The Reasoner

Volume 2, Number 2
February 2008

www.thereasoner.org

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§1 Editorial

It is truly a pleasure to be the guest-editor of this issue of *The Reasoner*. I wish to thank Jon Williamson for taking care of all the practical matters. In fact, my only contribution was to write this editorial and to conduct an interview.

I decided to interview Dov Gabbay because human reasoning has always been the focus of his research interests, spanning from logic to philosophy, and from computer science to economics.

I had the privilege to meet Dov when I was a visiting scholar at King's College London, at the end of my doctoral studies. I recall that the first time I met him I was very nervous. I was acquainted with his seminal works on logical consequence relations, and I had the vivid image of the several shelves full of his publications in my university library. Instead of the serious and distant professor I was expecting to meet, it was a smiling and charming man I was introduced to. I was immediately struck by his passion, enthusiasm and curiosity for anything related to logic and human reasoning.

The relation between logic and human reasoning is also the topic of the conversation we had and that I am presenting to the readers of *The Reasoner*. I hope that you will enjoy reading about Dov's vision of what logic is. Above all, I hope you will be captured by his enthusiasm, as I have been since my first encounter with him.

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§2 Features

Interview with Dov Gabbay

Dov Gabbay is the Augustus De Morgan Professor of Logic in the Computer Science Department and Visiting Professor in the Philosophy Department at King's College London.

GP: Can you tell our readers about your intellectual history and how you came to work in logic?

DG: I was interested in logic since I was a teenager. I studied at a religious school, so we read the Bible, and in the Bible there is Moses who argues with God. It always struck me that you can actually stand up to God, if you have a good argument. The story is like this: God wanted Moses to save the children of Israel. God came to him in the burning bush and Moses was arguing with God saying 'I dont want to do this job', which is rather unusual. There was also the argument between Abraham and God. God wanted to destroy Sodom and Gomorrah and Abraham pleaded with God not to destroy it, and said: 'You can't do this. Maybe there are fifty good people there'. God replied: 'Ok. If there are fifty good people there, I will not destroy it'. And then Abraham said again: 'Well, you agreed on fifty, what about forty-five people?' The point is that Abraham argued with God and brought God down to ten. God then looked at Sodom and Gomorrah and there was one guy, called Lot, who was a dubious case. So God did not know what to do, given the spirit of the argument, and so God said to Lot: 'Why don't you leave the town?' This is data-driven action! Logic is very strong in the Bible.

When I went to the university to study mathematics, I specialised in logic for my Master and for my PhD. I saw logic in any kind of human argument and action. I also studied game theory. I joined the game group at the Hebrew University of Jerusalem, with Robert Aumann. Logic has always been my main interest. In fact, I am collecting arguments against the Almighty, because he is a very controversial figure. I think that he might postpone calling me to heaven, just not to have the confrontation.

As I said many times, the assumption is that the Almighty has this big lump of logic which is sprinkled in our minds. My job is to find these pieces of logic and get an idea of what the big lump was. Also for those who do not believe in creation, they see that it is all coherent. The various areas of logic and reasoning, action, social choice theory, whatever it has to do with us and how we behave, is a coherent part of something. There are connections and we can learn from one part or another, and we can model it. Big lump or not, the coherence between all these parts is the assumption.

GP: You are the Augustus De Morgan Professor of Logic at King's College London. What does this mean to you?

DG: I wanted this because I think he had an open mind. In a sense, he was a revolutionary. He did many things that are still good today. In fact I got all De Morgan's books. He stuck to his principles. He even resigned from London University for his principles!

GP: You have been working in logic for forty years. How did the field evolve in this span of time? For example, how did computer science change the way logic is done?

DG: Aristotle realised that, in order to write all his books, he needed to start with logic. In fact *Organon* means 'the tool'. There is a human-oriented reasoning in his books. He started by looking at the humans and, all the way through the Middle Age, they were still looking at human reasoning. It is only in the nineteenth century that logic changed direction and looked into mathematics and science. In a way it was a sidetrack, although a very important one. It came back to the mainstream of modelling human reasoning when it became apparent that you need logic in computer science, and computer science wants to do devices for the humans. It went then into language, philosophy and into the computational aspects of logic and of modelling the human reasoning. The big impact of computer science and the applied sciences is the urgency of modelling humans because you want to sell them your products. You want to either replace the humans, or help them with various programs and devices. Therefore, you need to model them. So computer science is a big push back onto track to dealing with the humans and it is accelerated because of the business' aspects involved in it. It is a good thing for logic. I can say that after forty years of working in logic, only now I begin to understand what logic is about, and I feel now ready to write a book on Principia Logica Practica. We are very complex in our reasoning. It is even more complicated and challenging than studying physics or biology.

GP: It seems that you are confident that we will have a logic that captures the human reasoning.

DG: Yes! But it is not a single logic. It is not like some people in physics who want a unified big theory of physics. I do not think it is like this. I think we have various logical principles, we have a variety of logics, and these are interconnected. The different logics act as a committee. When you face a practical issue, one or some of these sub-committees will deal with the issue and communicate with all the others. It would not bother me to apply this point of view, for example, in physics. We could have the quantum world, the general relativistic world: as long as I can move from one world to the other, I am not bothered by what Einstein said: 'Does God do his own things on Monday, Wednesday, Friday, and the other things on Tuesday etc.?' The principles that make the different logics working as a whole are fibring logics, the object level, meta-level, various labels depending on the context. It is like a committee of logics, where the chairman chooses one sub-committee according to the needs. You have this in reality. There are companies of accountancy, companies of lawyers and, if you go to them (depending on whether it is a divorce or a murder), you have one of them dealing with you, but all the others are supporting.

GP: How do you think the field will evolve in the near future?

DG: There are traditional groups whose people believe that there is only recursion theory, model theory, set theory, proof theory etc. Some of this core will remain. The mathematical logicians will remain. But I think that the communities of people working on applied logics, algorithmic logics, etc. will be the predominant ones, because they are getting organised. Let's hope that they will talk to each other. My view is that you do not just do research, but you also have to do organisation. That is why I do all these handbooks, journals, and I have a charity, the International Federation of Computational Logic. I think it is important to organize the field and push it forward. Hopefully there will be more communication with other areas dealing with humans, like game theory and social choice theory. What unite them are the general principles, like object level, meta-level, revision, time, action, probability, etc. Now they are separate communities but they will interact more.

GP: Does the way you think the field will evolve correspond to the way you would like the field to evolve?

DG: The different communities I mentioned before will communicate more to each other. But you can accelerate the process. For example, it can take ten years to a PhD student to find the connections between voting theory and belief revision, or you can go ahead and organize a conference on it! Sooner or later the communities will talk to each other. It is like a boy and a girl on a trip. They are very compatible and they like each other. Sooner or later something will happen, but you can accelerate it by putting them together in the same room the first night of the holiday. One way or the other, it will happen.

GP: Dov, let me make a scoop for The Reasoner! Tell us your secret. How do you manage to write so many books?

DG: Sure, I can tell you! I can do two or three things at the same time. For example, while I am talking to you, I am also thinking of some proofs. I can do two or three things at the same time, but not when I am in a bank, or when I drive, because there is a risk. If now my attention gets distracted because I am thinking about the proof, you would correct me. But in a bank it is too dangerous, and the same applies when I drive. I also cannot think more than one thing when I am checking in at the airport. I do not want my suitcase to go to Timbuctu. Moreover, writing a book is not like lining bricks: give me another hour and I will put ten more bricks. You write a chapter and then you have to sit for three months thinking about the next one. But, in the meantime, you can write a chapter for another book. So you can write two or three books at the same time. I am also multi-tasking in the same day. I think about research also when I sleep. I do not mind cleaning toilets and doing the washing up, because it is automatic, so I think of something else in the meantime. And, of course, it is very important to collaborate with other researchers. It is good for stimulation. I owe my co-authors and colleagues a lot and also Mrs Jane Spurr (known in the community as SuperJane). I could not have done it without them. It is not like a budget: if someone else gets part of it, there is less for you. We are working against an infinite amount of budget. Everybody can get a Nobel prize. The more you work, the more there is!

Socio-empirical epistemology of mathematics

Starting with a very schematic picture of the iterative process of developing a philosophical theory of *X*, one may describe the process of providing a philosophical analysis of the concept of mathematical knowledge as follows: In a *first step of theory formation*, a structural philosophical account of the concept of mathematical knowledge is developed. In a *second step devoted to phenomenology*, data is collected to either corroborate or question the current philosophical theory. In a *third step of reflection*, the adequacy of the current philosophical theory is assessed, and the theory may be modified or even revised by reverting to step 1.

As one can observe in traditional philosophy of mathematics, step 2 usually consists in introspective investigations about the philosopher's use of the concept of mathematical knowledge, based on his or her intuition. One will hardly find any empirical material broadening the scope of data sources for step 2 in the above scheme.

Nevertheless, a number of present day philosophers of mathematics, following some more or less Lakatosian line of thought, claim that philosophy of mathematics should shift its research focus towards actual mathematical practice. Besides the works of Kitcher, Bloor, and Ernest, see for example Maddy (1997: *Naturalism in Mathematics*, Oxford: Clarendon Press), Larvor (2001: "What is Dialectical Philosophy of Mathematics?", *Philosophia Mathematica* (1) Vol. 9, 212–229), Leng (2002: "Phenomenology and Mathematical Practice", *Philosophia Mathematica* (3) Vol. 10, 3–25), Corfield (2003: *Towards a Philosophy of Real Mathematics*, Cambridge University Press), Corfield (2005: Review of Martin Krieger's *Doing Mathematics*, *Philosophia Mathematica* (1) Vol. 13, 106–111), Hersh (2005: Introduction to *18 Unconventional Essays on the Nature of Mathematics*, ed. by R. Hersh, Springer, vii–xv), or Löwe/Müller (2008: "Mathematical Knowledge is Context-Dependent", *Grazer Philosophische Studien* 76).

Still, at present there are only few philosophical studies exploiting "hard" empirical data on *current* mathematical practice, e.g., from psychology, cognitive science, didactics, or sociology. This holds especially for *socio-empirical* data. A few examples in that field are Markowitsch (1997: *Metaphysik und Mathematik. Über implizites Wissen, Verstehen und die Praxis der Mathematik*, unpublished PhD thesis), Corfield's blogs Philosophy of Real Mathematics and The n-category café, and Leng (2002).

The situation for socio-empirical studies is even worse—(esp. quantitative) data on mathematical practice appears to be hardly available *at all*. A few examples of genuine (qualitative) sociological studies are Maaß (1985: *Mathematik als soziales System*, unpublished PhD thesis), Heintz (2000: *Die Innenwelt der Mathematik: Zur Kultur und Praxis einer beweisenden Disziplin*, Springer), and MacKenzie (2001: *Mechanizing Proof: Computing, Risk, and Trust*, MIT Press).

We have observed that philosophical considerations about the concept of mathematical knowledge are hardly connected with empirical facts about current mathematical practice. I'd like to stress that this is not a state that we should remain in as philosophers of mathematics. As a special case, I want to emphasize the relevance of a socio-empirical approach in epistemology of mathematics.

Of course, employing socio-empirical methods in step 2 of our philosophical enterprise may turn out to be quite intricate. Firstly, as mentioned above, there are only *few already existing socio-empirical results* on mathematical practice. This fact even seems to be caused by certain hidden philosophical attitudes towards the nature of mathematics (cf., Heintz (2000: 17ff), MacKenzie (2001: 2ff)). Secondly, *philosophical questions do not have simple sociological answers* that can be read right off from socio-empirical results. Socio-empirical epistemology of mathematics faces the serious (and quite general) challenge of the need for interdisciplinary fine-tuning of its methodological and conceptual framework. Thirdly, the *question of normativity*, which shall not be abandoned by employing socio-empirical results in epistemology of mathematics, has to be carefully reconsidered at some stage.

Nevertheless, understanding the concept of mathematical knowledge as intimately linked to mathematical practice *requires* a certain convergence of philosophical theory and empirical data on that practice, with socio-empirical data as a special case. At least, the data may well serve as a starting point for developing an epistemology of mathematics. For this purpose, it does in particular not seem to matter whether the empirical methods employed are "inside-phenomenological" or "outside-observer" (cf. Larvor (2001: 213)). It remains an independent question as to which *conclusions* for the resulting philosophical theory will be drawn from the empirical findings. The answer to this question will also depend on the empirical outcomes themselves.

To give an impression of how socio-empirical research in the epistemology of mathematics would look like, I would like to give an example from my own recent work.

One possible topic for a philosophically relevant socio-empirical investigation of mathematical practice is the concept of *formalizability*. At least throughout philosophy of mathematics after Frege and Hilbert, formalizability of informal mathematical proofs became an important issue. Yet, formal and formalizable proofs play only a minor role in actual mathematical practice. Thus the question arises:

> What is the role of formalizability for a philosophical understanding of mathematical knowledge?

I started working on this particular topic last year. As a first step, I conducted a mainly quantitative online survey among about 80 working mathematicians to find out about their abstract concept of knowledge and the conditions for their positive knowledge ascriptions. A cluster analysis of the data emphasizes that the formalizability of an informal mathematical proof of some theorem p, given by some working mathematician, is in an *absolute* sense neither necessary nor sufficient for ascribing knowledge of p to him or her. Rather, the concept of mathematical knowledge employed in mathematical practice seems to be highly context sensitive, and closely linked to the notion of mathematical skills. The results shall be sharpened by a follow-up qualitative interview series.

For more details on this study see Wilhelmus (2007: "Formalizability and knowledge ascriptions in mathematical practice", *ILLC Publications*, PP-2007-24) and Löwe/Müller/Wilhelmus (2007: "Mathematical knowledge: a case study in empirical philosophy of mathematics", *ILLC Publications*, PP-2007-32). See the project homepage for first results from the cluster analysis.

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When is a statement not a statement?–When it's a Liar

Curry-Löb

In Alex Blum (2007:'On the Curry-Löb Paradox', *The Reasoner* 1:7, p. 6) it was claimed that a version of the Curry-Löb paradox-producing-expression, namely:

(A) If (S) is true then p

which purports to demonstrate that p is true no matter what, does not really produce paradox, for (S) is just necessarily false.

Laurence Goldstein pointed out in correspondence that there are versions of the Curry-Löb paradox which are not amenable to the solution provided above. An example would be the expression

- Either this statement is false or pigs can fly

This can't be true because that would mean that at least one disjunct is true. But the second disjunct is obviously false, and if the first disjunct were true, the disjunctive statement would be false. But that statement can't be false either, because, if it were, its first disjunct would be true, rendering the statement as a whole true.

Likewise, in replacing 'p' in (S) by a particular false statement namely 'Pigs can fly' we undermine the contention that (S) is necessarily false. If it were false, its antecedent would have to be true, but its antecedent is just......it! In fact, the Curry-Löb belongs in the Liar family, and is most strikingly similar to the Epimenidean version of the Liar—see Laurence Goldstein (1986: 'Epimenides and Curry', *Analysis* 46:3, pp.117-121.)

Anatomy of a Paradox

Let's suppose that 'L' is the name of the statement 'L is not true' (so the quote marks here are being used to point to a statement). Then

1. If L is true then L is not true, and hence L is not true

And, by similar reasoning,

2. If L is not true then L is true, and hence L is true.

Consequently, on the assumption that L is either true or not true

3. L is both true and not true.

Or,

4. L is true if and only if L is not true.

And that is impossible, for (*pace Dialetheism)* no statement can be both true and not true; no statement can be true if and only if it is not true.

We were led from true to not true and then from not true to true. One or two further steps lead to the conclusion that L *cannot* be true, and further, that L *cannot* be untrue as well. We reach these arresting conclusions, perhaps most glaringly, by seeing that *each* of L being true or untrue implies a *contradiction*, thus: Given 1.,

i.'L is true' implies ('L is true and L is not true')

And, given 2.,

ii. 'L is not true' implies ('L is not true and L is true').

Hence L *can* neither be true nor untrue. Consideration of L reveals more. For since L is necessarily untrue, '¬L' is necessarily true; and, since 'L is not true' is necessarily untrue, L is necessarily true. So we have not only that L is both true and untrue but also that it is *necessarily* both, as is its negation.

Sentences and Non-Statements

All of these unpalatable conclusions flowed from the assumption that L is either true or not true. But L and its indexical counterpart 'This statement is false' are constructed from perfectly grammatical *sentences*. We know that sentences—grammatically correct strings of words ending in a full stop—are not themselves true or false, but typically a sentence is used to say something, and it is what is said (i.e., the statement made) that has truth-value. So we can avoid all the unpalatable conclusions if we acknowledge that there is a sentence, the Liar sentence, that is grammatically correct and meaningful (we can, after all, understand it and translate it into French) but deny that it can be used to make a statement. This denial can be supported by a simple argument:

Suppose that L is a statement, i.e., it says something true or false. Obviously, L has a truth-value different from that of the statement 'L is not true'. Therefore there can be no question of *identifying* L and 'L is not true'. In other words, we cannot *let* L be the statement that L is not true. Now, 'L' was a letter arbitrarily chosen—any letter would have done. So the general lesson is: Nothing can say of itself that it is not true.

But the *only* thing that L could be plausibly understood as saying is that it is not true.

So L fails to say anything—in other words, it is not a statement. The same can be said, of course, of the Curry-Löb sentence.

Up to this point, we have been rather casual about the distinction between 'false' and 'not true'. We could regard predicating 'not true' of X as denying that X has the property of being true. So each of the following is, in this sense, not true

Chalk

The number 9

The statement that pigs can fly

The sentence 'Pigs cannot fly'.

Now consider these attempted 'strengthening's of the Liar:

SL1: The sentence used to make this statement is not true.

We can say, straight off, that the statement SL1 is true, because, as we have pointed out above, no *sentence* is true. What about

SL2: The sentence used to make this statement is false. ?

That's a category mistake, about which different philosophical views are possible, none of them leading to paradox. One view is that there is a statement SL2 that is straightforwardly false; another is that the displayed sentence fails to make a statement. Next

SL3: This statement is not true.

We have argued that the sentence here on display fails to make a statement and so SL3 fails to *say* (truly or falsely) anything, and hence, *a fortiori* to say of any statement that it is not true. Likewise

SL4: This statement is false.

No statement is made, hence *a fortiori*, no statement is made to the effect that it itself is false.

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The Mathematical Theory of Groups and Social Measurements

Any act of measurement necessarily involves a comparison between that which is to be measured and some standard of measurement. No simple act of comparison by itself can ever be enough; the comparison must be with some object or structure that defines the unit of measurement involved. That standard must be something that maintains its uniformity across the relevant changes necessary in order to bring it into comparison with the thing to be measured, such that the end result of the comparison can be functionally translated into other comparisons of the same kind—indeed, these changes *define* the sameness of kind. These changes might be spatial movements, conceptual transfers from one person or group to another, etc. But a standard is only a standard if it can be used many times and retain its operational meaning—its uniformity—throughout all of those uses. The formal study of such uniformity through change is what a mathematician would refer to as "invariance through transformation," and is the subject matter of the mathematical theory of groups.

There are three rules that go into the definition of a group. First, a group G consists of elements such that any two of them can be composed together to form another element of the group—a group is closed under compositions. Secondly, there is a special element (often designated as "e") that is called the identity. When e is composed with any other element of G, the result is simply that other element. The third and final rule is that every element of G has an "inverse," a unique "opposite" member in G such that when an element and its inverse are composed together the result is the identity element e, making every act of composition undoable. See Garrett Birkhoff and Saunders MacLane (1997: *A Survey of Modern Algebra*, AK Peters, pp. 124 ff.)

Groups may be studied abstractly in their own right, or as systems of transformations operating on some other structure. Viewed in this latter way, the elements of a group are the composable and invertible transformations that can be applied to some other thing, thereby highlighting that which remains *invariant* under the group's particular system of operations.

For example, a circle can be rotated around its center and remain a circle, even maintaining its exact orientation on the plane on which it is situated and thus remain invariant with respect to any other figure. A square can be rotated 90^{o}, and still maintain all of its invariances as well. However, if the square is only rotated 30^{o}, while it is still the "same" square as before, its relationships to the plane in which it is embedded are no longer the same. So the group of transformations that leave the square invariant is a much smaller set than that which operates on the circle.

In a similar fashion, I can "transform" my meter stick from its location in my house to the lumberyard and use it to measure off the necessary units of material needed to complete a carpentry project back at my house. That "transformation"—the spatial translation of my meter stick—can be undone, and it can be composed with other such transformations (it can be moved to other locations), all in such a way that it leaves the meter stick itself invariant. The meaning of the stick as a measuring unit is uniform under spatial translations. Indeed, so robust is this invariance that I need only carry the "idea" of the meter stick with me; a written note with the relevant numbers will effectively "transform" into the correctly sized materials by logically projecting from my meter stick at home to that of the assistant's at the lumberyard. The group of transformations is such that the meaning of the measurements will retain its invariance. Spatial and extensive magnitudes are such as to permit of such casual faith in these invariances.

Not every spatial or geometric relation will be retained in these transformations; the stick—whether mine or the assistant's—may not have the same orientational relations that it had before the measurement. But position and orientation are not the sorts of invariance that are necessary to retain the functional structures of this type of measurement. Further mathematical details can be found in David H. Krantz, R. Duncan Luce, Patrick Suppes and Amos Tversky (2007: *Foundations of Measurement*, Dover Publications, chapters 10 & 19, and throughout.)

However, social measures are not so amenable to the assumption of invariance necessary for successful measurement. Social and human activities are notoriously *variant* and *non*-uniform on both an individual and a collective level. Consider the case of education and standardized testing. Exactly what is it that the *standard* in the standardized test is supposed to measure? Is education genuinely invariant from student to student, such that a standardized test can be meaningfully said to measure *learning*? If Fred has grasped general concepts while George has internalized details of historical relatedness, has one of them learned more than the other? What standard of invariance permits me to measure the differences between the two? Even if the measure is non-numerical and purely comparative or "ordinal" (Krantz, *et al*: 2007, pp's 2ff, 14ff, 38ff), what group-theoretic transformations allow me to make intelligible the notion of "measuring" such differences? Similar problems press upon any form of Utilitarian measure, whether in contemporary terms of money or in Mill's original sense of "pleasure." And what are we to make of more subtle forms of social "measure" such as (for the Aristotelians in the crowd) "human flourishing"?

In conclusion, in any act of measurement the presumption of invariance is necessarily present in the act. But that presumption is not necessarily validated by the facts of the case, and both the facts and the presumption can be masked by the false concreteness of specious precision associated with "measurement" claims that lack genuine logical validity. In order for any measurement to be legitimate, it must be able to withstand the group-theoretic requirements of invariance.

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On Non-Standard Models of Peano Arithmetic

In *The Reasoner* 2(1), January 2008, in the article titled *Can we falsify truth by dictat?* Bhupinder Singh Anand claimed there is no model of *PA* (first order Peano Arithmetic) with an infinite descending sequence relative to the successor function S. As he shows, the non existence of such a model would imply the non existence of any non-standard model of PA. This in turn would contradict at least the Upward Skolem-Löwenheim theorem (1956), Gödel's Completeness and Compactness theorems (1930). All of them are well established mathematical results.

I restrict myself here to briefly showing how Compactness implies the existence of non-standard models of PA.

Theorem 2.1 *If Compactness holds, there is a model of PA with an infinite descending sequence with respect to the successor function S.*

Proof.
Let $[N]$ be the structure that serves as standard model of PA, namely $< N, =, S, +, *, 0 >$. Let $T[N]$ be the theory of $[N]$ in the language of PA. PA is a subset of $T[N]$. Let Γ be the countable set of all sentences of the form $c_n = S(c_{n+1})$ with n a natural number. Let T be the union of Γ and $T[N]$. PA is a subset of T.
$T[N]$ plus any finite set of members of Γ has a model, e.g., $[N]$ itself, since $[N]$ is a model of any finite descending chain of successors. Consequently, by Compactness, T has a model; call it M.
M has an infinite descending sequence with respect to S because it is a model of Γ. Since PA is a subset of T, M is also a model of PA. □

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Relevant alternatives and the subject's context

In a recent article, Lihoreau and Rebuschi (2008: 'The factivity failure of contextualist "knows"', *The Reasoner* 2(1), 4–5) present a dilemma against *indexical contextualism about 'knows'* (ICK). In a nutshell, the argument is this: there are two plausible ways of cashing out ICK, NICK and MICK. However, NICK faces an insuperable difficulty having to do with the importance of the subject's context in determining the content of 'knows', and MICK is inconsistent with the factivity of 'knows', an eminently plausible principle. Thus, ICK fails. I will defend ICK by arguing that the difficulty that supposedly afflicts NICK is illusory.

NICK states that the relation expressed by 'knows' in a context c and a world w obtains between a subject x and a proposition p if and only if there is no world w' such that (i)–(iii) obtain:

(i) x cannot rule out w';

(ii) w' is a relevant alternative to w in c;

(iii) p is false at w'.

The argument against NICK is based on the following case. Suppose f_1, f_2, and f_3 are all the Fs there are, and that Bob has discovered that each of f_1–f_3 is a G. Al is informed of Bob's discoveries; however, Al is very anxious about s_1: the possibility that there are Fs other than f_1–f_3 that Bob has not considered. I, however, am not attending to s_1 at all. What concerns me instead is s_2: the possibility that Bob has not observed, of each f_1–f_3, that it is a G. Intuitively, it would be false for me to utter:

(1) Al knows that Bob knows that all Fs are Gs.

However, Lihoreau and Rebuschi claim that it follows from NICK that my utterance of (1) would be *true*. For, they argue, Al can rule out s_2, the only alternative that is relevant in my context. On the other hand, though s_1 is relevant in Al's context it is not relevant in my context, so according to NICK Al does not have to rule out s_1 in order for my utterance of (1) to be true. Lihoreau and Rebuschi draw the lesson that NICK does not properly take into account the role of the subject's context in determining the content of 'knows'.

The argument against NICK relies on the claim that when I utter (1), s_1 is not a relevant alternative in my context. This claim, however, relies on a particular conception of what makes an alternative relevant in a context. There is a whole spectrum of possible contextualist theories of 'knows', each compatible with NICK, that differ with respect to how they answer this question. Some of these theories entail that s_1 is a relevant alternative in the context in which I utter (1). For example, imagine a contextualist theory, T, incorporating the following *Rule of Anxiety*:

> If the subject of a knowledge attribution is very anxious about a certain possibility, then that possibility is a relevant alternative in the attributor's context.

T entails that s_1 is a relevant alternative in my context. Since Al cannot rule out s_1 and Bob does not know that all Fs are Gs if s_1 obtains, it follows from T and NICK that my utterance of (1) is false, as intuition demands.

The reason that NICK and T give the correct answer to the case is that T allows features of Al's context to determine the alternatives that are relevant in my context. This shows that Lihoreau and Rebuschi's quarrel should not be with NICK, but rather with overly narrow accounts of relevance that allow no role for the subject's context in determining the alternatives that are relevant in the attributor's context.

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§3
News

Aesthetics and Mathematics, 10–11 November, Utrecht

One would have thought that a discussion of foundational issues qua aesthetic issues, in relation to figures such as Gödel, in a broader historical context, as well as in relation to the even more broad context of mathematics and contemporary set theory, would have been well underway by now. But the literature in this area of aesthetics, at least from the contemporary foundations of mathematics perspective, is sparse. This gap in the literature, insofar as philosophers of mathematics are responsible for it, is likely due to the fact that these have by and large been concerned with interpreting the spectacular developments in mathematical logic and set theory in the twentieth century, as well as with continuing the philosophy of mathematics discussion which predates these developments. But it is precisely in the context of those new developments that aesthetics may have come to bear on mathematical practice in a new and perhaps even more decisive way than it has before.

A symposium organized around the principle of aesthetic experience in science, seemed, therefore, timely. Speakers included five mathematicians, two art historians, three philosophers, and a physicist. They were:

Marek Bartelik (Art History, New York/MIT), Juliet Floyd (Philosophy, Boston), Rob van Gerwen (Philosophy, Utrecht), Wilfrid Hodges, (Mathematics, London), Gerard ′t Hooft (Physics, Utrecht), James McAllister (Philosophy, Leiden), Ina Prinz (Art History, Arithmeum Museum, Bonn), Wim Veldman (Mathematics, Nijmegen), Andres Villaveces (Mathematics, Bogotá), Hugh Woodin (Mathematics, Berkeley), Boris Zilber (Mathematics, Oxford).

The symposium was preceded by the event "A Day in Mathematical Logic," a day of technical logic talks, and ended with the opening at the Mondriaan House in Amersfoort of the exhibition "Logic Unfettered: European and American Abstraction Now."

That "Aesthetics and Mathematics" was organized as part of this tri-event reinforced the idea behind its organization. This was to provide a forum for theoretical as well as, and very importantly, *pre-theoretical* discussion of the topic to take place; the idea being not just to discuss aesthetics in the abstract—but to try to first understand what it is that might be characterized as an aesthetic judgement in science and mathematics in the first place; to have, simply, an aesthetic experience, also in art. What this meant in practice was that speakers were encouraged to speak from within their subject—not to stray from their own territory, so to speak. Hence the presence at the event of a great deal of mathematics, but also of art.

Realizing this concept meant that some of the non-mathematicians might have become lost periodically—though many of the mathematics talks were surprisingly accessible. On the other hand some of the scientists might have felt flummoxed by the hyperarticulation of the professional aestheticians, both of whom (McAllister and van Gerwen) mounted a spirited defense of the idea that there is no such thing as mathematical beauty! This challenge to mathematicians and scientists to try to reflect carefully on their use of aesthetic terms, such as beauty, turned out to be very important, though, for the project of making their intuition that aesthetics has some irreducible purchase on scientific practice precise.

A similar tri-event is being planned to take place in two years, with the location, and also exhibition concept under discussion. The idea is to hold such an event biennially.

Juliette Kennedy
Logic, Helsinki

European Philosophy of Science Association, 14–17 November

The end of 2007 saw a major event within the philosophy of science community in Europe, the first conference of the recently born *European Philosophy of Science Association*. EPSA07, its founding conference, took place in Madrid, at Complutense University, on 14–17 of November and constituted the official public launching of the new Association. The *European Philosophy of Science Association* (EPSA) was born as a response to a general sense that philosophy of science in Europe is in need of further promotion and advancement, as well as strengthening the relations amongst members of the community, through the diverse areas of the discipline. It is perhaps too soon to say whether EPSA will indeed become what it intends to, but there are already some signs that it may become an intellectually influential body and an essential reference for the philosophy of science community in Europe. The warm welcome to the new Association among European philosophers of science has been particularly encouraging, and is well reflected in the highly enthusiastic and optimistic atmosphere at the conference. From that point of view the EPSA07 conference was a great success—it showed that the time is ripe for an Association like this to be born. We should also mention that it has been particularly significant for the Spanish philosophy of science community to be given such an opportunity to gain international relevance and visibility.

The conference call for papers was launched last March and was even more successful that the Steering Committee of EPSA envisaged. Over 400 contributions from all over Europe in all kinds of disciplines and areas of specialisation were submitted. The Programme Committee, chaired by Mauro Dorato (Rome) and Miklos Redei (London), and consisting of more than 30 renowned names of philosophers working in Europe, had the extraordinarily difficult task of selecting only 175 contributions for presentation at the conference. As a result the quality of talks was extremely high throughout the conference.

The conference was divided in five sections: General Philosophy of Science, Philosophy of Natural Sciences, Philosophy of Social Sciences, Formal Methods in the Philosophy of Science, and Social and Historical Studies of the Philosophy of Science. Detailed information about the programme may be found at the conference website: http://www.ucm.es/info/epsa07. We give here a short review of the most relevant topics discussed.

Within the first section, General Philosophy of Science, there were sessions devoted to nearly every topic actively pursued in philosophy of science today, such as causation, realism and confirmation, reductionism, structuralism, experiment and observation, prediction, models and representation, and simulation. The second section, Philosophy of Natural Sciences concerned mainly philosophical aspects of physics and the biomedical sciences. Most of the philosophy of physics talks discussed conceptual issues regarding space-time theories—such as the interpretation of special relativity, the relationship between geometry and matter, and the question of general covariance—,or quantum theories—including historical issues, reflections about ontology, and debates regarding the implications of Bell's inequalities. There were also discussions about classical physics, statistical mechanics and the role of symmetries in physics. The sessions devoted to the biomedical sciences included among others papers on ethical issues in biology and conceptual problems of evolutionary theory. The third section Philosophy of Social Sciences had contributions discussing internal issues to many of the social sciences, particularly economics and social psychology, as well as more general methodological issues such as the scientific status of the social sciences. The fourth section on Formal Methods was mainly concerned with philosophy of mathematics and logic but also with the application of logical tools in the study of scientific knowledge. Finally in the last section of the conference, on the more social and historical studies, there were papers on the Vienna Circle, Carnap and Feyerabend, and many case studies ranging from experimental and laboratory techniques to issues of gender and values in science.

There were also three plenary lectures, which aimed to provide an overview of the state of philosophy of science in Europe. The first plenary lecturer was Anne Fagot-Largeault from the Collège de France, who in her lecture entitled "Styles in the Philosophy of Science" characterised a number of at least three diverse styles currently in practice, which we may roughly refer to as analytical philosophy of science, formal methods, and historical epistemology. She ended her lecture by inviting further interaction between these traditions. Ilkka Niiniluoto, from Helsinki University, is precisely one of the greatest and leading members of the formal methods tradition. In his lecture entitled "Theory-Change, Truthlikeness and Belief-Revision", Niiniluoto gave a detailed account of the evolution of attempts to characterise scientific knowledge formally through a notion of truthlikeness. The third and closing plenary speaker, Michael Friedman from Stanford University, gave a talk entitled "Einstein, Kant and the A Priori". The talk was devoted to the role of relative a priori principles in the history of physics, and mainly theories of space-time, from Kant's time onto the advent of general relativity.

One of the highlights of the conference was not an academic occasion properly speaking but the first general Assembly of the new Association, EPSA. A new Steering Committee was elected for the next two years, and Stathis Psillos (University of Athens) was chosen as its President. It was decided that a conference take place biennially, and venues of the next two conferences were already proposed (Amsterdam for 2009, and Athens for 2011). The organisers of the conference would very much like to thank everyone who participated and all those who have so warmly and encouragingly reacted to the birth of the Association. There is much excellent work in the field produced right now in Europe that remains opaque—e.g., in comparison with philosophy of science produced in North America. The main goal of the EPSA conferences is precisely to promote this work and make it better known across the world. The feeling in Madrid was that EPSA is an Association that looks to the future, and it would be nice if this conference was to serve as a springboard to even better and bigger occasions. Hopefully philosophers of science will continue to unite in advancing EPSA's goals—which should always be very much reflect their own.

Iñaki San Pedro
University of the Basque Country &
Complutense University of Madrid

Mauricio Suárez
Complutense University of Madrid

Adán Sus
Autonomous University of Barcelona

Causality Study Fortnight

The Causality Study Fortnight is a new project funded by the British Academy and run by Jon Williamson and myself. The Fortnight will take place between 8th and 19th of September at the Centre for Reasoning, University of Kent, Canterbury, UK. Anyone interested in causality and causal methods in the sciences is very welcome to join us. The Fortnight will have 2 days of tutorials on causality, probability and their use in science, followed by an international conference on causality and probability in the sciences (CAPITS 2008). The second week will be devoted to advanced research seminars on various topics such as: probabilistic causality, levels of causality and the interpretation of probability, mechanisms and causality, machine learning and causality, causality and the mind, causality in the history of philosophy.

We hope that the Fortnight will be a hub for fruitful and stimulating discussions between philosophers and scientists on cutting edge research, but also a place where postgraduate students can improve their knowledge and understanding of the philosophies of causality and probability and/or of causal methods. The tutorials are being organised in response to demand after CAPITS 2006, and will provide a solid basis for interdisciplinary exchanges between philosophy and science during the conference and the seminars.

Federica Russo
Philosophy, Louvain & Kent

Calls for Papers

Hybrid Logic: Special Issue of the Journal of Logic, Language and Information, deadline 1 March.

Machine Learning in Space: Special Issue of the Machine Learning Journal, deadline 31 March.

Multiple Simultaneous Hypothesis Testing: Special Issue of the Journal of Machine Learning Research, deadline 31 March.

Conditionals and Ranking Functions: Special issue of Erkenntnis, franz.huber@uni-konstanz.de, deadline 31 May.

Causality and Probability in the Sciences
Deadline 1 July

§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.

Cantor

The German mathematician Georg Cantor (1845-1918) is the founder of modern set theory. Prior to his work, mathematicians had been using an implicit, naive concept of set without a distinction between different sizes of infinite sets. Cantor proved that the set of real numbers is 'bigger' than the set of natural numbers, showing for the first time that there exist infinite sets of different sizes.

Cantor clarified the concept of an infinite set with his distinction between cardinal and ordinal numbers, on which he built an arithmetic of infinite sets. He showed that there are infinitely many possible sizes for infinite sets. He also introduced the concept of the power set of a set A, which is the set of all possible subsets of A, and he proved what became known as Cantor's theorem: the size of the power set of A is strictly larger than the size of A, even for an infinite set A.

Cantor also introduced the continuum hypothesis, which states: 'There is no set whose size is strictly between the set of the integers and the set of the real numbers.' Kurt Gödel and Paul Cohen proved the hypothesis to be independent from the axioms of ZFC set theory.

Koen Vervloesem

Modal Logic

A modal logic is one containing one or more modal operators, which can be used to formalize the language of necessity and possibility (alethic logic); knowledge and belief (epistemic and doxastic logic); the future and the past (temporal logic); obligations and permissions (deontic logic); and action (dynamic logic). Alethic modal logic contains the operators '$\Box$', read as 'necessarily' and '$\Diamond$', read as 'possibly', which are placed in front of a sentence to form a new sentence. Each operator is the dual of the other: $\Diamond\phi \leftrightarrow \neg\Box\neg\phi$ and $\Box\phi \leftrightarrow \neg\Diamond\neg\phi$.

Epistemic and doxastic modal logics contain modalities 'K_i' and 'B_i', read as 'agent i knows/believes that', respectively. Temporal modal logic contains the future modalities 'G' ('it always will be the case that') and its dual 'F' ('it will be the case that') and the past modalities 'H' ('it always was the case that') and its dual 'P' ('it was the case that'). Deontic modal logic contains a modality 'Ob' for 'it is obligatory that' and its dual, 'Pe', 'it is permitted that'. Dynamic logic contains operators for actions: '$[a]\phi$' and '$\langle a\rangle\phi$' say that, after action a, 'ϕ' will hold or might hold, respectively. In general, modal operators are non-truth-functional, so that the truth of '$\Box\phi$' for example will not be a function of the truth of 'ϕ'.

The basic propositional modal logic, **K**, is built from standard propositional logic by augmenting the language with '$\Box$' and '$\Diamond$' (in the case of an alethic logic, which will be used as the example here) and adding the necessitation rule:

$$\text{from} \vdash \phi, \text{infer} \vdash \Box\phi$$

plus all instances of the distribution scheme:

$$\Box(\phi \to \psi) \to (\Box\phi \to \Box\psi).$$

Other well-known systems of propositional modal logics are obtained by adding one or more of the following axioms:

(D) $\Box\phi \to \Diamond\phi$ (whatever is necessary is possible);

(T) $\Box\phi \to \phi$ (whatever is necessary is true);

(4) $\Box\phi \to \Box\Box\phi$ (whatever is necessary is necessarily so);

(5) $\Diamond\phi \to \Box\Diamond\phi$ (whatever is possible is necessarily so).

The modal logics **D** and **T** are simply system **K** plus the (D) and (T) axioms, respectively. **S4** is the logic **K** plus the (T) and (4) axioms and **S5** is **K** plus the (T) and (5) axioms. **K** through **D**, **T**, **S4** and **S5** form a hierarchy in which each logic includes the previous logic (so that the (D) axioms hold in **T**, **S4** and **S5** as well as in **D**, for example).

Modal logics are usually interpreted using relational semantics (often called 'Kripke semantics'). A frame $\mathcal{F}$ is a pair $\langle S, R\rangle$ where S is a set of points (which can be thought of as possible worlds) and R is a binary relation between points in S. A model $\mathcal{M}$ (in the propositional case) is a frame plus a valuation function V, assigning a truth-value to each primitive sentence at each point in S. '$\Box\phi$' is true at a point s in the model iff 'ϕ' is true at all points related to s by R and '$\Diamond\phi$' true at s iff 'ϕ' is true at some point related to s. In symbols:

$\mathcal{M}, s \Vdash \Box\phi$ iff $\mathcal{M}, s' \Vdash \phi$ for all s' such that Rss'.
$\mathcal{M}, s \Vdash \Diamond\phi$ iff $\mathcal{M}, s' \Vdash \phi$ for some s' such that Rss'.

Intuitively, '$\Box\phi$' means that 'ϕ' is true at all accessible possible worlds. **S5**, in which all points are accessible from all others, is usually taken to be the correct alethic logic.

The points in a model can also be interpreted as the states in a computational system, or the stages of a evolving process. Because of the myriad possible interpretations of the relational semantics, propositional modal logics have also found a home in computer science and AI. They also have attractive computational properties: they are decidable and have relatively low complexity.

Modal operators can be added to first-order logic to give first-order modal logic. In its semantics, each point has an associated domain of entities and the extension of a predicate can vary from point to point. One question is: should the domain be the same for all points? In alethic logic, the intuitive answer is: no, for some things might not have existed (and some non-existents could have existed). This variable-domain approach complicates the logic, as some constants will fail to denote at some points. An alternative is a constant domain approach in which, at a given point, just some of the entities in the domain exist.

Mark Jago
Philosophy, Nottingham & Macquarie

§5
Letters

Dear Reasoners,

Two comments on Fred Sommers' contribution to the last *Reasoner*. I am pleased to see his invocation of empirical evidence about how people reason, but his brief comment is restricted to our ability to get valid deductive inferences right. A very significant finding of empirical work on reasoning (as of common experience in teaching formal logic) is the extent to which we we are fooled by invalid deductive arguments. Sommers' algebraic suggestion neatly delivers one finding of my own work (1997: Jamaican teachers and deductive logic, *Caribbean Journal of Education* 19, 113-130): that people treat the form 'No A are B, no B are C, so no A are C' as about as good as regular hypothetical syllogism (on his view, $-A + B$, $-B + C$, so $-A + C$). But people's similar inclination to accept denying the antecedent or affirming the consequent does not seem to be so easily fitted into his scheme.

Ed Brandon
University of the West Indies

§6
Events

February

Logic Meeting: University of California, Los Angeles, 1–3 February.

FoIKS: Foundations of Information and Knowledge Systems, Pisa, Italy, 11–15 February.

Longitudinal Data Analysis: Department of Statistics, University of Auckland, New Zealand, 21–22 February.

March

Relativism and Rational Reflection: 10th Annual Pitt–CMU Graduate Student Philosophy Conference, University of Pittsburgh, 1 March.

Artificial General Intelligence: The First Conference on Artificial General Intelligence, Memphis, Tennessee, 1–3 March.

Science and Pseudoscience: University of Birmingham, UK, 15 March.

Russell: Proof Theory meets Type Theory, Swansea, 15–16 March.

Constraint-Sac: Track on Constraint Solving and Programming, at the 23rd Annual ACM Symposium on Applied Computing, Fortaleza, Brazil 16–20 March.

Causation: 1500-2000: King's Manor, University of York, 25–27 March.

UnCLog: International Workshop on Interval / Probabilistic Uncertainty and Non-Classical Logics, Ishikawa, Japan, 25–28 March.

Naturalism, Normativity, and the Space of Reasons: University College Dublin, 28–29 March.

April

AISB: Artificial Intelligence and Simulation of Behaviour, Aberdeen, 1–4 April.

Subjective Bayesian Methods: Department of Probability and Statistics, University of Sheffield, 2 April.

RelMiCS10-AKA5: 10th International Conference on Relational Methods in Computer Science & 5th International Conference on Applications of Kleene Algebra, Frauenwörth, Germany, 7–11 April.

Reduction and the Special Sciences: Tilburg Center for Logic and Philosophy of Science, 10–12 April.

Theoretical Frameworks: Theoretical Frameworks and Empirical Underdetermination Workshop, University of D"usseldorf, 10–12 April.

FLOPS: Ninth International Symposium on Functional and Logic Programming, Ise, Japan, 14–16 April.

Workshop: XVIII Inter-University Workshop on Philosophy and Cognitive Science, Madrid, luis.fernandez@filos.ucm.es, 22–24 April.

Practical Rationality: Intentionality, Normativity and Reflexivity, University of Navarra, 23–25 April.

SDM: 8th Siam International Conference on Data Mining, Hyatt Regency Hotel, Atlanta, Georgia, USA, 24–26 April.

May

SBIES: Seminar on Bayesian Inference in Econometrics and Statistics, University of Chicago Graduate School of Business Gleacher Center, 2–3 May.

SIG16: 3rd Biennial Meeting of the EARLI-Special Interest Group 16—Metacognition, Ioannina, Greece, 8–10 May.

CLE, EBL & SLALM: 30th Anniversary of the Centre for Logic, Epistemology and the History of Science (CLE), UNICAMP, 15th Brazilian Logic Conference, and 14th Latin-American Symposium on Mathematical Logic, Paraty, Brazil, 11–17 May.

ArgMAS: Fifth International Workshop on Argumentation in Multi-Agent Systems, Estoril, Portugal, 12–13 May.

Interval Probability: Workshop on Principles and Methods of Statistical Inference with Interval Probability, Durham, 12–16 May.

FEW: Fifth Annual Formal Epistemology Workshop, Madison, Wisconsin, 14–18 May.

UR: Special Track on Uncertain Reasoning, 21st International Florida Artificial Intelligence Research

Society Conference (FLAIRS-21), Coconut Grove, Florida, 15–17 May.

AI Planning and Scheduling: A Special Track at the 21st International FLAIRS Conference (FLAIRS 2008), Coconut Grove, Florida, 15–17 May.

RSKT: Rough Sets and Knowledge Technology, Chengdu, 17–19 May.

NAFIPS: North American Fuzzy Information Processing Society Annual Conference, Rockefeller University, New York, 19–22 May.

ISMIS: The Seventeenth International Symposium on Methodologies for Intelligent Systems, York University, Toronto, Canada, 20–23 May.

WCB: Workshop on Constraint Based Methods for Bioinformatics, Paris, 22 May.

COMMA: Second International Conference on Computational Models of Argument, Toulouse, 28–30 May.

AI: 21st Canadian Conference on Artificial Intelligence, Windsor, Ontario, 28–30 May.

Expressions of Analogy: Faculty of Social and Human Sciences, New University of Lisbon, 29–31 May.

June

ARea: International Workshop on Advancing Reasoning on the Web: Scalability and Commonsense, Tenerife, 1 June.

WCCI: IEEE World Congress on Computational Intelligence, Hong Kong, 1–6 June.

CSHPS: Canadian Society for History and Philosophy of Science, University of British Columbia, Vancouver, 3–5 June.

CiE: Computability in Europe 2008: Logic and Theory of Algorithms, University of Athens, Athens, 15–20 June.

IIS: Intelligent Information Systems, Zakopane, Poland, 16–18 June.

DM: SIAM Conference on Discrete Mathematics, University of Vermont, Burlington, Vermont, 16–19 June.

Logica: Hejnice, Czech Republic, 16–20 June.

IEA-AIE: 21st International Conference on Industrial, Engineering and Other Applications of Applied Intelligent Systems, Wroclaw, Poland, 18–20 June.

HOPOS: Seventh Congress of the International Society for the History of Philosophy of Science, Vancouver, Canada, 18–21 June.

HDM: Multivariate statistical modelling and high dimensional data mining, Kayseri, Turkey, 19–23 June.

EPISTEME: Law and Evidence, Dartmouth College, 20–21 June.

IPMU: Information Processing and Management of Uncertainty in Knowledge-Based Systems, Malaga, Spain, 22–27 June.

Med: 16th Mediterranean Conference on Control and Automation, Ajaccio, Corsica, 25–27 June.

ESPP: European Society for Philosophy and Psychology, Utrecht, 26–28 June.

EWRL: European Workshop on Reinforcement Learning, INRIA, Lille, 30 June – 3 July.

July

WoLLIC: 15th Workshop on Logic, Language, Information and Computation, Edinburgh, 1–4 July.

LOFT: 8th Conference on Logic and the Foundations of Game and Decision Theory, 3–5 July.

ICML: International Conference on Machine Learning, Helsinki, 5–9 July.

CAV: 20th International Conference on Computer Aided Verification, Princeton, 7–14 July.

Bayesian Modelling: 6th Bayesian Modelling Applications Workshop, Helsinki, 9 July.

UAI: Uncertainty in Artificial Intelligence, Helsinki, 9–12 July.

COLT: Conference on Learning Theory, Helsinki, 9–12 July.

Classical Logic and Computation: Reykjavik, 13 July.

BPR: The 1st International Workshop on Bit-Precise Reasoning, Princeton, 14 July.

ITSL: Information Theory and Statistical Learning, Las Vegas, 14–15 July.

IKE: International Conference on Information and Knowledge Engineering, Las Vegas, 14–17 July.

NorMAS: 3rd International Workshop on Normative Multiagent Systems, Luxembourg, 15–16 July.

DEON: 9th International Conference on Deontic Logic in Computer Science, Luxembourg, 15–18 July.

NCPW: 11th Neural Computation and Psychology Workshop, Oxford, 16–18 July.

ISBA: 9th World Meeting, International Society for Bayesian Analysis, Hamilton Island, Australia, 21–25 July.

Model Selection: Current Trends and Challenges in Model Selection and Related Areas, University of Vienna, 24–26 July.

ESARM: Workshop on Empirically Successful Automated Reasoning for Mathematics, Birmingham, UK, 26 July – 2 August.

First Formal Epistemology Festival: Conditionals and Ranking Functions, Konstanz, 28–30 July.

August

Conference: Language, Communication and Cognition, University of Brighton, 4–7 August, Brighton, UK.

ESSLLI: European Summer School in Logic, Language and Information, Freie und Hansestadt Hamburg, Germany, 5–15 August.

IJCAR: The 4th International Joint Conference on Automated Reasoning, 10–15 August.

ICT: The Sixth International Conference on Thinking, San Servolo, Venice, 21–23 August.

Compstat: International Conference on Computational Statistics, Porto, Portugal, 24–29 August.

September

IVA: The Eighth International Conference on Intelligent Virtual Agents, Tokyo, 1–3 September.

10th Asian Logic Conference: Kobe University, Japan, 1–6 September.

COMSOC: 2nd International Workshop on Computational Social Choice, Liverpool, 3–5 September.

KES: 12th International Conference on Knowledge-Based and Intelligent Information & Engineering Systems, Zagreb, 3–5 September.

SMPS: Soft Methods for Probability and Statistics, 4th International Conference, Toulouse, 8–10 September.

> Causality and Probability in the Sciences
>
> University of Kent, Canterbury UK, 10–12 September

ICAPS: International Conference on Automated Planning and Scheduling, Sydney, 14–18 September.

CSL: 17th Annual Conference of the European Association for Computer Science Logic, Bertinoro, Italy, 15–20 September.

PGM: The fourth European Workshop on Probabilistic Graphical Models, Aalborg, Denmark, 16–19 September.

October

SETN: 5th Hellenic Conference on Artificial Intelligence, Syros, Greece, 2–4 October.

Reason, Activism, and Change: University of Windsor, 3–5 October.

December

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

§7 Jobs

Philosophy, Exeter: Lecturer / Senior Lecturer / Associate Professor, deadline 5 February.

Planning under Uncertainty: Postdoc, Instituto Superior Técnico, Lisbon, deadline 7 February.

Philosophy: Lecturer, University of East Anglia, UK, deadline 15 February.

Constraint Reasoning: Internships, Constraint Reasoning Group, Microsoft Research, Cambridge, deadline 28 February.

Imprecise Probability: Postdoc, Imprecise-Probability Methods for Data Mining in Genomics and Medicine, Switzerland, deadline 29 February.

Philosophy of Science, Cambridge: Lecturer, Department of History and Philosophy of Science, deadline 29 February.

§8 Courses and Studentships

Courses

European Master's Program in Computational Logic: Free-University of Bozen-Bolzano in Italy, Technische Universität Dresden in Germany, Universidade Nova de Lisboa in Portugal, Universidad Politecnica de Madrid in Spain and Technische Universität Wien in Austria, deadline 10 February.

> MA in Reasoning
>
> An interdisciplinary programme at the University of Kent, Canterbury, UK. Core modules on logical, causal, probabilistic, scientific and mathematical reasoning and further modules from Philosophy, Psychology, Computing, Statistics and Law.

MLSS: 10th Machine Learning Summer School, Kioloa Coastal Campus, Australian National University, 3–14 March.

Algorithmic Decision Theory: MCDA, Data Mining and Rough Sets, Doctoral School, Troina, Italy, 11–16 April.

Logic School: State University of Campinas, Brazil, 7–9 May.

Logic and Formal Epistemology: Summer school for undergraduates, Department of Philosophy, Carnegie Mellon University, Pittsburg, 9–27 June.

SIPTA: 3rd SIPTA School on Imprecise Probabilities, Montpellier, 2–8 July.

Probabilistic Causality: Central European University, Budapest, 21 July–1 August.

ESSLLI: European Summer School in Logic, Language and Information, Hamburg, 4–15 August.

Mathematics, Algorithms, and Proofs: Summer School, Abdus Salam International Centre for Theoretical Physics, Trieste, 11–29 August.

> Causality Study Fortnight
>
> University of Kent, Canterbury UK, 8–19 September

Studentships

Arché Postgraduate Studentships: The Arché Research Centre at the University of St Andrews is offering up to six three-year PhD studentships for uptake from September 2008, deadline 1 February.

Philosophy of Technology: 2 PhDs, Delft, deadline 12 February.

Leeds Philosophy: 2-3 postgraduate studentships in philosophy and history and philosophy of science, deadline 1 March.

Acknowledgements

The Reasoner is a development of the progicnet academic network supported by the Leverhulme Trust.