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sor, both based at the University of Pittsburgh, who spoke at the event. Both of my interviewees have contributed to a wide range of topics in philosophy of science, being perhaps best known for developing an interventionist theory of causal explanation (Woodward), and a pluralist framework for understanding explanation and causality in the study of complex systems (Mitchell). Both practice a style of philosophizing that directly engages with science, while relying somewhat less on conceptual analysis as traditionally understood. Following such an approach, both have produced formidable results and insights about how scientific knowledge is produced and put to use in policy, providing a toolkit for analyzing as well as appraising scientific knowledge and its applications. Thus I was keen to get to hear their thoughts on the methodology of philosophy of science, broadly understood, and many other topics that came up in the discussion. Many thanks to my interviewees, the organizers of PragMaps, and *The Reasoner* for providing an opportunity for this.

VELI-PEKKA PARKKINEN
University of Kent

EDITORIAL

It is a great pleasure to act as the guest editor for *The Reasoner's* August issue. This issue's feature focuses on philosophy of science and its relations to general epistemology, metaphysics and scientific practice itself. These themes were discussed at the Pragmatism at the Intersection of Metaphysics and Philosophy of Science (PragMaps) workshop held at the University of Oslo 4th–5th June. I was in the audience and got a chance to talk to Sandra Mitchell, professor, and James Woodward, a distinguished profes-



FEATURES

Interview with Sandra Mitchell and James Woodward

Veli-Pekka Parkkinen: How did you first become interested in philosophy of science, and specifically of the special sciences such as biology and the social sciences?

Sandra Mitchell: I was first interested in epistemology generally. This led me to philosophy of science, as science is our best strategy of gaining understanding of nature. I was particularly attracted to the study of social and political issues that are incredibly complex. One of the strategies that social scientists employ to study such issues is to adopt language that comes from evolutionary biology, such as the idea of

functional explanation. In a way I got into philosophy of biology through thinking why these conceptual tools seem to work so well in biology, but seem to not work quite so straightforwardly in explanation of cultural and social behavior. This drew me to a comparison between how biology and social sciences deal with the complexity they encounter in their fields.

James Woodward: I was a mathematics major as an undergraduate. I did take some philosophy courses, but in retrospect I didn't have much philosophical education as an undergraduate. The school that I went to had a number of faculty that were heavily into Whitehead, and there was very little representation of analytical philosophy more generally. In fact I wasn't even aware that there was such a subject as philosophy of science. I knew later that



I did not want to go on to graduate school in mathematics, and was sort of casting around what to do. Part of what then attracted me to philosophy was the fact that philosophy is a kind of a license to stick your nose into just about anything you are interested in. From very early on I was interested in issues having to do with explanation. For example, when one reads history—something I've always enjoyed doing—one obtains a sense of intelligibility or understanding of the past, and I was very interested in how that worked. What then drew me into philosophy of science was the discovery of literature on the nature of explanation in history which in turn led me to an interest in explanation in science. Like Sandy (Mitchell) I've also always had an interest in the social sciences, partly out of activist, public policy kinds of reasons, such as how to evaluate various kinds of economic theorizing.

V-PP : A quick follow up to this—I feel at times that biology attracts more philosophical attention, as it provides in a way a more coherent target of analysis. Biologists by and large agree on the basic, domain-framing concepts and methods, and internal dispute occurs within this framework. By contrast, in the social sciences even the question of what is it that social scientists ought to study is subject to debate. Would you agree on this characterization?

SM: I would say yes and no. There is a sense in which there is large scale consensus in biology of what evolution is, for instance, and what molecular components are contributing to biological processes. On the other hand, there are some deep disputes within biology too, for example disputes about what it is that drives evolution. So although there is consensus on the basic framework of evolution, there are important debates about what forces are most important in driving evolutionary processes. There may be a difference between biology and social sciences due to the fact that in studying culture and social behavior, we are dealing with an additional level of complexity which opens up more room for debate about correct classification, for instance. But I wouldn't say there is a vast difference in kinds of dispute, it is more of a difference in degree.

JW: I would largely agree with Sandy. I would like to add,

though, that I think it is exactly those areas where there is lot of disputing going on that I think philosophers of science can be most useful. I see the unsettled character of a lot of issues in sociology or economics or portions of psychology as an opportunity for philosophers of science.

SM: In addition, when it comes to the social sciences and psychology, we have a first-person perspective and access into the issues in these domains, and the fact that we have this access that we cannot escape I think makes issues both more interesting and more complicated.

JW: Yes, certainly in psychology and even neurobiology, I think there is a very deep question about the extent to which we can get away from our first person, folk categories, or whether we even ought to. To a surprising extent, for example in cognitive neuroscience, investigation is carried out in terms of categories that are really quite commonsensical.

V-PP: How about the role of philosopher of science in relation to scientific practice, do you think that philosophy of science should be useful for scientists?

JW: I guess I wouldn't impose that as a criterion in the sense that philosophy of science has to be useful to be worthwhile at all, but I do think it is one of several goals that philosophers of science should have. Also, for myself I must say that I have learned enormously from collaborating with scientists, and I think it should be a presupposition that you can also bring something to the table that is useful or interesting to the other party when engaging in collaboration.

SM: I think the kind of philosophy of science that directly engages with science has access to, and can formulate a clear understanding of what the goals of science are, and what exactly the problems are that scientists are struggling with. Then, given the kinds of tools philosophers of science have for analysis, conceptual clarification, framing and so forth, I think philosophy of science can engage with science in a way that is useful for both. Often philosophers and scientists are asking what on the surface look like very similar questions, but employ very different tools to try to get at the answers. I think sharing across the fuzzy boundary between philosophy and science can help both sides. Also thinking of the boundary between science and policymaking, philosophers have a perspective that isn't tied to a particular science and can therefore get a more distanced picture of what's going on, and can act in a negotiating or translating role even.



V-PP: Sometimes philosophers of science also criticize particular scientific research programmes or heuristics from a philosophical point of view. Do you think this has value in itself, or should philosophers of science that engage in such criticism have something to offer in place of the research strategies they criticize?

JW: I always think that it is better if you can offer a constructive, positive alternative. I understand that you can't perhaps always do that, but I think that philosophy as a discipline is a little bit too much in a mode of just criticizing and opposing things. I think the challenge should always be to see what you can contribute to a particular debate that would be also constructive and positive.

SM: I think there has been real constructive yet critical contribution from philosophy for instance in the research on various statistical tools for causal reasoning, where scientists can benefit from philosophical analysis of the assumptions that one has to make in order to use these methods, and thereby learn about their possible limitations. Another example I would mention is the work done on the role of randomized controlled experiments in medicine and pharmacology, in which philosophers have engaged in ways that are directly relevant to science, and also at the intersection between science and policy.

JW: Another related area where you see this connection is the role that normative models of causal reasoning have come to play in psychology. Normative models of various sorts, construed as descriptive models of human causal cognition, have had a big influence within psychology, and some of those normative models come at least in part out of work done by philosophers.

V-PP: How do you think philosophy of causality, specifically, should be done? I have two somewhat caricatured pictures in mind about how one might think of this. One would be to start with a metaphysical framework that is independently argued for, and proceed to regiment the use of causal concepts based on that. The other would be to take some parts of causal reasoning at face value, as paradigmatic examples of good causal reasoning, and proceed to analyze the notion of causation underlying those cases.

SM: I think it's a dialectic, it's not just choosing one or the other. I think we start with some provisional views on what causality is, drawing for example from the proposals made in history of philosophy, and proceed to see how those views might be in evidence (or not) in causal reasoning in science. I also think there are multitude of ways that certain events or properties come about in nature, which might call for different analyses of how to properly think about them causally. Think for instance about the question of whether natural selection is a cause: it influences evolution by selecting and eliminating, rather than by directly producing something. It is a very powerful explanatory tool but does not share a feature that some other, paradigmatic examples of causes have. So I think the scientific practice can challenge some of the traditional philosophical views, and that's an opportunity to revise and improve those views.

JW: I'd say I don't fully agree with either of the pictures you presented. I certainly would reject the metaphysical starting point. But like Sandy I don't think the correct way is to be purely descriptive either. I think the way to proceed is first of all to formulate some set of goals that are associated with causal reasoning, and then to ask, does some particular set of means lead to those goals or not? This introduces the

possibility of a critical dimension to the whole inquiry. I'm willing to be a pluralist about the goals: I have particularly focused on predicting the outcomes of interventions as the goal of causal reasoning, but there may be other possibilities as well. I guess I am a little skeptical of the idea of just starting with some set of paradigmatic cases as we first want to know what it is that is good about those paradigmatic cases.

V-PP: Do you think we could do a kind of a functionalist analysis of causation—just identifying causation with whatever role it plays in our inferences—without having to say anything about what metaphysically speaking grounds that role?

SM: I'm a little bit suspicious of attempts to say on what metaphysical grounds some scientific practice is successful. However, you can say for instance that for causal reasoning to work you must assume that the system of interest exhibits some stability features, and then ask in virtue what the system has those features. But I think the interesting questions of this type are amenable to empirical analysis, and so the idea of trying to find the deepest most fundamental metaphysical grounds is undermotivated.

JW: I would certainly agree with that. I think it is a good question to ask, in a scientific spirit, what is it about some system that permits us to understand it in a certain kind of way. I am quite willing to acknowledge that there may kinds of systems for which causal analysis as ordinarily understood just is not the appropriate way to go. But I see this as a scientific, empirically grounded inquiry, and once it gets beyond that I become very skeptical.

V-PP: Both of you have written on the interventionist account of causality and explanation. What do you think we should make of cases where some of the assumptions that underlie interventionist analyses of causation—such as the causal Markov or modularity conditions—are not satisfied. Can we for instance relax these assumptions without losing too much inferential power?

JW: First of all, at least in the way I understand interventionism, it does not presuppose the truth of the causal Markov condition. To establish the Markov condition you have to make a number of more specific assumptions that go beyond the basic interventionist framework. I did argue in a couple of papers with Dan Hausman that if you make those additional assumptions and you combine them with interventionist ideas, you can derive the Markov condition. But I see the Markov condition as a great deal more specialized. For example, you can perfectly well apply interventionist-style reasoning to systems that contain causal cycles, but there are cyclic systems that don't satisfy the Markov condition. My attitude in general to these issues is that all of these assumptions will break down under some conditions, and the constructive thing to do is to look for ways of weakening the assumptions in such ways that you can still get useful results. That is an ongoing project.

SM: I think there are choices to be made when we recognize a system that fails to meet the conditions for implementing a particular form of causal analysis, and we must look at the consequences of choosing between ways to think about this. We might say that some particular way we reason

about causal systems defines causality, and therefore we must rule that systems where the conditions for applying this reasoning are not met are not causal. Perhaps there are systems for which this would be the right conclusion they are just not exhibiting the features we usefully associate with causal systems. Or we might conclude that we need to expand our notion of causality in some ways, or to acknowledge that there is more than one meaningful notion of causality.

V-PP: Both of you advocate, or at least welcome some form of pluralism of approaches and models as part of good science. Is this pluralism pragmatically motivated—as cognitively limited beings this is the best strategy we have for understanding a very complicated world—or does the pluralism of epistemic practices suggest a metaphysical pluralism of some kind, could we perhaps make a kind of a transcendental inference from the plurality of epistemic approaches to metaphysical pluralism?

SM: I think there are reasons from both domains to support pluralism. I think there is sufficient evidence of diversity of phenomena to justify the need for different models to understand what they are and what they are doing. As far as I am willing to go in terms of a transcendental argument to something that supports that kind of claim of pluralism, I think there is sufficient evidence that there are differences in kinds of things, but I do not see the need to provide a basis for this in terms of different fundamental substances or anything like that. I think a fundamental assumption in natural science is that everything is made of physical stuff. I see this as a metaphysical claim to the effect that an object's chemical properties or biological properties and so on are not unrelated to its physical properties. But this doesn't mean that the physical properties are determinative of, or explain all the other properties. I think this is a less metaphysically loaded pluralism, but one that suggests that there are real differences in nature that require differences in the scientific approaches we employ to understand them. Nature affords multiple ways of describing it, to use Gibson's terminology, and pluralism is required for getting accurate descriptions of nature in order to explain, predict and intervene. Also, I think the institutional structures of science, like replicability and peer review and so on, are designed in response to some human components in generating scientific knowledge, and they try to limit the idiosyncratic features of that. There has been, at least since the 17th century, an assumption that some kind of intersubjective agreement is a better locus of scientific knowledge than the possibly idiosyncratic individual human. So the human component is evident also in the social organization of science.

JW: I would agree with all of that. Given the alternatives you described—is it about us or about the world—I would certainly say both. There are certain kinds of limitations in terms of what information we can get and what kinds of calculations we can do and so forth, and science attempts to work its way around those limitations with various strategies. But it also has to be the case that the world cooperates, as it were. There have to be structures in the world that support those strategies that we develop. So it's because of the fact that there are stable patterns at different levels of scale, let's say, that we can exploit those patterns in the various models that we construct. So I think both of the alternatives you described are true in the sense that there are limitations to the kind of inquiry we

are capable of, but there are structures in the world that enable us to can engage in the kind of pragmatic reasoning strategies that we do engage in. I also think that when you talk about human limitations, you don't need to inject some radically subjectivist or relativist component into the picture. Many of these limitations are just highly general features that have to do with the scale and size of humans as macroscopic agents, which limits our access to certain kinds of information and limits our abilities to manipulate things.

NEWS

The Idea of Pragmatism: in honour of the work of Chris Hookway, 18–19 May

'The Idea of Pragmatism' conference was hosted by the Leverhulme-funded *Idealism and Pragmatism* research network and the philosophy department at the University of Sheffield. The conference celebrated the work of Chris Hookway. Hookway has published widely in philosophy and has books on scepticism, Quine, and cognitive science, but is best known for his work on Peirce and pragmatism. It is thanks in great part to Hookway's work not only that Peirce's place in the analytic philosophy canon has been secured, but also that many philosophers today recognise that his oeuvre constitutes a veritable goldmine for considering numerous live issues in contemporary philosophy.



The conference papers covered a broad range of issues and included a wonderful discussion of Hookway's dialogue with Quine given by Hilary Putnam (Harvard), and an exposition and defence of Frank Ramsey's development of Peirce's pragmatism by Cheryl Misak (Toronto). However, a main theme of the conference focused on Hookway's reconstruction of Peirce's work on truth and inquiry.

Shannon Dea (Waterloo) began the conference by introducing us to the 'two Peirces'. On the one hand, there is the metaphysically inclined and terminologically baffling Peirce, but, on the other, there is the modest, sensible, likeable Peirce that was unearthed by Hookway through his careful scholarship, charitable interpretation, and philosophical acuity. Both Albert Atkin (Macquarie) and Andrew Howat (California State, Fullerton) assessed the contemporary prospects for this second Peirce by focusing on Peircean truth (henceforth, PT). Both clarified PT's position in logical space by presenting it as an alternative to the traditional dichotomy of inflationist or ontological accounts and deflationist or semantic accounts. Atkin discussed Peirce's theory in relation to the work of Huw Price. Price, like many others, has objected to PT because he understands it as suggesting that ' p is true if and only if p is ultimately fated to be agreed upon by a community of inquirers at the end of inquiry'. Price

argues that since PT is held only at some far off point (the hypothetical ‘end of inquiry’), it cannot supply the conversational ‘friction’ necessary for genuine debates. Furthermore, this still seems to be a *metaphysical* account, since truth is a property of some state at the end of inquiry. Both Atkin and Howat showed that there is a more promising account of PT to be found in Hookway’s reading of Peirce. According to this view, truth is not a property of some state at the end of inquiry, but rather it is to be understood as a Kantian ‘regulative ideal’. We *hope*, i.e., virtually assume, that there is an ascertainable truth for any question we investigate, because this is the only way to engage in rational inquiry. However, this is not an indispensability argument, but a *modest* transcendental argument. It assumes truth to get inquiry going but makes no claims regarding what this truth predicate is. Atkin argued that PT is an anthropological rather than an ontological account because it focuses on what speakers who think they are using the truth predicate are *doing*. As it focuses on truth in practice, it says something *substantial* about truth without ‘inflating’ into metaphysics. As Howat stressed, PT is important because it tells us about the normative commitments and responsibilities a speaker takes on when she asserts that p is true. Atkin believed that this could be clarified through speech-act theory and argued that PT should be understood as not a claim about *semantics* but rather *pragmatics*—the illocutionary act—the speech act’s practical dimension.

For Philip Kitcher (Columbia), the pragmatist conception of truth is closely connected to progress. What is true is what emerges as stable when we have made significant *progress*. Progress, therefore, is prior to truth. Accordingly, Kitcher argued that pragmatism’s contemporary prospects depend on a refinement and clarification of the concept of truth and he argued for an understanding of such progress without commitments to ‘end states’, ‘teleology’, and ‘globality’.

Paper titles can be found at the conference [website](#).

JEREMY DUNHAM
University of Sheffield

Meaning and Reference, 19–21 June

This year’s edition of the Bucharest Colloquium in Analytic Philosophy (BCAP 2015), organized by the Romanian Society for Analytic Philosophy, Department of Theoretical Philosophy and the Center for Logic, History and Philosophy of Science, was held between the 19th and the 21st of June at the University of Bucharest, Faculty of Philosophy, and focused on two major topics in the philosophy of language: *meaning and reference*. The 22 speakers, be they professors, post-doctoral researchers or PhD students, were affiliated with universities such as: the University of Alabama-Tuscaloosa, St. Andrews, Barcelona, Belgrade, Bucharest, Edinburgh, Göttingen, Hamburg, Kansas State, Iași, Reading, Turin, Warsaw, Witwatersrand, the City University of New York, the Massachusetts Institute of Technology, and research centers such as Arché, ICREA and LOGOS.

Most of the presentations could be aggregated into groups that tackled themes of philosophical inquiry such as the meaning and the reference of proper names, demonstratives, natural kind terms and artifactual terms, rigid designation, modalities, X-Phi accounts of semantics, and themes from Frege’s philosophy. The keynote speakers were Michael Devitt (CUNY), Manuel García-Carpintero (LOGOS, Barcelona),

Diego Marconi (Turin), Genoveva Marti (ICREA, LOGOS, Barcelona), Benjamin Schnieder (Hamburg), Marián Zouhar (Slovak Academy). Michael Devitt’s talk was concerned with discussing his proposal on what the meaning of a proper name should consist of—namely its causal mode of reference—and the reception his idea had in the philosophical community. Manuel García-Carpintero examined two metalinguistic accounts of proper names: the predicativist and the presuppositional one, and argued in favor of the latter. Diego Marconi showed that there is a sense in which we can make discoveries about artifacts. Genoveva Marti’s presentation compared two different semantic views on general terms stemming from results in the experimental research on semantic intuitions: the hybrid account and the ambiguity approach, and defended the second one. Benjamin Schnieder argued that even though Russell’s Paradox can be posed in terms of properties, our discourse about properties is not inconsistent. Marián Zouhar challenged a common tenet among modal metaphysicians that mathematical definite descriptions are rigid designators.

The 2015 edition of BCAP continued the tradition lasting for a decade now of being a place where young researchers and professionals meet and discuss their contributions and related subjects of interest to analytic philosophy (at previous editions themes pertaining to logic and philosophy of logic, mathematics, physics or the philosophy of Frege and Russell were addressed).

BIANCA SAVU
ALEXANDRU DRAGOMIR
University of Bucharest

Formal Ethics, 3–5 July

An international conference on Formal Ethics was held at the University of Bayreuth, Germany, from July 3–5. It was the third of a series of conferences; previous Formal Ethics conferences took place in Rotterdam (2014) and Munich (2012). The aim of the conference series is to provide an international platform for the discussion and advancement of formal approaches to ethics. Research done in this field applies a wide range of tools from, e.g., logic, rational choice theory and natural language semantics in order to tackle problems in ethics and political philosophy, and to further conceptualise notions and theories. Formal Ethics 2015, with the headline ‘Practical Reasoning & Responsibility’, comprised 20 contributed and invited talks by scholars from universities in Europe, Australia, the US and China. They covered a number of tools and formalization methods and touched on diverse topics, including utility aggregation, responsibility, cooperation and team agency, bargaining, and normative sufficiency.

One Keynote Lecture was given by Rudolf Schüßler (Bayreuth) on Ethics, Nash and Meta-Bargaining. He first presented the thesis that (fair) bargaining requires that all sides make concessions. Formalizing the requirement, Schüßler showed that the Nash bargaining solution satisfies this requirement in first-level bargaining, but not in meta-bargaining, i.e., in bargaining over bargaining solutions. He then argued that the Nash-solution is not acceptable as a meta-bargaining solution; it does not make concessions in higher level bargaining, while other bargaining solutions do.

On the second conference day, Natalie Gold (King’s College London) gave a tutorial on Team Reasoning. She explained how team reasoning leads to rational cooperation by agents in

a team and how problems in game theory, e.g., the Prisoner's Dilemma and the Hi-Lo Paradox, can be solved by appealing to team reasoning, rather than individual reasoning. Further, Gold presented the idea of modelling problems of self-control as intra-personal dilemmas in which the agents are the transient versions of an individual at different points in time. Modelled this way, she explained, team reasoning can lead to following intentions over time. After Gold's tutorial, there was a series of 5-minute talks and a poster session which gave young researchers and graduate students the opportunity to present and then discuss their work with conference participants.

On the third day, Fenrong Liu (Tsinghua University) closed the conference with the second Keynote Lecture on Priority Structures in Deontic Logic. She presented the result of joint work with Johan van Benthem and Davide Grossi: a framework that allows modelling standard betterness (ideality) orderings as well as the underlying reason. Liu discussed how they use priority graphs to formalize contrary-to-duty scenarios/sets of norms. The resulting framework supports information dynamics and norm change in deontic scenarios, both of which entail deontic obligation change. Liu explained how the dynamics work at different levels and considered some tracking results. She pointed out that there are also complete dynamic logics matching the framework.

Overall, the conference was considered very engaging and inspiring among the roughly 70 participants. For researchers, it offered the opportunity to connect approaches and ideas and to stretch the boundaries of the research on formal ethics on an international level. High student participation over the three days showed that there is a growing interest in the field. The organizers are looking forward to the next Formal Ethics conference. Please refer [here](#) for the full programme of Formal Ethics 2015 and more information on the conference series.

FRANZISKA POPRAWE
Philosophy, Bayreuth

Calls for Papers

PROBABILISTIC BELIEFS: special issue of *Theory and Decision*, deadline 1 October.

UNCERTAIN REASONING: special issue of *Journal of Applied Logic*, deadline 15 October.

REASONING, ARGUMENTATION, AND CRITICAL THINKING INSTRUCTION: special issue of *Topoi*, deadline 30 October.

WHAT'S HOT IN . . .

Uncertain Reasoning

Readers of this column will know that I favour the subjective foundation of probability. My preference is rooted in the robustness of the argument which leads to tying—with a double knot—the informal notion of “rational degrees of belief” with the fundamental properties of the calculus of probability. In analogy with the Church-Turing Thesis, the Dutch Book Argument pivots on formal results but it cannot be encapsulated in a statement which one can hope to be able to (dis)prove with mathematical rigour. For at one end of the knot lies the intrinsically non-formal notion of rational belief. So you can argue about the consequences of its identification with probabil-

ity, you can check it against your intuition, you can set up experiments to test the predictions of the model, but there's little you can demonstrate formally about the identification of rational belief with probability (*pace* the currently popular literature which aims at proving various semi-formal claims about “probabilism”, (im)precise probabilities, the Dutch Book Argument and so on.)

Like all attempts at formalising informal concepts, the Dutch Book Argument requires serious abstraction. Arguably more abstraction than the one which leads to identifying the informal notion of computation with the activity of a Turing machine. The abstraction leading to the Dutch Book *theorem* essentially enables us to reduce the problem of assigning rational degrees of belief (to well-defined events) to the problem of exhibiting *consistent* preferences in well-defined betting problems, where the latter notion is axiomatised rigorously. De Finetti, who made substantial contributions to this argument referred to the corresponding property of degrees of belief as *coherence* and insisted for about five decades that coherence is the only logical constraint that degrees of probability should obey. This, in essence, is the point of view which is sometimes labelled as strict subjectivism.

Strict subjectivism isn't very popular in contemporary epistemology. Many of those who agree that coherence is a necessary condition for capturing an abstract formalisation of rationality, reject the claim that no further constraint should be imposed on the rationality of degrees of belief. In recent years Jon Williamson contributed to reviving the interest in the problem by defending an *objective* view of Bayesian epistemology, see J. Williamson (2010: *In Defence of Objective Bayesianism*, Oxford University Press). In a nutshell Williamson argues that on top of coherence, subjective probabilities should satisfy two further norms termed Calibration and Equivocation, respectively. The first states, roughly, that *if* objective chances (or physical probabilities) are available for the problem at hand, degrees of probability should be consistent with them. This may or may not lead to a unique probability distribution being consistent with the available information. If it doesn't, then the second norm recommends choosing the (unique) probability distribution which is closest to the so-called “equivocator”, the minimally informative distribution consistent with the available information. Two well-known instantiations of those norms are Lewis's Principal Principle and the Principle of Maximum Entropy, which in the special case of “no information available”, coincides with the uniform distribution. Quite interestingly the two additional norms are justified in (Williamson 2010) by appealing to essentially the same argument used by de Finetti, namely the identification of rationality with avoiding sure loss (in suitably defined problems). This clearly makes a case for strict subjectivists to rethink their own strictness.

An even stronger case for that is provided by the recent paper by J. Hawthorne, J. Landes, C. Wallmann, and J. Williamson, (2015: “[The Principal Principle Implies the Principle of Indifference](#),” *The British Journal for the Philosophy of Science* Ad-



vance Access 1 July 2015). As the title anticipates, the authors prove that under rather mild conditions one cannot coherently endorse the Principal Principle while resisting the temptations of the uniform distribution. As noted by the authors, this has two important consequences. The first effectively amounts to a *ju jitsu* move, for the much criticised Principle of Indifference gains epistemological credit just by climbing on the shoulders of the much endorsed Principal Principle. The second interesting consequence is a refinement of the many distinct varieties of Bayesianism (of which I.J. Good counted 46656 interpretations). Indeed an immediate implication of this paper is the epistemological untenability of the so-called empirically based Bayesianism, quite popular in statistics, and which roughly corresponds to the endorsement of Calibration without Equivocation.

HYKEL HOSNI

Marie Curie Fellow,
CPNSS, London School of Economics

Evidence-Based Medicine

The ongoing debate over the use of statins has been covered extensively in the [mainstream media](#). Recently, [Sally Davies](#), the chief medical officer, has expressed concern that the lack of resolution to debates such as this is damaging public confidence in medicine. In response, [Davies concluded](#) that what is needed is “an authoritative independent report looking at how society should judge the safety and efficacy of drugs as an intervention.” As a result, the Academy of Medical Sciences has begun a working group project on [Evaluating evidence](#). The aim of the project is to “explore how evidence that originates from different sources (e.g., randomised clinical trials and observational data) are used to make decisions about the safety and efficacy of drugs and medical interventions.”

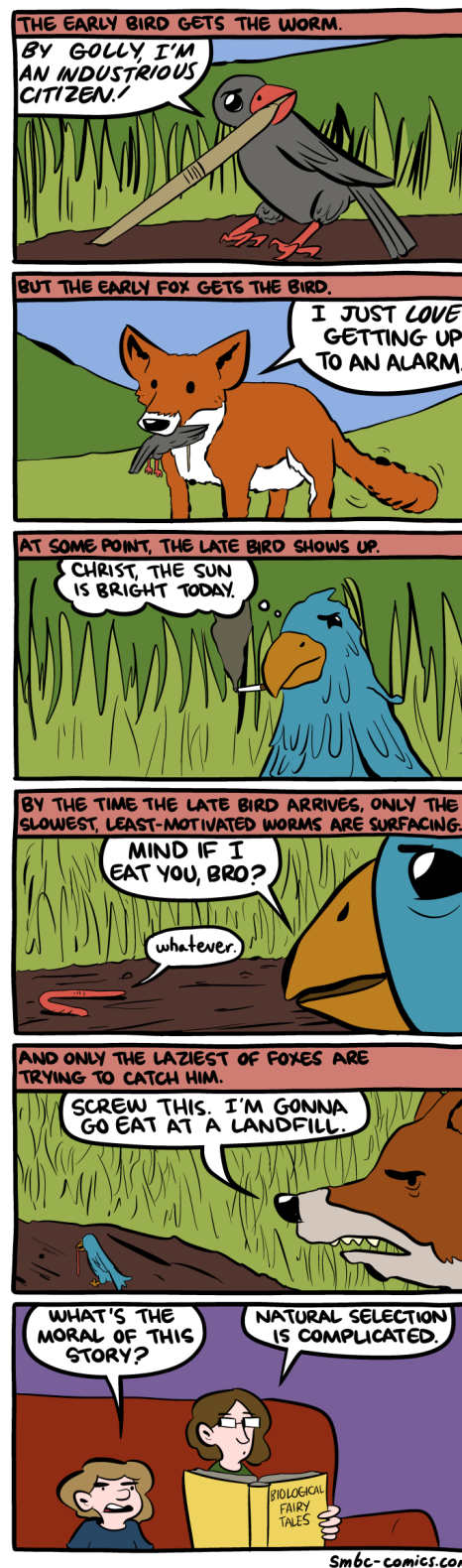
More recently, in a [BMJ editorial](#), [Ben Goldacre](#) and [Carl Heneghan](#) have expressed concern that this working group project may suffer from a lack of ambition, at a time when medicine has a real opportunity to effect necessary changes in how evidence in medicine is evaluated. They conclude:

The public is increasingly aware of the shortcomings we collectively tolerate in the evidence base for clinical practice. We now have the opportunity to use public frustration as fuel to update our implementation of evidence based medicine in the light of new technology and get our house in order.

They argue that it would be recklessly backward looking to only focus on the interpretation of inadequate existing data instead of making real fixes to evidence-based medicine. In addition, they provide a number of proposals for how to fix evidence-based medicine, e.g., addressing publication bias, the costs of independent trials, and encouraging better evidence.

In the meantime, another [Evaluating evidence in medicine](#) project is ongoing, with the project kick-off workshop taking place last month. Among other talks, Phyllis Illari (UCL) spoke about dealing with fears about mechanisms, Mike Kelly (Cambridge) spoke about the role of biological and social mechanisms in the development of guidelines, and Christian Wallmann (Kent) spoke about the reference class problem.

MICHAEL WILDE
Philosophy, Kent



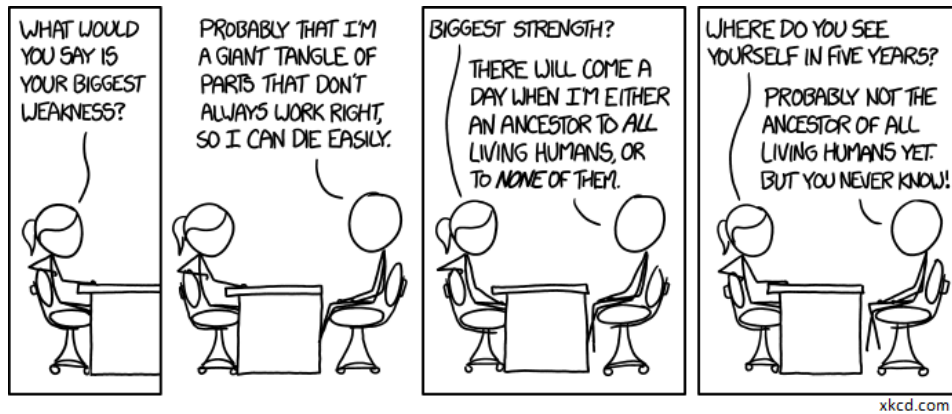
EVENTS

AUGUST

[AD](#): Automated Deduction, Berlin, 1–7 August. [CLMPS](#): 15th Congress of Logic, Methodology, and Philosophy of Science, Helsinki, 3–8 August.

[EPI](#): Epistemology Workshop, University of Helsinki, 11–12 August.

[MSS](#): Modelling for Social Sciences, London School of Economics, 17–21 August.



SEPTEMBER

ITA: 6th International Conference on Internet Technologies & Applications, Wrexham, North Wales, 8–11 September.

EPSA: 5th conference of the European Philosophy of Science Association, Heinrich Heine University, Düsseldorf, 23–26 September.

OCTOBER

LORI: 5th International Conference on Logic, Rationality and Interaction, Taipei, Taiwan, 28–31 October.

COURSES AND PROGRAMMES

Courses

COMBINING PROBABILITY AND LOGIC: University of Kent, 20–21 April.

EPICENTER: Spring Course in Epistemic Game Theory, Maastricht University, 8–19 June.

EPICENTER: Mini-course on Games with Unawareness, Maastricht University, 22–23 June.

Programmes

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

MASTER PROGRAMME: MA in Pure and Applied Logic, University of Barcelona.

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

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

MASTER PROGRAMME: in Statistics, University College Dublin.

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

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

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

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

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

MA PROGRAMMES: in Philosophy of Science, University of Leeds.

MA IN LOGIC AND PHILOSOPHY OF SCIENCE: Faculty of Philosophy, Philosophy of Science and Study of Religion, LMU Munich.

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

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

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

MA IN PHILOSOPHY: by research, Tilburg University.

MA IN PHILOSOPHY, SCIENCE AND SOCIETY: TiLPS, Tilburg University.

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

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

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

MRES IN METHODS AND PRACTICES OF PHILOSOPHICAL RESEARCH: Northern Institute of Philosophy, University of Aberdeen.

MSc IN APPLIED STATISTICS: Department of Economics, Mathematics and Statistics, Birkbeck, University of London.

MSc IN APPLIED STATISTICS AND DATAMINING: School of Mathematics and Statistics, University of St Andrews.

MSc IN ARTIFICIAL INTELLIGENCE: Faculty of Engineering, University of Leeds.

MA IN REASONING

A programme at the University of Kent, Canterbury, UK. Gain the philosophical background required for a PhD in this area. Optional modules available from Psychology, Computing, Statistics, Social Policy, Law, Biosciences and History.

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

MSc IN COGNITIVE SYSTEMS: Language, Learning, and Reasoning, University of Potsdam.

MSc IN COGNITIVE SCIENCE: University of Osnabrück, Germany.

MSc IN COGNITIVE PSYCHOLOGY/NEUROPSYCHOLOGY: School of Psychology, University of Kent.

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

MSc IN MIND, LANGUAGE & EMBODIED COGNITION: School of Philosophy, Psychology and Language Sciences, University of Edinburgh.

MSC IN PHILOSOPHY OF SCIENCE, TECHNOLOGY AND SOCIETY: University of Twente, The Netherlands.

MRES IN COGNITIVE SCIENCE AND HUMANITIES: LANGUAGE, COMMUNICATION AND ORGANIZATION: Institute for Logic, Cognition, Language, and Information, University of the Basque Country (Donostia San Sebastián).

OPEN MIND: International School of Advanced Studies in Cognitive Sciences, University of Bucharest.

JOBS AND STUDENTSHIPS

Jobs

POST DOC: in History & Philosophy of Science, University of Notre Dame, deadline 15 August.

ASSOCIATE PROFESSOR: in Probability Theory, University of Copenhagen, deadline 25 September.

Studentships

PHD POSITION: in Theoretical Philosophy, University of Oslo, deadline 1 September.