THE REASONER

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EDITORIAL

It is with great pleasure that I open the third issue of *The Reasoner*. This month's issue presents a variety of topics ranging from logic to law, from philosophy to linguistics. Articles are very diverse in their themes and in their technical level. If they are so diverse, what keeps all these pieces together in *The Reasoner*? The answer is quite straightforward: they deal with reasoning in a specific field or they present an argument about a specific topic, in line with *The Reasoner*'s aims and scope.

However, despite this common denominator, the question remains as to why *The Reasoner* would host pieces so different to each other. The number of subscriptions to the mailing list, the number of downloads and visits to www.thereasoner.org, and the number of submissions received spotlights the need of a different method of spreading information, other than the standard academic milieux. Notably, it points to a faster circulation of ideas, arguments, projects. It also tells about people's interest in addressing an audience specialised outside one's area of research. *The Reasoner* thus represents a concrete step toward interdisciplinarity—undoubtedly a challenging objective for reasoners of all horizons, but perhaps the only way to go to produce good research. If scholars and academics are looking for alternative places to disseminate their ideas and to find out about new research, this doesn't mean that gazette pieces can or will replace publication in the form of journal articles or monographs. Instead, it shows a renewed interest in reasoning as the skeleton of all our intellectual enterprise.

It is a great success for *The Reasoner* to receive so much interest from scientists, philosophers and non-academics, but I'd like to launch an additional call for contributions. That is short notes about events you attended—do let *The Reasoner*'s community know about your thoughts on conferences you have been to.

Happy reasoning!

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§2

FEATURES

Pierre may be ignorant, but he's not irrational

Saul Kripke put forward a puzzle about belief attribution that has subsequently spawned an extensive literature (see Kripke's 1979: 'A Puzzle About Belief' in *Meaning and Use*),[1] David Sosa (1996: 'The Import of the Puzzle About Belief', *The Philosophical Review*, 105: 378-379), offers the following rendition of Kripke's argument.

1. Pierre is rational. (assumption for *reductio*)

2. Pierre, on reflection, assents to "Londres est jolie".

3. "London is pretty" is a translation of "Londres est jolie".

4. Pierre, on reflection, assents to "London is not pretty".

5. Pierre believes that London is pretty. (2,3, D)

6. Pierre believes that London is not pretty. (4, D)

7. Pierre believes that London is pretty and Pierre believes that London is not pretty. (5, 6, adjunciton)

8. If Pierre believes that London is pretty and Pierre believes that London is not pretty, then Pierre has contradictory beliefs. (analytic?)

9. Pierre has contradictory beliefs. (7, 8, *modus ponens*)

10. If Pierre has contradictory beliefs, then Pierre is not rational. (analytic?)

11. Pierre is not rational.

In the argument above, "D" stands for what might be dubbed the disquotation-translation principle; which Sosa (1996, 377) characterizes as follows:[2]

If a normal L-speaker, on reflection, sincerely assents to "p" (a sentence of L), and if "q" is a translation (into English) of "p", then the speaker believes that q.

One might be tempted to deny principle D.[3] But, as Sosa remarks, denying D might not be the best way to go here. He writes,

> . . . the principle of disquotation is a relatively basic principle governing our practice of belief attribution. Whatever its merits, it is clear that we rely on it in practice . . . There may be many principles upon which we rely in our practices which are philosophically questionable. If it were the case, however, that without such principles of disquotation many belief attributions that we take to justified would not be justified, then denying those principles would leave us with the task of finding an alternative justification for those attributions (1996, 383).

However, if we do not deny D, where does the argument go awry? Interestingly (and perhaps peculiarly), Sosa's attempt at a solution is to deny (8). That is, he denies that if Pierre believes that London is pretty and Pierre believes that London is not pretty, then Pierre has contradictory beliefs. However, I think we need not make such a questionable maneuver.

It appears that the most plausible thing to say is that premise (10) is false. To see why (10) ought to be rejected, consider the following case of an agent who has contradictory beliefs. Suppose that Tim is taking an introductory course in logic and he believes that the form of an argument (*modus ponens*, say) is valid. Imagine that he later encounters a different argument that is of the same form, but for some reason he believes that the form of this argument is invalid. So Tim believes of this argument form that it is valid and he also believes of this argument form that it is invalid. It seems that he thus has contradictory beliefs. Would we consider Tim to be irrational? Tim is surely not astute. One might be

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[1] Responses to Kripke's paper include, for example, Fogelin (1993: 'Pierre, Saul, Ruth and Bob, and a Puzzle About Belief', in *Modality, Morality, and Belief: Essays in Honor of Ruth Barcan Marcus*, Sinnott-Armstrong, Raffman and Asher (eds.), Cambridge University Press: New York, 201-214), Lewis (1981: 'What Puzzling Pierre does not believe', *Australasian Journal of Philosophy*, 59:283-289), Marcus (1981 'A Proposed Solution to the Puzzle of Belief', *Midwest studies in Philosophy* 6:501-510), Over (1983: 'On Kripke's Puzzle', *Mind*, 92:253-259), Schiffer (1992: 'Belief Ascription', *Journal of Philosophy* 89:499-521).

[2] Sosa simply dubs this principle the "disquotation principle", but this principle is different from the one that Kripke discusses. I think a more appropriate name for Sosa's principle is the disquotation-translation principle. It is worth noting that Kripke (1979) discusses two principles which Sosa's principle seems to capture. Kripke's disquotation principle is:

If a normal English speaker, on reflection, sincerely assents to "p", then she believes that p (where 'p' is to be replaced, both inside and outside quotation marks, by an appropriate standard English sentence which lacks indexical or pronominal devices or ambiguities).

Kripke's second principle is called the translation principle. It runs as follows.

If a sentence of one language expressed a truth in that language, then any translation of it into any other language also expresses a truth (in that other language)

[3] See, for example, Moore (1999: 'Misdisquotation and Substitutivity: When Not to Infer Belief From Assent', *Mind* 108:335-366) for an argument against the disquotation principle. See Frances (2000: 'Disquotation and Substitutivity' *Mind*, 109:519-526) for a reply to this argument.

tempted to call him obtuse, but he is certainly not irrational. We might be willing to call Tim irrational if it were pointed out to him that the argument form of the first argument and the second argument *are the same*, and yet he continued to believe of that form that it is valid and also believe that it is invalid. That is, what would make Tim irrational is his believing a contradiction, his being aware that he believes that contradiction, and his obstinacy in continuing to believe the contradiction even in the face of this awareness.

We might imagine a number of similar cases (not unlike Kripke's) where an agent has contradictory beliefs, and yet it does not seem correct to say that she is irrational. (One person's *modus ponens* is another's *modus tollens*!) What I am arguing is that simply having contradictory beliefs is not sufficient for irrationality. Lacking certain information or knowledge of certain relevant facts does not make one irrational—it simply makes one ignorant.

So it seems that appealing to an agent's contradictory beliefs is not enough to show that she is irrational. The upshot regarding Kripke's puzzle should be obvious. If premise (10) is false, then the argument is unsound, and so we have no difficulty in regard to Pierre being irrational when we claim that he believes both that London is pretty and believes that London is not pretty. In the end, it appears that Kripke's puzzle dissolves once we realize that Pierre is merely ignorant and not some sort of bizarre irrational being.

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Free Will and Lucky Decisions

If causal determinism is true then the future is fixed. Given that the past is what it is, and given that the laws of nature are what they are, things can only unfold one way. However, if causal determinism is false, then indeterminism is true. If indeterminism is true then the future is not fixed. More than one future is compatible with the past and the laws of nature.

Libertarians maintain that free will is incompatible with determinism. Their reasoning normally goes as follows: neither the past nor the laws of nature are under our ultimate control. So, if the past and the laws of nature determine our actions, then our actions are not under our ultimate control either.

The problem for libertarians is that free will also seems incompatible with indeterminism. The reasoning here normally goes as follows: if an agent's decision is nondeterministically caused by his prior deliberative process then if we roll back the clock to the moment just prior to decision-making and run the sequence through again an innumerable number of times, in some re-runs he will decide one way, and in some another. (Some of the most prominent discussions of this argument include van Inwagen, 1983: *An Essay On Free Will*. Oxford: Clarendon Press, p. 41; Fischer, 1999: "Recent work on moral responsibility", *Ethics*, pp. 93-139; Mele, 1995: *Autonomous Agents: From Self-Control to Autonomy*, Oxford: Oxford University Press, pp. 195-209; Kane, 2002: "Some Neglected Pathways in the Free Will Labyrinth", in Kane, R. (ed.) *The Oxford Handbook of Free Will*, Oxford: Oxford university press; Strawson, 2000: "The Unhelpfulness of Indeterminism", *Philosophy and Phenomenological Research*, pp. 149-155). Because the agent's prior desires, beliefs, and all other motivational states, are being kept constant it seems to be a matter of chance or luck that the agent decided one way rather than another. Luck or chance annul control and so therefore indeterminism seems to preclude free will.

Popular though the above argument is, its power depends upon the almost universally accepted assumption that luck or pure chance annuls control and free will. But the assumption is open to challenge. Consider the following. We all sometimes face torn decisions—cases where we take ourselves to have equally compelling reasons for competing courses of action but must choose one. When we make a torn decision, can we not readily admit that it was a matter of luck that we decided one way rather than the other? When I make a torn decision it seems quite plain to me that it was a matter of luck that I decided as I did. Up to the moment of choice, my decision could have gone either way. Yet when I state that my torn decision was a matter of luck, this is not in order to disown responsibility or deny my free will in having made it. After all, the decision was still *mine*, and nothing or no-one made me decide as I did. Again, from my perspective it seems perfectly obvious that my decision was both a matter of luck *and* that I am responsible for having made it, and by extension, that I made it of my own free will and controlled it sufficiently.

Many libertarians argue that the replay argument begs the question because it assumes that in the absence of deterministic causation all we have is pure chance, thereby ruling out by fiat the possibility of contra causal control. These libertarians would feel entitled to say that in the replays it was *not* a matter of luck that the agent decided one way rather than the other. But my point above is that irrespective of whether we have contra causal control, our torn decisions are *still* a matter of luck in every important sense, because we make them arbitrarily. For by definition, in the case of a torn decision, the reasons for one course of action over another are not compelling, and so I ultimately just have to decide randomly between the competing options. So, possession of contra-causal control would do nothing to render my torn decisions any less 'lucky' than if my torn decisions were chancy.

In fact, insisting that I exercise contra-causal con-

trol over my torn decisions actually makes them more *controlled* than they appear to be, and more controlled than they *need* to be. For the only reason to see chanciness as undermining of free will is its association with luck. In other words, indeterminism is plausibly only a problem because indeterminism renders our decisions chancy, and chanciness is a problem because if our decisions are chancy then it is a matter of luck which way we decide. But if our torn decisions are *obviously* a matter of luck irrespective of whether or not we exercise contra-causal control over them, and if in addition, the 'luck' of our torn decisions is not taken to indicate any lack of free will and responsibility, then chanciness is *not* toxic to free will. The upshot therefore is that the replay argument does not raise a problem for the compatibility of free will with indeterminism. Rather than denying chance, libertarians should try to be bolder and simply reject that chance is a problem.

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United States v. Shonubi: Statistical Evidence and "The Same Course of Conduct" Rule

In the United States, when determining a sentence, judges are allowed to take into account the accused's alleged previous offences even if s/he was not convicted of them (*Williams v. New York* 337 U.S. 241). To make this interpretation of the constitutional presumption of innocence even more peculiar, judges are allowed to take into account the accused's alleged offences even if s/he was acquitted of them (*United States v. Watts*, 519 U.S. 148 (1997)). In *Shonubi*, the prosecution sought a "sentence enhancement" under the "the same course of conduct" rule (U.S.S.G. §1B1.3(a)(2)), and relied on statistical evidence to prove drug quantities allegedly smuggled by Shunobi on eight occasions. The *Shonubi* case demonstrates the importance of the context in which statistical evidence is used and the fact it is meant to prove.

Charles Shonubi was arrested on 10.12.1991, at JFK Airport after flying from Nigeria, carrying 103 balloons containing 427.4g of heroin in his digestive tract. He was convicted by the jury of heroin importation (*United States v. Shonubi*, 802 F. Supp. 859 (E.D.N.Y., 1992) [Shonubi I]). The controversy arose in the context of sentencing, when the judge followed a table in the Sentencing Guidelines fixing the imprisonment period according to overall drug quantities smuggled (U.S.S.G. §2D1.1). Judge Weinstein took into account Shonubi's previous frequent trips to Nigeria and found that he had made eight smuggling trips. He then multiplied the amount of heroin found on Shonubi by eight, producing a total of 3,419.2g. Shonubi appealed, and

the sentence was vacated for lack of "specific evidence" (*United States v. Shonubi*, 998 F.2d 84 (2d Cir, 1993) [Shonubi II]). Judge Weinstein then accepted evidence from reports of heroin quantities seized from 117 Nigerian heroin swallowers arrested at JFK Airport during the same time period that spanned Shonubi's eight trips. These reports were accompanied by testimony of expert statisticians. He also surveyed the federal judges of the Eastern District to obtain their opinions concerning heroin swallowers. Based on this evidence, he found that Shonubi had carried between 1,000g and 3,000g of heroin during his eight trips (*United States v. Shonubi*, 895 F.Supp. 460 (E.D.N.Y.) [Shonubi III]. Shonubi appealed again and the Court of Appeal found that this evidence still did not satisfy the specific evidence requirement about 'what Shonubi has done' (*United States v. Shonubi*, 103 F.3d 1085 (2d Cir, 1997) [Shonubi IV]). Judge Weinstein then reduced the sentence, albeit protesting that the "specific evidence" category lacks both theoretical and legal foundations (*United States v. Shonubi*, 962 F. Supp. 70, p. 375 (E.D.N.Y. 1997) [Shonubi V].

This legal ordeal has received intense scholarly attention. It was even suggested '[t]he opinion of the court of appeals is …*depressing* because …it suggests that quite a few judges …still may not have a grasp of some basic characteristics of probabilistic and statistical methods and arguments' (Tillers, "Introduction: Three Contributions to Three Important Problems in Evidence Scholarship", 18 Cardozo L. Rev. 1875 (1997), p. 1879). Without any attempt to summarise this debate, it is nevertheless worth making the following point.

Compare two hypothetical cases. In the first, the accused admits making another seven previous successful drug-smuggling trips. Negligently, during the police investigation, he is not examined about the drug quantities in those trips. He later refuses to provide further information. The judge resorts to statistical evidence to calculate overall drug quantities. There is something intuitively troubling in allowing the accused evade the law in regards to his seven previous drug-smuggling trips, *which he admitted*, just because there is no "specific evidence" about the quantities.

In the second hypothetical, the accused also made previous trips. However, not only does he dispute the number of trips, he repeatedly denies smuggling drugs in those trips. If similar statistical evidence is used to prove the drug quantities, it does not prove that the previous trips were drug-smuggling trips but rather *assumes* it. Using the same statistical evidence seems more objectionable than in the previous case.

Shonubi resembles more the second case than the first. Whilst strongly disagreeing over the evidence required to prove drugs quantities, both courts in their five decisions accepted the prosecution's contention that

drugs were smuggled in eight trips. Yet Shonubi was only charged and convicted of drug importation in one trip (the last). It is highly questionable whether he could have been convicted, and the prosecution decision not to press those charges speaks for itself. True, the circumstances and Shonubi's contradictory explanations indicate that it is likely that he did commit further offences. But individuals should only be punished for offences that have been proved beyond reasonable doubt. They should not be punished for offences of which they were not convicted (or even acquitted, see *Watts* above) just because they were convicted of another offence.

For various reasons, one may argue that drug importation should be punished severely. Be that as it may, this argument can only support increasing the punishment for the captured 427.4g and it should apply equally to other offenders with the same captured quantity regardless of their previous trips. It cannot justify punishing an individual for unproven offences just because s/he commits another offence.

Therefore, when debating the usage of statistical evidence in courts, it is important to remember which fact this evidence is meant to prove. The otherwise-important debate around *Shonubi* focused on the statistical proof of drug quantities. It nevertheless seems to neglect the fact that the mere misconduct of *eight* drug importations (rather than only one) has never been properly proven beyond reasonable doubt, with or without statistical evidence. The question of whether statistical evidence should be allowed to prove "sentence enhancement" facts such as drug quantities seems to depend on whether the individual's misconduct was proven or not. The *Shonubi* case demonstrates the importance of the context in which the statistical evidence is used (sentencing or actually convicting) and the fact it is meant to prove (drug quantities or actually misconduct).

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Williamson on Counterpossibles

A CASE FOR VACUISM

Lewis/Stalnaker semantics has it that all counterpossibles (i.e., counterfactual conditionals with impossible antecedents) are vacuously true. Non-vacuism, by contrast, says the truth-values of counterpossibles are affected by the truth-values of the consequents. Some counterpossibles are true, some false. Timothy Williamson objects to non-vacuism in his third Hempel Lecture (2006), and in Chapter 5 of his manuscript, *The Philosophy of Philosophy*. He asks us to consider someone who answered '11' to 'What is 5 + 7?' but who mistakenly believes that he answered '13'. For the non-vacuist, (1) is false, (2) true:

(1) If 5 + 7 were 13, x would have got that sum right

(2) If 5 + 7 were 13, x would have got that sum wrong

Williamson is not persuaded by the initial intuitiveness of such examples:

... they tend to fall apart when thought through. For example, if 5 + 7 were 13 then 5 + 6 would be 12, and so (by another eleven steps) 0 would be 1, so if the number of right answers I gave were 0, the number of right answers I gave would be 1. (Manu., Ch. 5)

That's the whole argument—much of it implicit. Alan Baker's critique (2007, "A Counter on Counterpossible" *The Reasoner* 1(2): 7-8) of Brogaard and Salerno (2007, "Why Counterpossibles are Non-Trivial" *The Reasoner* 1(1): 5-6.) prompts us to say something less abbreviated about a less abbreviated form of Williamson's argument.

Williamson's above conclusion is (3):

(3) If the number of right answers I gave were 0, then the number of right answers I gave would be 1.

The implicit reductio must be this. If (3) is true, then (1) and (2) are true contrary to what the non-vacuist supposes. For if I gave 0 right answers (in close worlds where 0=1), then I also gave 1 right answer (in those worlds). Hence, I got the sum right and wrong.

Williamson's abbreviated eleven-plus-one steps, we believe, goes like this:

(i) If 5 + 7 were 13, then 5 + 6 would be 12
(ii) If 5 + 7 were 13, then 5 + 5 would be 11
...
(xi) If 5 + 7 were 13, then 5 + -4 would be 2.
(xii) If 5 + 7 were 13, then 5 + -5 would be 1.

Williamson doesn't say how he gets to (3) from here. The reasoning may be that any world where 5 + -5 = 1 is one where 0 = 1, substituting '0' for '5 + -5'. Hence,

(xiii) If 5+7 were 13 then 0 would be 1.

Therefore,

(3*) If 5+7 were 13 (and I gave 0 right answers), then (since 0 would be 1) I would have given 1 right answer.

A REPLY

If this is Williamson's argument, then it's unsuccessful. First, substituting '0' for '5+5' (i.e., step xiii) is illicit, since, as Williamson himself notes (Manu., Ch. 5), the non-vacuous counterfactual is hyperintensional.

Hyperintensional operators do not permit substitutions of co-referring terms *salva veritate*.

Incidentally, Williamson takes hyperintensionality to be a mark against non-vacuism, because substitution is valid in more ordinary counterfactual contexts. However, we need not throw out the baby with the logically ill-behaved bath water. Only non-trivial counterpossible contexts (i.e., counterfactual contexts whose accessibility relation invokes impossible worlds) are hyperintensional. Substitution, and the rest of our logic principles, can be restricted accordingly.

A second related problem for Williamson's position emerges in steps (i) through (xiii). These conclusions hold, if the game is to evaluate the consequent of each at *deductively closed* worlds where 5+7 = 13. But if there are non-trivial counterpossibles, the relevant worlds of evaluation must not be deductively closed—lest they collapse into the trivial world where everything is true.

Once we deny deductive closure, Williamson's reasoning fails. Let the following world, W, be non-deductively closed:

(W): { 5 + 7 = 13, the number of right answers I gave wasn't 1, the number of right answers I gave was 0, ... }

In contexts where W-worlds are closest, (2) is true and (1) false, as the non-vacuist predicts. For Williamson's argument to succeed, however, the relevant impossible worlds in which I gave 0 right answers *and* I gave 1 right answer must be closer than the relevant impossible W-worlds. This hasn't been shown. Indeed, pending further discussion, W seems closer to the actual world than Williamson's impossible world, since W conflicts with fewer salient background conditions.

Using Counterpossibles Non-Trivially

We noted in (2007) that despite his vacuism, Williamson intends a non-vacuous reading of some counterpossibles that he uses. Baker (2007) alleges that we commit the opposite "fallacy". He quotes us:

(6) "If all counterpossibles were trivially true, much of philosophy would be less substantial than it is."

and then argues "according to B & S, (6) is a counterpossible. Thus, in the situation where its antecedent is true all counterpossibles are trivially true, including (6) itself".

Reply: (6) may well be true *at worlds at which its antecedent is true*. But that does not make (6) in fact trivial. (6) is asserted in *this* world, where (ex hypothesi) not all counterpossibles are trivial. Besides, (6) wouldn't be a counterpossible in a situation in which its antecedent is true—so, it needn't be trivially true even if

all counterpossibles would be. Baker's underlying mistake seems to be a failure to distinguish between worlds at which one evaluates counterfactuals and worlds at which they are assigned truth-values. That is, he fails to distinguish world(s) of evaluation from world of utterance. So, we do not make the mistake that we attribute to Williamson.

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Mathematical Blogging

A blogging phenomenon is taking place right now in mathematics, with a flourishing of exposition and discussion. Now this in itself would not be so surprising, as many disciplines have reacted similarly to the advent of blogging software. What is noticeable in mathematics, however, is that not just any old mathematician is becoming a blogger. Indeed, in the past few months three Fields medallists have taken the step. As you may know, a Nobel Prize is not awarded in mathematics, and until the recent introduction of the Abel prize, the analogue of the Nobel was considered to be the Fields Medal. This award is strikingly different, however, in that it is only awarded to people under the age of forty.

In the past few months we find Alain Connes (Medal winner in 1982), Richard Borcherds (1998), and Terence Tao (2006) joining the blogging ranks. Why then the decision of senior mathematicians to take to the internet? This is Connes' opinion:

I guess one possible use of a blog, like this one, is as a space of freedom where one can tell things that would be out of place in a "serious" math paper. The finished technical stuff finds its place in these papers and it is a good thing that mathematicians maintain a high standard in the writing style since otherwise one would quickly lose control of what is proved and what is just wishful thinking. But somehow it leaves no room for the more profound source, of poetical nature, that sets things into motion at an early stage of the mental process leading to the discovery of new "hard" facts.

He then goes on to discuss Alexandre Grothendieck's (1966) notion of "le rêve mathématique" and how preventing its expression leads to sterility. Elsewhere, Grothendieck had written:

There are people who ... are content to shrug their shoulders with a disillusioned air and to bet that all this will give rise to nothing, except dreams. They forget, or ignore, that our

science, and every science, would amount to little if since its very origins it were not nourished with the dreams and visions of those who devoted themselves to it. (251)

More prosaically, it is important that the organising ideas, often buried in the formalism of papers, be exposed. Popular writings, as we see from Sir Michael Atiyah (1966) to Timothy Gowers (1998), could fill this role to some extent, but with the arrival of the blog we have the opportunity to make available an arena for discussion and serious collaborative research. What becomes very clear is that rationality of mathematics is not confined to what constitutes the correctness of proofs, but that a very important part of it is embodied in discussions as to the right way to conceptualise a field, and the best way to take that field forward.

I myself run a blog with two mathematical physicists—John Baez and Urs Schreiber—called The n-category café. Next issue I shall report on the revolutionary new ideas we like to talk about there. You can read about our philosophy of blogging in this post and linked interview, and in my post about it.

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Does direct inference require Pollock's Principle of Agreement?

John Pollock (1990: *Nomic Probability and the Foundations of Induction*, Oxford: Ch. 4) has argued that direct inference requires a presumption that the "reference class" is homogeneous throughout its various subclasses with respect to the probability at issue. More formally, if we know that $\mathrm{prob}(F|G) = r$ and that Gc, we can, on Pollock's view, reasonably infer $\mathrm{prob}(Fc) = r$ only if we accept the additional principle that

(1) $\mathrm{prob}(F|G) = r$ is a defeasible reason to believe that $\mathrm{prob}(F|H) = r$, where $H \subseteq G$.

Pollock's argument focuses on *nomic* probabilities (probabilities fixed by laws of nature), which he equates with proportions among sets of "physically possible objects". For example, if the nomic probability that a particular isotope of thorium will undergo beta decay in 10 minutes is $\frac{1}{2}$, Pollock takes this to mean that *half* of all physically possible thorium atoms of this isotope undergo beta decay within 10 minutes. (See Pollock 1990: 33).

Thus, where '**F**' and '**G**' designate the sets of physically possible F's and G's respectively, and '$\rho(\cdot|\cdot)$' designates the proportion function, Pollock holds that:

(2) $\mathrm{prob}(F|G) = \rho(\mathbf{F}|\mathbf{G})$.

So, (1) becomes:

(3) $\rho(\mathbf{F}|\mathbf{G}) = r$ is a defeasible reason to believe that $\rho(\mathbf{F}|\mathbf{H}) = r$, where $\mathbf{H} \subseteq \mathbf{G}$.

This is dubious. As Pollock acknowledges, the sets **F** and **G** "will almost invariably be infinite, if for no other reason than that there are infinitely many physically possible worlds in which there are F's and G's." (Pollock 2007: "The Y-function", in William Harper and Gregory Wheeler (eds.), *Probability and Inference: Essays in Honour of Henry E. Kyburg, Jr.*, College Publications: 237). It seems unlikely that infinite sets would exhibit the homogeneity that (3) presupposes.

Pollock's defense of (3) rests on an intuitive leap from a set-theoretic fact about *finite* sets, which he calls "the finite principle of agreement" (FPA), to a "strong principle of agreement" (SPA) for infinite sets:

(FPA) For any γ and $\delta > 0$, there is a finite n such that: if B is of cardinality n, then $\rho\big(\rho(A|X) \approx_\delta \rho(A|B)|X \subseteq B\big) \approx_\gamma 1$ (Pollock 2007: 237. Here $W \approx_z Y$ means "W is approximately equal to Y, the difference being at most z".)

(SPA) For any $\delta > 0$, if B is infinite then $\rho\big(\rho(A|X) \approx_\delta \rho(A|B) \,|\, X \subseteq B\big) = 1$.

Pollock argues that (SPA) is warranted because "principles that hold for finite sets in the limit (as their size goes to $\aleph_0$) hold for infinite sets." (Pollock 2007: 237).

To test this argument, consider the claim:

(4) $\rho(x \text{ is even} \,|\, x \in \mathbb{N}) = \frac{1}{2}$,

for the set $\mathbb{N}$ of natural numbers. Let $\delta = .05$ and let X_1 be the subset $\{1, 2, 3, 4, 5, 6, 7, 8, 9\}$. Now:

(5) $\rho(x \text{ is even} \,|\, x \in X_1) >_\delta \rho(x \text{ is even} \,|\, x \in \mathbb{N})$,

and there are an infinite number of subsets just like X_1, for example:

(6) $X_2 = \{1, 2, 3, 4, 5, 6, 7, 8, 11\}$,

(7) $X_3 = \{1, 2, 3, 4, 5, 6, 7, 8, 13\}$,

and so on.

Thus, (SPA) can be accepted only if subsets like X_1, X_2, X_3, etc., can be ignored in the assessment of $\rho\big(\rho(x \text{ is even} \,|\, x \in X_1) \approx_\delta \rho(x \text{ is even} \,|\, x \in \mathbb{N}) \,|\, X \subseteq \mathbb{N}\big)$. The only way that I can see to do this is to assume that we are given a measure μ on sets of subsets of $\mathbb{N}$ such that:

(8) $\rho\big(\rho(x \text{ is even} \,|\, x \in X) \approx_\delta$
$\rho(x \text{ is even} \,|\, x \in \mathbb{N}) \,|\, X \subseteq \mathbb{N}\big)$ is equal to:
$\frac{\mu(\{X \,|\, X \subseteq \mathbb{N} \wedge \rho(x \text{ is even } |\, x \in X) \approx_{.05} \rho(x \text{ is even } |\, x \in \mathbb{N})\})}{\mu(\{X \,|\, X \subseteq \mathbb{N}\})}$,

where

$$(9) \quad \mu\Big(\{X|X \subseteq \mathbb{N} \wedge \rho(x \text{ is even} \mid x \in X) >_{.05}$$
$$\rho(x \text{ is even} \mid x \in \mathbb{N})\}\Big) = 0.$$

One could try to motivate this by pointing out that X_1, X_2, X_3, etc., are finite and so should be of "size 0" in comparison with $\mathbb{N}$. But this overlooks that infinite sets like $\{x|x \text{ is even}\}$ also fail to agree with $\mathbb{N}$ to the specified degree of approximation.

Furthermore, (SPA) is supposed to hold for *any δ* arbitrarily close to 0. Thus it must be that for any $\delta > 0$,

$$(10) \quad \mu\Big(\{X|X \subseteq \mathbb{N} \wedge \rho(x \text{ is even} \mid x \in X) >_{\delta}$$
$$\rho(x \text{ is even} \mid x \in \mathbb{N})\}\Big) = 0,$$

and *only*:

$$(11) \quad \mu\Big(\{X|X \subseteq \mathbb{N} \wedge \rho(x \text{ is even} \mid x \in X) =$$
$$\rho(x \text{ is even} \mid x \in \mathbb{N})\}\Big) = 1.$$

In short, the viability of (SPA) demands that only subsets that *exactly agree* with B with respect to the proportion at issue can count in the assessment of $\rho\big(\rho(A|X) \approx_{\delta} \rho(A|B) \mid X \subseteq B\big)$. And this appears to be a purely *ad hoc* (even question-begging) stipulation. It seems then that the rationality of direct inference cannot hang on a presumption that the reference class is homogenous throughout its various subclasses with respect to the probability in question. What then warrants such inferences? My suspicion is that, far from resting on a presumption of homogeneity, they rest on an understanding that the reference class picks out the "right" or the "relevant" categorization of the individual at hand, so that *even if* there are subclasses that fail to agree with the reference class with respect to the probability in question (and presumably there are), and *even if* that individual is a member of one or more of these subclasses (and she may well be), this fact is deemed irrelevant in the assessment of the definite probability that is taken to attach to the individual in the conclusion of the inference.[4]

<div align="right">
Stephen Fogdall
Schnader Harrison Segal & Lewis LLP
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The Pirahã language, the language template, and the mind

In a series of publications, Daniel Everett (see Nevins, A., Pesetsky, D. & Rodrigues, C. 2007, 'Pirahã Exceptionality: a Reassessment', lingBuzz, here and here, and included references; and Colapinto, J. 2007, 'The

Interpreter' *The New Yorker*, April 16:118-137) proposes that the Pirahã language exhibits certain anomalies. These include: No syntactic embedding; No names for colors; No names for numbers; Smallest pronoun inventory of any language; Smallest phoneme inventory of any language; No creation myths or fiction. Everett proposes that minimalism in the Pirahã language is caused by minimalism in Pirahã culture as-a-whole; and that human language is thus generated by culture—not determined by a biologically or genetically fixed template.

Everett's most interesting single claim is that the Pirahã language has no syntactic embedding of the type 'The man drinking coffee is seated at his desk', where the phrase 'drinking coffee' is embedded into the sentence 'the man is seated at his desk'. Since embedding ('recursion') has been a central object of study in linguistics over the past fifteen years or so, and since embedding is critical in Chomskyan theory, Everett's claim has drawn considerable notice (see Nevins et al. 2007; Colapinto 2007).

Nevins and colleagues (2007:12) point out that a language as familiar as German lacks certain kinds of embedding ('*Hansens Autos Motor'); and that even if Pirahã lacks some kinds of embedding, this may be the result of some syntactic rule, as in German. On this basis, Nevins et al. argue in favor of a biologically determined template for language.

What, then, is the status of embedding in language? Embedding may be obtained from simple sentences by stringing them together to obtain the same meaning, then deleting repeated material, thus. (Deleted material is shown between square brackets.)

> The man [is] drinking coffee. [The man] is seated at his desk.

Once there are sentences in language, it is easy to obtain embedding by what appears to be the historical process in language change. But how sentences were obtained in the first place, and why we have sentences rather than something else, remains largely a mystery (but see Abler, W.L. 2006, 'The Nature of Language', *Science Progress*, 89(1):61-70).

People use language in social situations, both real and imagined. But, since such situations do not obviously generate the structures of language, and since language is remarkably stable both over space and time, we may accept the template hypothesis. What, then, is the content of the template? Sentences are certainly part of it because they amount to a kind of language postulate; but embedding, at least in surface structure, is not, even if it is a language universal. Embedding must have emerged as a result of the historical process, after the origin of language, but before the African exodus.

Four conclusions suggest themselves. First, there is indeed a language template, although that is not to say

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that it is of biological origin, or that it is coded in the genes. Next, even language universals are not inevitably part of the language template, i.e., some of them may have arisen through the historical process. Third, while Everett's hypothesis in its strong form (that language is socially determined on an ongoing basis) is not supported, a weak form of Everett's hypothesis may nevertheless be true (that social forces may help to prune away universals that are not part of the template). And, last, that the correct theory of language will predict that sentences are part of the language template, but that embedded clauses are not part of the template. (For such a theory, see Abler, W.L. 2005, *Structure of Matter, Structure of Mind*, Pensoft, Sofia; Bainbridge, Philadelphia; and Abler, W.L. 2006.)

While everyone seems to agree that language and mind are somehow related, opinion concerning the degree and, to some extent, the kind of relatedness, falls along a sliding scale that goes from no relatedness to complete relatedness. At the center of the scale, language and mind remain distinct, yet capable of interacting, like the ocean and the land. Such theories include the idea that the content of language influences the content of the mind (Whorf, B.L. 1956, *Language, Thought, and Reality*, MIT, Cambridge), or that the content of the mind influences the content of language (Everett, ibid). Next, there is the view that inner speech is closer to thought than ordinary speech (Sokolov, A.N. *Inner Speech and Thought*, Plenum, New York), or that language and mind remain distinct, but interact intimately, like respiration and circulation (Chomsky, N. 1972, *Language and Mind*, Harcourt Brace, New York). At the extreme of the scale is the idea that language and mind are alternate manifestations of the same thing, like electricity and magnetism; and that the human concept of truth has its basis in formal properties of the ancestral sentence (Abler, 2005; 2006). The views mentioned above are not necessarily mutually exclusive.

<div align="right">William L. Abler
Geology, The Field Museum</div>

§3
NEWS

Evidence in Medical Decision-Making

On June 4, the Evidence Project hosted a one-day workshop on Evidence in Medical Decision-Making (a joint event with the 16 May workshop organised within the Contingency and Dissent in Science project at the CPNSS, LSE). In line with the tradition of the Evidence Project, we aimed at bringing together scientists and philosophers to engage into an interdisciplinary dialogue. It was a very interesting day—invited speakers

delivered different perspectives on the general topic of the workshop.

The two talks on randomised controlled trials (RCTs) addressed the problem from very different angles. Instead of debating whether RCTs are, if at all, the gold standard of causal inference, Paul Glasziou made the case for the value and usefulness of observational studies in providing, in certain circumstances, good evidence for causal relations. David Teira, on the other hand, analysed the criteria that, historically, led to accept results of RCTs, and raised the question of whether Bayesian RCTs meet, nowadays, those criteria. Matt Williams presented an innovative view of combining argumentation and Bayesian nets methodology in medical reasoning, especially for prognosis. The joint work of Gianluca Baio and Philip Dawid reviewed the problem of health economic assessment from the standpoint of Bayesian statistical decision theory. Finally, Julian Reiss presented work in normative philosophy of medicine addressing the controversial problem of neglected diseases and of the responsibility the big pharma has in this issue. Titles, abstracts and slides are all available on the Evidence website.

I would like to stress again the interdisciplinary character of the event. Challenging as it may be, reasoning about a problem can prove successful only if informed by different backgrounds, and within such an eclectic environment, the presentation of innovative and controversial ideas and approaches can be fruitful and fecund.

<div align="right">Federica Russo
Philosophy, University of Kent</div>

Contingency in Science: Its Origins and Outcomes

The *Contingency in Science: Its Origins and Outcomes* Workshop was held June 21-22 at the Centre for Philosophy of Natural and Social Sciences at the London School of Economics. The workshop (part of the AHRC funded Contingency and Dissent in Science project) aimed to explore several key issues. The central focus was 'Contingency in Science', which is the idea that the way science is done and its results may depend on factors which 'could have been different'. In the current post-positivist state of philosophy of science, determining the relationship between evidence and theory and how these depend on contingent factors is central. As suggested in the title, the aims of the workshop were two-fold, to explore how contingency comes about and to analyse its implications.

Speakers at the workshop included Dr James McAllister (Philosophy, University of Leiden) who presented a thorough philosophical analysis of the concept of contingency in science, with the central idea is that contingency arises when claims are not fully determined and

not necessary. The workshop also had policy-oriented talks, for instance, that of Dr Eileen Munro (Social Policy, LSE) who spoke of the selective use of evidence in policy-making. While Merete Konnerup (Nordic Cambpell Center) presented a review of the difficulties in the evaluation of policy, focusing particularly on randomized control trials and metanalyses, Prof Michael Marmot (Epidemiology, UCL) presented an important and influential case study that argued for the role of status in determining health. Linsey McGoey (BIOS, LSE) presented a sociological analysis of the recent controversial case of anti-depressants and suicide in adolescents, looking at how the institutional frameworks influenced the behaviour of key actors in the case. Sophia Efstathiou (Philosophy, University of California, San Diego) presented an original analogy between 'found art' and science in order to shed light on how some scientific ideas are developed by being imported from a non-scientific domain. Dr Damien Fennell (CPNSS, LSE) presented a critical analysis of randomized control trials to set out conditions under which these trials can be used to support claims about the population of interest.

The workshop presented an all-too-rare interdisciplinary event, allowing academics from philosophy, social policy, epidemiology and sociology to discuss important issues with each other and with policy practictioners in both the public and private sectors. For more see here.

Damien Fennell
CPNSS, London School of Economics

Formal Epistemology Workshop, Carnegie Mellon University, May 31 - June 3, 2007

Formal epistemology is the philosophical study of knowledge and belief with mathematical and logical means. The last few years have seen a gradual increase of interest in this discipline, and its activities are well worth reporting in *The Reasoner*. A major contribution in the formation of the discipline have been the Formal Epistemology Workshops (FEW), initially organised by Branden Fitelson and Sahotra Sarkar, in Berkeley and Austin and Berkeley respectively. Now FEW has decided to go national. This year's workshop was organised by the Philosophy Department at Carnegie Mellon University, specifically Richard Scheines. The programme of this year's FEW consisted of three invited talks, fourteen contributed talks, and a poster session. All contributed papers were followed by a commentary, typically by a graduate student but incidentally by a senior researcher. The topics of the posters and contributed papers ranged from Bayesian networks and causation to issues in social epistemology, the dynamics of science, confirmation theory, probabilistic logic,

models of coherence, the logic of imperatives, simplicity, curve-fitting, minimum description length, all the way to Euclidean proofs, modal logic, and the psychology of reasoning. It will be clear that describing all these talks will occupy too much space. The reader is referred to the website for abstracts and slides.

Instead I want to sketch the invited talks by Isaac Levi, Frank Arntzenius, and Joseph Halpern. Isaac Levi, the grand old man of formal epistemology, spoke of logical probability and probabilistic logic. He started by sketching his views on the relation between states of full belief and various measures of partial belief, arguing that partial belief is best represented by a "credal state", or a set of probability functions. He then discussed a number of principles that may be adopted in a probabilistic logic concerning these states, such as the convexity of the sets and the restriction that updating is done by conditionalisation. Levi emphasised that the requirement that the credal state is a singleton, thus opting for a notion of logical probability, is quite independent of other requirements making up a probabilistic logic. Logical probability can best be seen entirely separately from probabilistic logic.

Frank Arntzenius spoke about the use of various symmetry principles in the determination of probability assignments. Against the common argumentation that the use of probability for representing epistemic states does not warrant the imposition of symmetry requirements, the message of Arntzenius' talk was rather that there are too many symmetries that epistemic states should satisfy for these states to be adequately represented by probability functions. In a series of characteristically clever examples, involving collections of infinitely many agents spending nights in subsets of infinitely many houses of various colours, using variously coloured telephones to communicate their whereabouts to each other, Arntzenius illustrated this main message. At the end of the talk he briefly sketched how we may be able to deal with all these symmetries, by allowing epistemic states to be represented rather by, yet again, sets of probability assignments.

Joe Halpern, finally, presented his recent work in the foundations of decision theory. He started off by a brief overview of Savage's decision theory, in which models of states, actions, and preferences take centre stage. Against this background Halpern presented his own formulation of decision theory, in which syntactic structures rather than models are central. States are tests, actions are primitive programmes, and so on. Halpern showed that we can obtain the same decision theoretic structures as Savage, but he further argued that the syntactic view has various advantages over the way Savage has set things up. The syntactic theory more easily accommodates changes in preferences and possible states, and the fact that two different programmes may come down to the very same action in Savage's sense leaves

room for dealing with the so-called framing effects that Tversky and Kahneman are famous for. All in all, the Formal Epistemology Workshop was again a great success. There was lots of discussion and a genuine sense of a growing community of formally oriented epistemologists. Plenty of reason to visit FEW next year, when it will be organised at Madison Wisconsin by Peter Vranas and Malcolm Forster. Plans to take FEW to Europe the year after are in the making.

<div align="right">

Jan-Willem Romeijn
Philosophy, University of Groningen

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Calls for Papers

COMBINING PROBABILITY AND LOGIC, special issue of the Journal of Applied Logic, deadline 1 July 2007.

THEORETICAL COMPUTER SCIENCE: Special issue in honour of Jean-Yves Girard on the occasion of his 60th birthday year, deadline 30 September.

SPECIAL ISSUE OF FOUNDATIONS OF SCIENCE: Mathematics and Argumentation, deadline 1 November 2007.

LU: Logica Universalis, Publisher: Birkhäuser Basel

§4
EVENTS

WoLLIC'2007: 14th Workshop on Logic, Language, Information and Computation, Rio de Janeiro, Brazil, July 2-5, 2007.

BSPS: British Society for the Philosophy of Science, Bristol, 5-6 July 2007.

MAXENT 2007: The 27th International Workshop on Bayesian Inference and Maximum Entropy Methods in Science and Engineering, The Saratoga Hotel, Saratoga Springs, New York, USA, July 8-13, 2007.

SCIENTIFIC MODELS: SEMANTICS AND ONTOLOGY: Barcelona, July 9-10 2007.

LCC'07: Workshop on Logic and Computational Complexity, (affiliated with LICS 2007), Wroclaw, Poland, 15th July 2007.

CADE-21: Workshop on Empirically Successful Automated Reasoning in Large Theories (ESARLT), 15th July 2007.

LFMTP'07: International Workshop on Logical Frameworks and Meta-Languages: Theory and Practice, Affiliated with CADE-21, Bremen, Germany, July 16 2007.

LORI: Logic, Rationality and Interaction, Beijing, 5-9 August 2007.

TANCL'07: Algebraic and topological methods in non-classical logics III, 5-9 August 2007, Oxford.

WORKSHOP: Construction and properties of Bayesian nonparametric regression models, Isaac Newton Institute for Mathematical Sciences, Cambridge, UK, August 6-10 2007.

LMPS: 13th International Congress of Logic, Methodology and Philosophy of Science, Beijing, 9-15 August 2007.

IJCNN2007: 2007 International Joint Conference on Neural Networks, Orlando, Florida, August 12-17, 2007.

UNI-LOG: 2nd World Congress and School on Universal Logic, Xi'an, 16-19 August 2007.

C&O:RR-2007: The Third International Workshop on Contexts and Ontologies: Representation and Reasoning, August 21, 2007, CONTEXT Workshop Program, Roskilde University, Denmark.

LSFA'07: Second Workshop on Logical and Semantic Frameworks, with Applications, August 28th, 2007, Ouro Preto, Minas Gerais, Brazil.

ASAI 2007: IX Argentine Symposium on Artificial Intelligence Mar del Plata, Argentina, August 27-28, 2007.

PROGIC 2007

The Third Workshop on Combining Probability and Logic, University of Kent, 5-7 September 2007.

IDA 2007: The 7th International Symposium on Intelligent Data Analysis, Ljubljana, Slovenia, September 6-8, 2007.

SOFT METHODS FOR PROBABILITY AND STATISTICS: 4th International Conference, Toulouse France September 8-10, 2008.

DYNAMICS OF KNOWLEDGE AND BELIEF: Workshop at KI-2007, 30th Annual German Conference on Artificial Intelligence, Osnabrück, 10 September 2007.

DYNAMICS OF KNOWLEDGE AND BELIEF: Workshop at KI-2007, 30th Annual German Conference on Artificial Intelligence, September 10, 2007.

CSL 2007: Computer Science Logic 11-15 September, 2007, Lausanne (CH).

AIPL-07: Workshop on Artificial Intelligence Planning and Learning, Providence, Rhode Island, September 22, 2007, organized in conjunction with the International Conference on Automated Planning and Scheduling (ICAPS-07).

SYMCON'07: The Seventh International Workshop on Symmetry and Constraint Satisfaction Problems, Providence, RI, USA, September 23rd 2007.

ICAPS 2007: Workshop on Planning in Games, Providence, Rhode Island, USA, September, 23, 2007.

SPRING BAYES 2007: The 4th annual meeting of Australasian Society for Bayesian Analysis (ASBA) will take place in Coolangatta, 26-28 September, 2007.

TBILISI: The Seventh International TBILISI Symposium on Language, Logic and Computation, 1-5 October 2007.

REASON, INTUITION, OBJECTS: The Epistemology and Ontology of Logic, Buffalo, 13 October 2007.

LPAR 2007: Yerevan, Armenia, 15th-19th October 2007.

WOMEN IN MACHINE LEARNING: Orlando, Florida, October 17, 2007.

MWPMW 8: Eighth annual Midwest PhilMath Workshop, to be held at Notre Dame, October 27th and October 28th.

ECSQARU'07: Ninth European Conference on Symbolic and Quantitative Approaches to Reasoning with Uncertainty, October 31, November 1-2 2007, Hammamet, Tunisia.

INTERNATIONAL CONFERENCE ON INFINITY IN LOGIC AND COMPUTATION: 3-5 November 2007, University of Cape Town, South Africa.

WORKSHOP: 3rd Workshop on Uncertainty Reasoning for the Semantic Web, Busan, Korea, November 12, 2007.

EPSA07: 1st Conference of the European Philosophy of Science Association, Madrid, 15-17 November 2007.

GRADUATE CONFERENCE: 1st Cambridge Graduate Conference on the Philosophy of Logic and Mathematics, 19th-20th January 2008 St. John's College, Cambridge.

REDUCTION AND THE SPECIAL SCIENCES: Tilburg Center for Logic and Philosophy of Science, 10-12 April 2008.

WORKSHOP: XVIII Inter-University Workshop on Philosophy and Cognitive Science, Madrid, April 22nd - 24th 2008, luis.fernandez@filos.ucm.es.

ISBA08: 9th World Meeting of the International Society for Bayesian Analysis (ISBA), Hamilton Island, Australia, 21st-25th July 2008.

§5
JOBS

POSTDOC: Natural Language Processing and Machine Learning Post-Docs at The Cognitive Computation Group at the University of Illinois at Urbana/Champaign, rbking@uiuc.edu.

POSTDOC: Rensselaer Polytechnic Institute, Human-Level Intelligence, Laboratory at the Rensselaer Department of Cognitive Science, cassin@rpi.edu.

CALL FOR RESEARCHERS: Basque Foundation for Science, Institute for Logic, Cognition, Language, and Information at Donostia / San Sebastian, Spain.

ONE-YEAR FIXED-TERM POSITION: Minnesota State University, Mankato, MN. Philosophy of Science & interdisciplinary Cognitive Science Program, deadline July 1, 2007.

ASSISTANT LECTURER AND RESEARCHER: Department of Philosophy, University of Konstanz, Closing date: 2 July 2007.

FELLOWSHIP: Cognitive Science and Robotics in the Centre for Cognitive Neuroscience and Cognitive Systems at Kent, Closing date Friday 6 July 2007.

LECTURESHIP: Department of Mathematics and Statistics, University of Otago, New Zealand, specialising in statistics, closing date Tuesday 31 July 2007.

§6
COURSES AND STUDENTSHIPS

Courses

MASTER PROGRAM IN INTELLIGENT SYSTEMS: University of Lugano in collaboration with IDSIA, Switzerland, Enrolment deadline: 1 July 2007.

RESEARCH MASTER IN LANGUAGE, COGNITION, ACTION, AND MIND STUDIES: The Institute for Logic, Cognition, Language, and Information of the University of the Basque Country (Donostia-San Sebastian).

SUMMERSCHOOL: 19th European Summer School in Logic, Language and Information, Dublin, Ireland, Aug. 6-17.

LOGIC SUMMER SCHOOL: Italian Association of Logic and its Applications (AILA), Italian Society for Logic and Philosophy of Science (SILFS), Palazzo Feltrinelli, Gargnano, Italy, 26 August - 1 September 2007.

FORMAL METHODS IN PHILOSOPHY AND LINGUISTICS: Summer School 2007, August 19 - August 31, Tartu, Estonia.

SECEVITA 2007: Summer School in Artificial Life and Evolutionary Computing, 31 August – 4 September 2007, Baia Samuele, Ragusa, Italy

SECOND INDIAN WINTER SCHOOL ON LOGIC: January 14-26, 2008, IIT Kanpur.

Studentships

PH.D. STUDENTSHIP IN COMPUTER SCIENCE: The Individual and Collective Reasoning group, deadline July 15, 2007.

PHD POSITION: Emmy Noether junior research group Formal Epistemology, 27 month position for a PhD student in Formal epistemology, deadline July 15, 2007.

BSPS DOCTORAL SCHOLARSHIP IN PHILOSOPHY OF SCIENCE, closing date 1st August 2007.

PHD STUDENTSHIP IN STATISTICS: Fully funded PhD Studentship in Statistics, Umea University, Northern Sweden, deadline August 23 2007.

Acknowledgements