindly agreed to allow Alexandre Borovik and me to organise one of these workshops on the Philosophy of Mathematics.

So, on Saturday 4 October, we began with Mary Leng, a philosopher at Liverpool, talking about whether the creation of mathematical theories, e.g., Hamilton's quaternions, gives us any more reason to think mathematical entities exist than does the discovery of new consequences within existing theories. She concluded that it does not—both concern the drawing of consequences from suppositions, e.g., "Were there to be a 3 or 4-dimensional number system sharing specified properties with the complex numbers, then ...".

George Joseph, author of the 'Crest of the Peacock', then told us about sophisticated work in 16th century Kerala concerning expansions of trigonometric functions, and better proved equivalents to the protocalculus of Wallis. He then moved on to China where in search of the evenly tempered scale the mathematician Zhu calculated the value of $2^{1/12}$ to an extraordinary number of decimal places. Discussion centred around the question of why we continue to ignore non-Western roots of modern mathematics.

Marcus Giaquinto, a philosopher from University College London, talked about visual intuition and proof. He explained how a great deal of care is needed in assuring ourselves of the validity of a geometric demonstration. This follows up on the work of Ken Manders (Pittsburgh) to understand when Euclidean diagrammatic reasoning is valid.

Angus MacIntyre, a model theorist at Queen Mary College London, told us about the limited interest for mainstream mathematics of incompleteness results. He argued that the kind of Diophantine equations appearing in incompleteness results are not of the kind number theorists deal with, and he explained to us how he is trying to show that Wiles' proof of Fermat's Last Theorem can be written in first-order Peano arithmetic.

I finished off the talks by speaking about my paper Lautman and the Reality of Mathematics. This argues that philosophy should study mathematics less as (potentially) concerning abstract entities, but rather as concerning the development of certain ideas. While Lautman pointed us to some excellent examples of this phenomenon, the Galoisian idea and the idea of duality, it seems less clear to me that they are situated somehow superior to mathematics as he believed.

We ended with a brief general discussion, which included a deliberately provocative comment from the philosopher John Kennedy that the majority of mathematicians dont think hard enough about basic concepts of identity and relation, and that this marked an impoverishment of mathematics since the days of Hilbert.

David Corfield
Philosophy, Kent

Calls for Papers

Humana.mente: Volume 8, Models of Time, deadline 15 November.

Sir Karl Popper Essay Prize: British Society for the Philosophy of Science, deadline 31 December.

Reasoning for change: Special issue of the journal *Informal Logic*, deadline 10 February.

Just Reason: Special issue of the journal *Studies in Social Justice*, 1 April.

Experimental Philosophy: Forthcoming issue of *The Monist*, deadline April 2011.

§4

Introducing …

In this section we introduce a selection of key terms, texts and authors connected with reasoning. Entries will be collected in a volume *Key Terms in Logic*, to be published by Continuum. If you would like to contribute, please click here for more information. If you have feedback concerning any of the items printed here, please email thereasoner@kent.ac.uk with your comments.

Inference to the Best Explanation

A method of reasoning, also known as abduction, in which the truth of an hypothesis is inferred on the grounds that it provides the best explanation of the relevant evidence. In general, inference to the best explanation (IBE) is an ampliative (i.e. nondeductive) method. In cases where a is not only the best explanation of b but a also entails b then IBE is formally equivalent to the logical fallacy of affirming the consequent. However IBE does not license inferring a merely on the basis of the fact that a entails b. Criticisms of IBE come in both local and global varieties. Locally, such inferences are always defeasible because one can never be sure that all potential explanations have been found and hence that there is not some better, hitherto undiscovered explanation of the given evidence. Globally, some philosophers have questioned the grounds for taking explanatoriness as a guide to truth in the first place. There is also the practical issue of determining criteria for the comparison of different explanations, perhaps borrowing from more general criteria of theory choice such as simplicity, fruitfulness, expressive power, and so on. There has been a tendency to see IBE as a distinctive feature of the empirical sciences. However, there are reasons for thinking that IBE may also play a role in the formal sciences, including both logic and mathematics, when it comes to choosing axioms. Thus a rationale for favoring one particular set of axioms may be that it provides the best explanation of the core results in the theory under scrutiny.

Alan Baker
Philosophy, Swarthmore

The Indiscernibility of Identicals

The indiscernibility of identicals is formally rendered thus:

$$(\forall x)(\forall y)(x = y \supset (Fx \equiv Fy))$$

Informally, it is the claim that if, say, Jane is identical to Nancy, then whatever is true of Jane is true of Nancy and vice versa. This seems obvious: if Jane and Nancy are not two different people but one person (who goes by two names) then it seems impossible for Jane to have a property Nancy lacks. Even so, there are some troublesome consequences of this principle. Here is one. Assume that Lois is unaware that mild-mannered Clark is actually flying superhero Superman. Accordingly, Lois believes (and will assert) that Superman can fly but Clark cannot. It seems to follow that Superman has a property that Clark lacks, viz. the property 'being thought by Lois to be able to fly.' But this contradicts the principle of the indiscernibility of identicals. If we accept that principle we must say either that Lois believes Clark can fly (something she will deny) or claim that a property like 'being thought by Lois to be able to fly' is, perhaps because it concerns the propositional attitudes, somehow not a genuine property. Neither option is free from problems of its own.

Andrew P. Mills
Otterbein College

§5

Events

November

Peter Lipton Memorial Conference: Department of History and Philosophy of Science, Cambridge, 1 November.

LNAT: Logic Now and Then, The Center for Research in Syntax, Semantics and Phonology (CRISSP), Brussels, 5–7 November.

AUTOMATED SCIENTIFIC DISCOVERY: AAAI Fall Symposium, Arlington, Virginia, 7–9 November.

MWPMW 9: Ninth annual Midwest PhilMath Workshop, 8–9 November.

WPE: Workshop on Philosophy and Engineering, The Royal Academy of Engineering, Carlton House Terrace, London, 10–12 November.

LEBENSWELT AND LOGIC: The Erlangen school as heir to logical empiricism, Nancy, France, 13–14 November.

NATURE AND STRUCTURE: Philosophy of Physics Graduate Student Conference, SUNY at Buffalo, 15 November.

PROPOSITIONS: ONTOLOGY, SEMANTICS, AND PRAGMATICS: Venice, Italy, 17–19 November.

PHYSICS MEETS BIOLOGY: Perspectives from Philosophy, History, and Science, Royal Society of Edinburgh, 18–20 November.

GAME THEORY: 5th Pan-Pacific Conference in Game Theory, Auckland, 19–21 November.

NEW DIRECTIONS IN EPISTEMOLOGY: International Symposium, Canadian Society for Epistemology, Carleton University, Ottawa, Canada, 21–22 November.

KEAPPA WORKSHOP: Knowledge Exchange: Automated Provers and Proof Assistants, Doha, Qatar, 22 November.

LPAR: 15th International Conference on Logic for Programming, Artificial Intelligence and Reasoning, Carnegie Mellon University, 23–27 November.

DECEMBER

INFERENCE, CONSEQUENCE, AND MEANING: Sofia, 3–4 December.

ICLP: 24th International Conference on Logic Programming, Udine, Italy, 9–13 December.

CAUSALITY: OBJECTIVES AND ASSESSMENT: Whistler Resort & Spa and Westin Hilton, BC, CANADA, 12 December.

CIMCA: International Conference on Computational Intelligence for Modelling, Control and Automation, Vienna, Austria, 10–12 December.

TRENDS IN LOGIC VI: Logic and the foundations of physics: space, time and quanta, Brussels, Belgium, 11–12 December.

ICDM: 8th IEEE International Conference on Data Mining, Pisa, 15–19 December.

PRICAI: Tenth Pacific Rim International Conference on Artificial Intelligence, Hanoi, Vietnam, 15–19 December.

JANUARY 2009

LFCS: Symposium on logical foundations of computer science, Deerfield Beach, Florida, 3–6 January.

SODA: ACM-SIAM Symposium on Discrete Algorithms, New York Marriott Downtown, 4–6 January.

BIOMOLECULAR NETWORKS: from analysis to synthesis, Pacific Symposium on Biocomputing, Fairmont Orchid, The Big Island of Hawaii, 5–9 January.

3RD INDIAN CONFERENCE ON LOGIC AND ITS APPLICATION: The Institute of Mathematical Sciences, Chennai, India, 7–11 January.

GRADUATE CONFERENCE: Second Cambridge Graduate Conference on the Philosophy of Logic and Mathematics, 17–18 January.

VAF: 3th Conference of Dutch Flemisch Association for Analytical Philosophy, Tilburg University, the Netherlands, 22–23 January.

BAYESIAN BIOSTATISTICS: Houston, Texas, 26–28 January.

VERY INFORMAL GATHERING OF LOGICIANS: UCLA Logic Center, 30 January–1 February.

FEBRUARY

ACM INTERNATIONAL CONFERENCE ON INTELLIGENT USER INTERFACES: Sanibel Island, Florida, 8–11 February.

AIA: IASTED International Conference on Artificial Intelligence and Applications, Innsbruck, Austria, 16–18 February.

CICLING + LEXICOM: 10th International Conference on Intelligent Text Processing and Computational Linguistics; pre-conf event: Lexicom-Americas workshop, 24–28 February.

INTERONTOLOGY: 2nd Interdisciplinary Ontology Conference Tokyo, Japan, 27 February–1 March.

MARCH

MODELS AND SIMULATIONS 3: Charlottesville, Virginia, 3–5 March.

&HPS2: Integrated History and Philosophy of Science, University of Notre Dame, 12–15 March.

ADS: Agent-Directed Simulation Symposium, Part of the Spring Simulation Multiconference, San Diego, California, 22–27 March.

EVIDENCE, SCIENCE AND PUBLIC POLICY: Sydney Centre for the Foundations of Science, 26–28 March.

CSIE: World Congress on Computer Science and Information Engineering, Los Angeles/Anaheim, 31 March–2 April.

APRIL

FOUNDATIONS OF MATH: New York University, 3–5 April.

EUROGP: 12th European Conference on Genetic Programming, Tübingen, Germany, 15–17 April.

AISTATS: Twelfth International Conference on Artificial Intelligence and Statistics, Clearwater, Florida, 16–19 April.

ESANN: 17th European Symposium on Artificial Neural Networks Advances in Computational Intelligence and Learning, Bruges (Belgium), 22–24 April.

May

Logic of John Duns Scotus: 44th International Congress on Medieval Studies at Western Michigan University, 7–10 May.

AAMAS: The Eighth International Joint Conference on Autonomous Agents and Multi-Agent Systems, Budapest, Hungary, 11–15 May.

Philosophy and Cognitive Science: The XIXth edition of the Inter-University Workshop, Zaragoza, 18–19 May.

UR: Uncertain Reasoning, Special Track of FLAIRS, Island, Florida, USA, 19–21 May.

AI: The twenty-second Canadian Conference on Artificial Intelligence, Kelowna, British Columbia, 25–27 May.

June

Argument Cultures: Ontario Society for the Study of Argumentation, Windsor, Canada, 3–6 June.

CNL: orkshop on Controlled Natural Languages, Marettimo Island, Sicily, 8–10 June.

NA-CAP: Networks and Their Philosophical Implications, Indiana University in Bloomington, 14–16 June.

NAFIPS: 28th North American Fuzzy Information Processing Society Annual Conference, University of Cincinnati, Cincinnati, Ohio, 14–17 June.

ICML: The 26th International Conference On Machine Learning, Montreal, Canada, 14–18 June.

WoLLIC: 16th Workshop on Logic, Language, Information and Computation, Tokyo, Japan, 21–24 June.

July

Two Streams in the Philosophy of Mathematics: Rival Conceptions of Mathematical Proof, University of Hertfordshire, Hatfield, UK, 1–3 July.

Metaphysics of Science: University of Melbourne, 2–5 July.

SPT: Converging Technologies, Changing Societies, 16th International Conference of the Society for Philosophy and Technology, University of Twente, Enschede, The Netherlands, 8–10 July.

ISHPSSB: International Society for the History, Philosophy, and Social Studies of Biology, Emmanuel College, St. Lucia, Brisbane, Australia, 12–16 July.

Logic and Heresy in the Middle Ages: Leeds Medieval Congress, 13–16 July.

August

Meaning, Understanding and Knowledge: 5th International Symposium of Cognition, Logic and Communication, Riga, Latvia, 7–9 August.

Practice-based Philosophy of Logic and Mathematics: ILLC, Amsterdam, 31 August–2 September.

September

Mechanisms and Causality in the Sciences
University of Kent, Canterbury, UK, 9–11 September

MoS: Grand Finale Conference of the Metaphysics of Science AHRC Project, Nottingham, 14–16 September.

ISMIS: The Eighteenth International Symposium on Methodologies for Intelligent Systems, University of Economics, Prague, Czech Republic, 14–17 September.

October

EPSA: 2nd Conference of the European Philosophy of Science Association, 21–24 October.

§6
Jobs

Professor of Stochastics: Radboud University Nijmegen, Faculty of Science, 3 November.

Three-year Fixed Term Lectureship: Philosophy Department, Stirling, 3 November.

The Ludwig Lachmann Research Fellowship: Department of Philosophy, Logic and Scientific Method London School of Economics and Political Science, 7 November.

Research Fellow: Faculty Of Philosophy, University of Oxford, 7 November.

Lecturer in Philosophy of Science and Medicine: UCL, Department of Science & Technology Studies, University College London, 17 November.

Independent New Investigator: Onna-son or Urumashi, Okinawa, Japan, applications open 17 November, until position is filled.

Assistant Professor in Logic: Department of philosophy, University of Calgary, 21 November.

Lectureship in Philosophy: University of Leeds Faculty of Arts, Department of Philosophy, 28 November.

5 Research Positions: University of Konstanz, 30 November.

Assistant Professor: Institute of Cognitive Science at Carleton University, 1 December.

Assistant Professor: Philosophy of social sciences, Université du Québec à Montréal, Montréal, Canada, 1 December.

LECTURER: Philosophy / Critical Thinking / Informal Logic, Department of Philosophy, University of Auckland, New Zealand, 5 January.

§7
COURSES AND STUDENTSHIPS

Courses

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

MA IN REASONING
An interdisciplinary programme at the University of Kent, Canterbury, UK. Core modules on logical, causal, probabilistic, scientific, mathematical and machine reasoning and further modules from Philosophy, Psychology, Computing, Statistics, Social Policy and Law.

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

MIND AS MACHINE: Department for Continuing Education, University of Oxford, 1–2 November.

HEALTH IN CONTEXT: A short course in multilevel modelling for public health and health services research, Universidade Nova de Lisboa, Lisbon, Portugal, 10–14 November.

PHILOSOPHY OF PSYCHOLOGY: Bochum / Tilburg, First European Graduate School, Philosophy of Language, Mind and Science, 10–21 November.

SUMMER INSTITUTE ON ARGUMENTATION: University of Windsor, Canada, contact H.V. Hansen or C.W. Tindale, 25 May – 6 June 2009.