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about absolutely everything there is), plural and higher-order quantification and paradoxes. These topics are intertwined. For instance, some of the most powerful arguments for adopting languages equipped with plural and higher-order quantification rest on absolute generality. In turn, some of the most powerful arguments against absolute generality come from paradoxes, especially those of Russell and Burali-Forti.

Agustin, Gabriel, and Øystein have developed their views on these issues in the same philosophical context. In the interview, I try to find out more about their philosophical influences and the new directions of their research. Hopefully, this will give the reader an insight into their work and into some key debates in philosophical logic and philosophy of mathematics.

SALVATORE FLORIO

Kansas State University

EDITORIAL

It is a great pleasure to be this month's guest editor of *The Reasoner*. I am grateful to [Agustín Rayo](#), [Gabriel Uzquiano](#), and [Øystein Linnebo](#) for accepting to be interviewed. In the last fifteen years, they have made influential contributions to a variety of areas, including philosophical logic and philosophy of mathematics, and their work has had a significant impact on many philosophers of my generation working in these fields.

Our conversation centers on their shared research interests. We discuss topics such as absolute generality (i.e., the thesis that it is possible to talk and theorize



FEATURES

Interview with Agustín Rayo, Gabriel Uzquiano, and Øystein Linnebo

Salvatore Florio: You met as graduate students in Cambridge, Massachusetts, in the second half of the 1990's. It must have been an exciting time and place to be trained as a young philosopher. What was it like? What were your main influences?

Agustín Rayo: I can certainly see in retrospect how lucky I was to have been in Cambridge then. But none of that was obvious at the time. At the time it just felt like ordinary life, except that I was constantly in awe of the people around me.

Øystein Linnebo: Yes, we were certainly lucky. Having two great philosophy departments—at Harvard and MIT—only a couple of miles apart was particularly nice. Each department had its own, very distinctive character. Harvard favored a more “big picture” approach to philosophy, often combined with a

vast knowledge of the history of philosophy. MIT was much more focused on tight argumentation. Graduate students at Harvard sometimes made fun of the MIT style, claiming that it consisted of deductively valid arguments (although perhaps formulated in plural logic...) from premises that the Harvard gang would regard as supported by nothing but vulgar appeals to intuition. But I have to say many of the most rewarding philosophical discussions of my graduate student days were with MIT people, not least when the “big picture” Harvard approach became oppressive for someone who had only quite recently converted from mathematics to philosophy.

AR: Harvard-MIT interactions played a big role for me too. The Intuitionism Reading Group, which was jointly organized by Harvard and MIT students, was super influential in my intellectual development. Although the group started out focusing on intuitionism in mathematics, it quickly branched out to other topics. We read a lot of work by Dummett, Lewis, Stalnaker and Parsons, and these four authors have continued to influence my thinking.

ØL: Charles Parsons was a big influence on graduate students at Harvard who were interested in logic and philosophy of mathematics, including myself. And although George Boolos had passed away just months before I arrived in 1996, his influence was still strongly felt, especially at MIT. My sense is that graduate students were often quite loyal to the philosophers they perceived as “their own”.

Gabriel Uzquiano: I’m not sure I realized at the time how lucky I had been to have a chance to work with Boolos. He certainly made an impression on me, and he certainly remained an important influence in my work for time to come. So did Richard Cartwright and Vann McGee, and I still remember the excitement of working with the first two on my second-year review paper sometime before McGee’s arrival to MIT. I remember learning from Boolos that MIT had just hired another philosophical logician from Rutgers, Vann McGee. For a while it looked like Boolos and McGee would both be in the same department, which made for an exhilarating prospect. Unfortunately, Boolos passed away before McGee’s arrival in MIT. Soon enough, however, Vann became a central part of the department and an intellectual role model for many of us. I think my first contact with the Harvard approach must have been through Michael Glanzberg, who soon became one of the members of my dissertation committee. Glanzberg had just completed his graduate work at Harvard, and I remember I learned a great deal from my interactions with him. I soon realized I had much to learn from Charles Parsons’ writings, and his work has been on my mind ever since.

AR: Boolos had passed away by the time I arrived at MIT, but, as Øystein says, his presence was very much felt. I learned much of my Boolos through Gabriel, who was a few years ahead of me at MIT. Gabriel and I used to talk a lot about absolute generality and plural quantification, and these discussions led to our joint paper ‘Towards a Theory of Second-Order Consequence’. Many of the ideas I worked on during the next few years were based on philosophical tools that Gabriel and I developed as part of that project.

GU: Yes, those were very exciting years, and much of my



Left to right: Øystein, Agustín, and Gabriel

work for the first few years into my career derived from those interactions. Even after I left MIT for Rochester and Agustín moved to St Andrews, we had a few more chances to interact and I spent my first and one of my most productive sabbatical semesters in Archè.

SF: The problem of absolute generality plays an important role in your work as well as in the work of some of your teachers (e.g. Boolos, Cartwright, McGee, and Parsons). Interestingly, your views on the problem differ.

ØL: I started out defending a Parsons-inspired form of generality relativism, according to which the phenomenon of indefinite extensibility could be used to break out of any fixed interpretation of the quantifiers in favor of an even more inclusive interpretation. But I was also very concerned about Tim Williamson’s misgivings about generality relativism. I tried to reconcile these opposing pressures in my contribution to Agustín and Gabriel’s great anthology, *Absolute Generality*. Writing this article took more time and caused me more pain than any other article I have ever written! In the article I try to hold on to the phenomenon of indefinite extensibility—in the sense that any definite collection of sets, propositions, or things in general can be surpassed by an even larger such collection. Simultaneously, I tried to make sense of the idea that there can be indefinite totalities, such as that of all sets or all things. The logical manifestation of this indefiniteness takes the form of restrictions to various sorts of comprehension principles, e.g., principles concerning which pluralities or concepts there are. I find it amusing that I started out with a very Harvard-like view on the matter, but ended up with a much more MIT-like view in which a defense of absolute generality plays a central role.

AR: And I’ve moved in the exact opposite direction. I started out with a very Boolos-oriented picture, and have ended up defending a view very similar to Parsons’.

SF: What made you change your view?

AR: It happened in two stages. I first convinced myself that the very same arguments that had led me to embrace plural quantification could be used to argue for the legitimacy of an open-ended hierarchy of languages of greater and greater logical power. I then came to believe that endorsing an open-ended ideological hierarchy of this kind isn't so different from endorsing an open-ended ontological hierarchy. (Øystein and I develop this idea in our joint paper, 'Hierarchies Ontological and Ideological'.)

SF: Gabriel, how close is your view to that of Boolos?

GU: Boolos has clearly been an important influence in my work, especially my early work on classes, but the focus of my research eventually shifted to a different set of issues in ontology. The hypothesis of absolute generality remained a crucial presupposition for some of the puzzles I went on to address in some of my work in metaphysics and modal metaphysics. But I never thought of the problems as a serious challenge for the prospect of absolutely general theorizing but rather as a cautionary note over the scope of certain domains of inquiry such as mereology. I realize, though, that others might be inclined to construe them as a threat to the availability of absolute generality.

One area in which I seemed to have come to differ from Boolos concerns my take on plural and higher-order quantification. Some of my recent work has led me to take the language of higher-order logic at face value as an intelligible interpreted language not subject to reinterpretation in more familiar terms. Moreover, I prefer to keep plural and higher-order quantification separate. In fact, I prefer to think of the status of the theory of plural quantification as more closely aligned with mereology than to quantificational logic: I think it is best to regiment the theory of plural quantification in an interpreted first-order two-sorted language, and while I'm inclined to believe all instances of impredicative plural comprehension, I'm no longer sure they have the epistemological credentials philosophers often associate with the logic of quantification and identity. A principle of plural extensionality may seem more secure, but I have recently been tempted to explore non-extensional fragments of the theory of plural quantification, which are formally not very different from a non-extensional mereology.

ØL: Something I greatly admire in the work of Boolos is the way technical and philosophical considerations are woven together. This is certainly something I try to emulate in my own work.

SF: How does his view on plural and higher-order quantification relate to yours?

ØL: The status of plural and higher-order logic was a central bone of contention in debates between Harvard and MIT people. Viewed from the point of view of Harvard, MIT was full of philosophers whose enthusiasm for plural logic was far too uncritical. Although Quine was quite a shadowy presence in the department by the time I arrived, the weight of his views was still strongly felt. While I still think the contrast

between plural logic and set theory is often overstated, I now don't hesitate to admit that plural logic can be a very valuable philosophical tool.

SF: Is there anything that, philosophically speaking, you wish you had learned or discovered earlier?

ØL: Agustin mentioned our Harvard-MIT reading group on intuitionism. As far as I recall, we actually spent very little time on intuitionism. We mostly found the philosophical psychology and radical anti-realism in which this approach is stooped infuriating. It is only very recently that I've come to appreciate how there are valuable technical and conceptual ideas in this tradition which can be separated from the features we found so annoying. I'm thinking particularly of the analysis of potential infinity and the pressure that this idea exerts in the direction of intuitionistic logic. More generally, I wish I had studied even more mathematical logic and more physics.

GU: I wish I had paid more attention to the role of impredicativity in different generalizations of Cantor's theorem. In particular, I wish I had realized earlier the role impredicative class comprehension plays in class-theoretic generalizations of Cantor's observation that there is no one-one function from the set of subsets of a set (Not 1–1) and in contrast to class-theoretic generalizations of the claim that there is no function from the members of a set onto the set of its subsets (Not Onto), which require nothing more than predicative class comprehension. (Quite ironically, this distinction is at the core of Boolos's last paper 'Constructing Cantorian Counterexamples'.) The role of impredicativity is important because different class-theoretic forms of Cantor's theorem are involved in the demise of Frege's Basic Law V, the Russell-Myhill paradox of propositions (Not 1–1), and some versions of Kaplan's paradox of propositions (Not Onto). Of course, impredicativity has played a prominent role in discussions of Frege's Law V, but I think that its role (or lack thereof) in some other cases deserves some further scrutiny.

AR: When I first learned the ways of analytic philosophy I tended to think that Carnap had been refuted by Quine. I have since come to believe that many of Carnap's insights are robust enough to survive Quine's critique. It seems to me, in particular, that it is a mistake to think that Carnap's views are inextricably linked to the notion of analyticity that Quine objected to. As a result, I have come to believe that Carnap's work has a lot to teach us about how metaphysical questions can be tied up with linguistic questions. I wish I had come to see this earlier.

SF: My last question is about the new directions of your research. What do you see as exciting open problems?

GU: To my mind, one important set of open problems concerns the nature of intensional paradoxes like the Russell-Myhill paradox of propositions, Kaplan's paradox, and generalizations thereof. There is, in each case, a subtle connection between them and different forms of suitably generalized forms of Cantor's theorem, but it seems to me that they are not well understood. Some of these problems, e.g., Kaplan's paradox, have often been obscured by the prominence of cardinality considerations to the effect that however you conceive of

a possibility, there must be strictly more propositions than possibilities. But I think that cardinality considerations fail to reach at the root of the problem. Kaplan's observation, for example, is sometimes glossed as the claim that, on pain of contradiction, some proposition is such that it is not possible for it, and only it, to be entertained. But when supplemented with the rule of necessitation, the logic of propositional quantification entails that some proposition is such that it is not possible for it, and only propositions materially equivalent to it, to be entertained. Whatever the problem is in this case, it has little to do with considerations of cardinality.

ØL: I agree with Gabriel that Russell-Myhill style paradoxes deserve more attention than they have received. There are very deep and general problems here. Under what conditions is it permissible to transition from many objects to a unique individual object (such as the proposition that these many objects exist)? On my view, it matters greatly whether the many objects are given extensionally, say as a plurality, or intensionally, say by means of some defining condition. My hope is that we can motivate restrictions to plural and higher-order comprehension axioms which will make the world safe for the mentioned transitions. Another great problem, which deserves more attention, is whether the phenomenon of indefinite extensibility can provide any help with the liar and other semantic paradoxes.

AR: I have become increasingly interested in understanding the nature of the relationship between our language and the world it represents. By building on ideas that go back to Frege and the later Wittgenstein, I have come to believe that language only makes contact at the world at the level of sentences. (A little more precisely: I think that atomic sentences with different logical forms can be used to accurately describe the same feature of the world, and therefore that it would be a mistake to assume that a sentence can only accurately describe a fact if the logical form of the sentence is somehow in sync with the "ontological structure" of the fact.) This sort of view has been around for some time, but I'm interested in reviving it to address certain contemporary problems in metaphysics and the philosophy of mathematics.

How paradoxical is Fitch's paradox?

I. In order to produce Fitch's paradox (Fitch 1963: 'A logical analysis of some value concepts', *Journal of Symbolic Logic* 28(2): 135–142), we only need these very intuitive premises, where 'K(x)' means 'x is known by someone at some time', for any x:

- P1. If a conjunction is known, each of its members is known: $K(p \& q) \rightarrow K(p) \& K(q)$.
- P2. Knowledge is factive, i.e., only what is the case can be known: $K(p) \rightarrow p$.
- P3. Tarski's T-schema: $T(p) \leftrightarrow p$.
- P4. Whatever implies a contradiction is impossible: $(p \rightarrow q \& \neg q) \rightarrow \Box \neg p$

One easily derives this conditional:

$$(1) T(t) \& \neg K(t) \rightarrow T(t \& \neg K(t)) \& \Box \neg K(t \& \neg K(t)).$$

For assume the antecedent of (1). From P3 you get the first conjunct of its consequent. You can prove the second conjunct by deriving a contradiction from $K(t \& \neg K(t))$ in this way:

- 1'. $K(t \& \neg K(t))$ assumption
- 2'. $K(t)$ from P1 and 1'
- 3'. $K(\neg K(t))$ from P1 and 1'
- 4'. $\neg K(t)$ from P2 and 3
- 5'. $K(t) \& \neg K(t)$ from 2' and 4', P

From this derivation and P4, you can infer the second conjunct of the consequent of (1).

This reasoning seems to show that, if there is a truth that was, is and will remain unknown, then there is a proposition that is true and unknowable:

$$\exists x (T(x) \& \neg K(x)) \rightarrow \exists x (T(x) \& \Box \neg K(x)).$$

Most of us are prepared to accept that some truths will remain forever unknown but few of us take this to be a necessity. Let us first show that Fitch's argument has no bearing on this.

Let us use square brackets to denote propositions outside formulas: let $[x]$ denote, for any x , the proposition expressed by "x" within formulas. Note that, even if we accept that $[t \& \neg K(t)]$ is unknowable—i.e., known in no possible world—it is not shown that it is true at all possible worlds; it might well be that, for some possible world w_i , $[t \& \neg K(t)]$ is not true at w_i , either because $[t]$ fails in w_i or because $[t]$ is not forever unknown in w_i . So, Fitch's reasoning doesn't prove:

$$\exists x (T(x) \& \neg K(x)) \rightarrow \Box \exists x (T(x) \& \neg K(x))$$

Hence, it is not shown that, if some truth remains forever unknown, then necessarily some truth remains forever unknown. Despite this, Fitch's theorem seems incompatible with a cherished Principle of Knowability, stating that every truth is knowable even if it in fact remains forever unknown:

$$(KP)T(p) \rightarrow \Diamond K(p)$$

for if $[t]$ is forever unknown, then $[t \& \neg K(t)]$ is true and, in the presence of 1'–5', it seems unknowable.

II. But is it?

There is something odd about the unknowability of $[t \& \neg K(t)]$ as usually interpreted, namely, that we interpret that unknowability as known in no possible world and at the same time we leave unspecified the world for which K is meant to denote known: does 'K' mean 'known in the actual world', with 'actual' a rigid designator, or 'known in the actual world' with 'actual' denoting in each possible world w_i what is actual in w_i ? Difficulties with the modal aspects of Fitch's reasoning have been already discussed (see e.g., Edgington 1985: 'The Paradox of Knowability', *Mind* 94(376):557–568; Kvanvig 1995: 'The Knowability Paradox and the Prospects for Anti-Realism', *Noûs* 29(4): 481–500; Williamson 2000: 'Tennant on Knowable Truth', *Ratio* 13(2): 99–114; Slater 2011: 'The Problem with Fitch', *The Reasoner* 6 (10): 9–11). I'll show, without going into the details of the discussion, how modal considerations might alleviate the paradoxicality of Fitch's case.

Dispositions in Science, 20–21 April

The context in which the notation was introduced suggests that ‘K’ has been used as meaning known in the actual world. This is so because, $[t]$ was never supposed to necessarily or possibly remain forever unknown but to *actually* remain so.

Let ‘ w_k ’ rigidly denote the actual world and let $[t]$ be a true proposition that will remain forever unknown in w_k . Is the notation ‘ $t \& \neg K(t)$ ’ appropriate for modal speech? What would it mean for other people in some other possible world w_i , with $i \neq k$, to come to know what we have assumed to be true, namely, $[t \& \neg K(t)]$? Would it mean to know $[t]$ and that $[t]$ will remain forever unknown in their world? Or would it rather mean to know $[t]$ and that $[t]$ remains forever unknown in w_k ? For we cannot assume that ‘ $[t]$ remains forever unknown in w_i ’ and ‘ $[t]$ remains forever unknown in w_j ’ express the same proposition when $i \neq j$.

Using modal speech without relativizing predicate ‘K’ to the possible world at stake seems incongruous. In order to avoid world-ambiguity, we should replace the predicate ‘K’ by the family of predicates $\langle K_i \rangle$, with subscripts corresponding to the indices in the family $\langle w_i \rangle$ of possible worlds. Then we note that the proposition $[t \& \neg K_k(t)]$ need not be unknown in all possible worlds even if $[\neg K_k(t)]$ is true: there may be some w_i , with $i \neq k$, where $[t \& \neg K_k(t)]$ is known. Let w_j be such a world; assume $[\neg K_k(t) \& K_j(t \& \neg K_k(t))]$ to see that our reasoning leads now to no contradiction:

- | | |
|--|------------------|
| 1". $\neg K_k(t) \& K_j(t \& \neg K_k(t))$ | assumption |
| 2". $K_j(t)$ | from P1, P", PL. |
| 3". $K_j(\neg K_k(t))$ | from P1, P", PL. |
| 4". $\neg K_k(t)$ | from 1", PL. |
| 5". $K_j(t) \& \neg K_k(t)$ | from 2", 4", PL. |

Which is no contradiction.

Though there is some difficulty regarding the way in which inhabitants of w_i could individualize w_k in order to know truths about it, nothing in Fitch’s premises prohibits the existence of a possible world where $[t \& \neg K_k(t)]$ is known. It follows from $[\neg K_k(t)]$ that

$$\Box \neg K_k(t \& \neg K_k(t))$$

but not that

$$\forall i (K_k(t \& \neg K_k(t))),$$

which is what we would usually mean by

$$\Box (\neg K(t \& \neg K_k(t))).$$

If knowability is understood as possibility to be known in some possible world, then Fitch’s reasoning only seems to refute (KP) when our concept of known remains world-ambiguous.

LAUREANO LUNA

Philosophy. IES Doctor Francisco Marin.

This workshop was held at the University of ‘Roma Tre’ on April 20–21, organized by the local philosophy of science research group. The workshop brought together philosophers of science and metaphysicians working on the metaphysics of dispositions.

The first speaker was Andrea Borghini, who spoke about ‘Vindicating the Dispositional Theory of Possibility’. The main idea is that what is metaphysically possible is grounded in the dispositions instantiated in the actual world. Borghini defended this position from some important criticisms, and claimed that all those objections could be answered within a dispositional account of metaphysical possibility. Second and last speaker of the first day was Anjan Chakravartty, who gave a talk on ‘Symmetry, Dispositions, and Explanation’. Chakravartty addressed an important question concerning the role of symmetries in the explanation of physical phenomena and considered as an example the Standard Model of elementary particles. The formalism of the theory describes the fundamental properties of elementary particles as invariants of certain symmetry group transformations. However, Chakravartty claimed that this description doesn’t take into account the ontology of these fundamental properties and their relation to the entities or phenomena which they are associated to. Then, Chakravartty opposed two different approaches to the relevant explanation: a ‘top-down’ approach, starting from mathematical considerations to arrive at an explanation of the nature of properties; and a ‘bottom up’ approach, reflecting on the nature of properties and then describing their mathematical representation. The first approach would consider symmetries as providing a kind of ‘structural explanation’. The latter would lead instead to a dispositional metaphysics as an explanation of the nature of particle properties according to the Standard Model, and to consider symmetry groups as a mathematical tool to represent such metaphysics.

The second day opened with the talk of Simone Gozzano on ‘Necessitarianism and Dispositions’. Gozzano discussed an important objection to necessitarianism (that is, the idea that causal relations underpinning dispositions are necessary) and showed that this objection is not conclusive. In particular, Gozzano discussed the test of antecedent strengthening, which fails if causal relations are considered as necessary. Even though this is usually taken as an argument against necessitarianism, Gozzano claimed that this is not the case, given that this test may actually be accepted by necessitarians and rather be considered as a test for causal relevance. The second speaker was Andreas Hüttemann, who spoke about ‘Dispositions and Conditional Metaphysical Necessity’. Hüttemann started from the important assumption according to which law statements ascribe dispositional properties to physical systems, and then analysed the modal connection between dispositions and their manifestations, by focusing on the form of law statements and their role in explanations. After the lunch break, the stage was for Stephen Mumford and Rani Lill Anjum, whose talk was on ‘Dispositionalism: A Dynamic Theory of Causation’. Mumford and Anjum claimed that ‘causal dispositionalism’ offers an alternative to the prevalent metaphysics according to which the world consists of a mosaic of facts and events at spatiotemporal points connected by causal relations. Considering examples from the biological domain, they claimed that “causation

occurs in a continuously changing process rather than a relation between static relata” and this is exactly what their ‘causal dispositionalism’ says. Mumford and Anjum then discussed some advantages of their proposal over the prevalent view. The final speaker of the workshop was Neil Williams who explained us ‘Why I’m Not a Dualist’, in a very aesthetical way indeed, by using pegs and rubber bands painted on his slides. Even though there are some examples from science and metaphysics that seem to support a dualist position about properties, Williams claimed that fundamental dualism about properties is not a viable option, since powers cannot make room for a distinct category of properties additional to dispositions.

The workshop ended with a round table that allowed all the speakers and the audience to continue the discussion and enrich the debate created by the two days of this very interesting workshop.

EMANUELE ROSSANESE
University of Roma Tre

Explanation and Abduction, 7–8 May

On Thursday the 7th and Friday the 8th of May, the Centre for Logic and Philosophy of Science at Ghent University marked its twentieth birthday by hosting the workshop ‘Explanation and Abduction: Logico-Philosophical perspectives’. This was the first in a series of workshops entitled ‘Logic, Reasoning, and Rationality’, that the centre plans to organize twice a year.

While both explanation and abduction have been much discussed by philosophers, their combination has received far less attention. In all, fifteen papers were presented. Obviously, these cannot all be discussed in this brief report, so I will confine myself to the most salient presentations, and the ones that most explicitly addressed both topics in combination.

In the first keynote lecture, Leen de Vreese of Ghent University addressed the relation between explanation and scientific understanding in medicine from the standpoint of explanatory pluralism. While recent approaches differentiate between explanation and understanding by pointing to the role of the cognizing subject and context dependency within the latter, she argued that if one takes into account the epistemic interests underlying explanations, the issue is not so much what kinds or types of understanding can be distinguished, but rather in what ways cognizing subjects can achieve understanding. In the next presentation, Diderik Batens, emeritus professor at Ghent University, took issue with the notion of practical abduction (a notion he admitted of having introduced himself), arguing that all abductive reasoning is theoretical, i.e., consists of deriving potential explanantia from generalizations and theories. As the logics underlying these generalizations and theories may be different in nature, abductive reasoning requires the general statements it employs to have an implicative connection that exhibits maximal specificity. Pace the received view, this requirement does not apply to singular statements—a confusion that may well stem from the erroneous notion of practical abduction.

In the second keynote lecture, Igor Douven of the Paris-Sorbonne University drew on experimental evidence to show that in accounting for people’s explanatory inferences, relying on the tools of Bayesianism alone is insufficient. Instead, judgments about explanatory goodness have to be included. This suggests that inference to the best explanation (IBE) is actually a scientifically bona fide notion, and so goes against the

often heard criticisms of IBE that it has never been satisfactorily explicated, and that any explication of IBE should reduce to Bayes’ rule.

In the third keynote lecture, Gerhard Schurz of the Heinrich Heine University of Düsseldorf argued that in accounting for learning new hypotheses from new evidence, neither AGM-style belief revision, nor belief base revision is successful. He developed an account of input-driven abductive belief expansion and revision more suited to this task. This account describes abductive expansion and revision functions in the domains of inductive generalization, factual abduction, and theoretical model abduction. A result of this approach is that abductive belief revision does not satisfy the Levi-Identity.

All in all, the participants felt that it had been an inspiring and engaging workshop—a fitting start for the workshop series ‘Logic, Reasoning, and Rationality’.

RAOUL GERVAIS
Tilburg University

Calls for Papers

REASONING ABOUT PREFERENCES, UNCERTAINTY, AND VAGUENESS: special issue of *IfCoLog Journal of Logics and their Applications*, deadline 21 June.

CRITIQUING TECHNOLOGIES OF THE MIND: special issue of *Phenomenology and the Cognitive Sciences*, deadline 30 June.

LOGIC THEOREMS: special issue of *Logica Universalis*, deadline 31 July.

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

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

WHAT’S HOT IN . . .

Uncertain Reasoning

According to a variously attributed quote (from Confucius to Niels Bohr, to baseball manager Yogi Berra) “Prediction is very hard, especially if it is about the future”. Yet, it is the art of prediction that “models” aim at making scientific. Dramatic model failures are all-too-easy to pick, and one of my personal favourites relates to *the* financial crisis. On 13th August 2007 B. Bonner commented (in the *Financial Times*) as follows on Goldman Sachs’s failure to see the credit crunch coming:



“We were seeing things that were 25-standard-deviation events, several days in a row,” said David Viniar, CFO of the smartest financial firm in the world, Goldman Sachs. According to Goldman’s mathematical models, August, Year of Our Lord 2007, was a very special month. Things were happening that were only supposed to happen once in

every 100,000 years. Either that ... or Goldman's models were wrong".

Putting the blame on "models" is not so straightforward, albeit very popular, in the currently much talked about pre-election poll failure in the UK. Virtually every analyst and commentator, from the academia to politics to finance, agreed that the outcome of the 2015 UK Parliamentary Election would have resulted in a coalition government (again) owing to the expected tie between Labour and Conservatives. Much to everyone's surprise this forecast was wildly wrong, as the Tories secured a sound victory with a 6.6% lead. Hence something went wrong with the vast majority of the forecasts made with pre-election polls. Naturally, many are now trying to understand what exactly *did* go wrong, and why.

David Spiegelhalter tackles the problem with the usual clarity on his *Understanding Uncertainty* blog, in a post titled [Was anyone right about the pre-election polls?](#) According to Spiegelhalter, who speaks openly in favour of the [electionforecast](#) model, "models" *tout court* are not to be blamed. He in fact reports that betting agents also got it similarly wrong (unlike their betting odds for the Scottish independence referendum). To subjectively-minded statisticians the bookmakers' large-scale failure does call for further inquiry. In essence, Spiegelhalter suggests that, unlike exit polls, pre-election polls suffer from clear design and data-collection issues. This clearly prompts an explanation which, as the statistician reports, is currently being investigated by the British Polling Council.

I wholeheartedly recommend interested readers to turn to Spiegelhalter's post for the details of his own analysis and many interesting pointers to academic as well as media discussions on the matter.

[HYKEL HOSNI](#)
Marie Curie Fellow,
CPNSS, London School of Economics

Evidence-Based Medicine

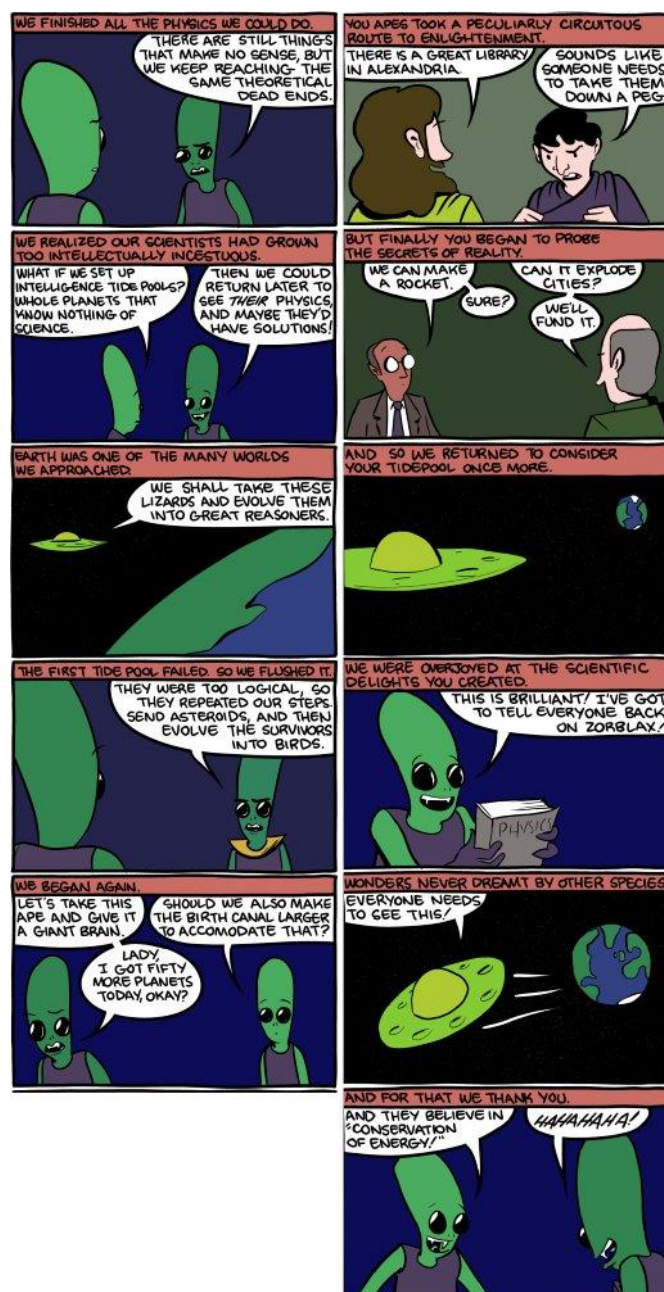
This month sees the publication of a [philosophy themed edition](#) of the [Journal of Evaluation in Clinical Practice](#). The edition contains a number of articles on the nature of health, disease, diagnosis and care, and a section on rethinking medical epistemology. There is also a debates section, which is made up of responses to papers from a previous edition of the journal. The topics discussed there include the nature of causality and also whether evidence-based medicine is failing as a result of industry contamination of research. I recommend that interested readers of *The Reasoner* take a look at the edition.

One article in the edition is by [Luis Flores](#) (King's College, London) and is on [Therapeutic inferences for individual patients](#). Flores focuses on the issue of applying the probability estimates provided by clinical trials to individual patients. It is generally acknowledged that these generic estimates are not straightforwardly applicable to a particular patient, but that methodological developments in the design and analysis of clinical trials may overcome this obstacle. But Flores argues against the view 'that recent developments in research methodology have resulted in improved clinical trials capable of providing clinicians with probability estimates readily applicable to individuals'. He thinks that clinicians should not rely solely on developments in research methodology in order to address the problem of maximizing the relevance of probability esti-

mates for individual patients. Instead, Flores argues that clinicians should also reflect upon the particular characteristics of each patient. He concludes that the problem of applying probability estimates to individual patients can be addressed by considering a variety of evidence and not just that evidence which results from clinical trials.

This is a conclusion close to the heart of the [Evaluating evidence in medicine](#) project, which is funded by the AHRC and begins this month. This is a collaboration between researchers at UCL, the University of Kent, the University of Amsterdam, NICE, IARC, the Institute of Public Health at Cambridge University, and the Medical School at Leiden University. The project aims to understand how to best consider evidence of mechanisms alongside statistical evidence in medical research and health policy. Interested readers can keep up to date with the project at the [EBM+ blog](#).

[MICHAEL WILDE](#)
Philosophy, Kent



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EVENTS

JUNE

ICCS: International Conference on Computational Science, Reykjavik, Iceland, 1–3 June.

PR & MR: Practical Reasoning and Motor Representation, University of Warwick, 1–3 June.

TTL: 4th International Congress on Tools for Teaching Logic, Rennes, France, 1–4 June.

SR & BCS: Scientific Realism and Basic Common Sense, University of Athens, 2 June.

P & E: Philosophy and Economics, Uppsala, June 8–10.

ECA: Argumentation and Reasoned Action, Lisbon, Portugal, 9–12 June.

TSC: Towards a Science of Consciousness, Helsinki, 9–13 June.

HP TL: Hilberts Epsilon and Tau in Logic: Informatics and Linguistics, University of Montpellier, 10 June.

OiS: Objectivity in Science, Tilburg University, 10–12 June.

PLS: 10th Panhellenic Logic Symposium, Samos, Greece, 11–15 June.

SEL: Studying Evidence in the Law: Formal, Computational and Philosophical Methods, University of San Diego, 12 June.

SF: Scientific Fictionalism, London, 12 June.

PC & S: Propensities, Chances, and Statistics, London, 12–13 June.

GiB: Ground in Biology, Geneva, 19–20 June.

RE: Religious Epistemology, Heythrop College, 19–20 June.

UL: Universal Logic, Istanbul, 20–24 June.

MR: Meaning & Reference, University of Bucharest, 19–21 June.

PoM: Philosophy of Mathematics Conference, University of Oxford, 22 June.

UNILOG: 5th World Conference on Universal Logic, Istanbul, 25–30 June.

LA: Legal Argumentation, Rotterdam, 26 June.

E & L: Emergence and Laws, University of Leeds, 26–27 June.

CaMITS: Causality and Modeling in the Sciences, Madrid, 29 June–1 July.

JULY

AAL: Conference of the Australasian Association of Logic, Sydney, 2–3 July.

CRS: Context-relativity in Semantics, University of Salzburg, 2–4 July.

FE: Formal Epistemology Conference, University of Bristol, 4–8 July.

ICoML: International Conference on Machine Learning, Lille, France, 6–11 July.

AiCI: Advances in Causal Inference, Amsterdam, 16 July.

SR AI: Statistical Relational Artificial Intelligence, Amsterdam, 16 July.

BMAW: Bayesian Modeling Applications Workshop, Amsterdam, 16 July.

CoNR: Conference on Computing Natural Reason, Indiana University, Bloomington, 19–20 July.

ISIPTA: Society for Imprecise Probability, Pescara, Italy, 20–24 July.

MoM: The Making of Measurement, University of Cambridge, 23–24 July.

WLAI: Weighted Logics for Artificial Intelligence, Buenos Aires, 25–27 July.

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.

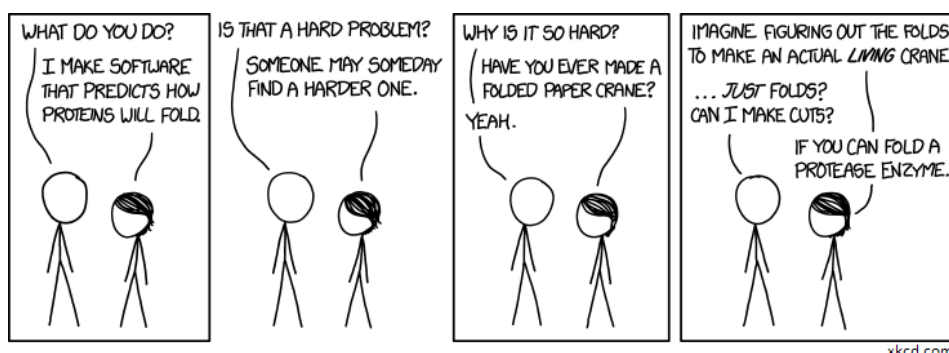
MASTER PROGRAMME: in Statistics, University College Dublin.

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

PROFESSORSHIP: in Statistical Methods in Social Sciences, TU Dortmund University, deadline 10 June.

LECTURER: in Philosophy of Science, University of Nottingham, deadline 17 June.

PROFESSORSHIP: in Statistics, University of Kent, deadline 21 June.

CHAIR: in Metaphysics, Durham University, deadline 16 July.

CHAIR: in Philosophy of Mind, Durham University, deadline 16 July.

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

Studentships

PHD POSITION: in Statistical Modeling, University of Warwick, deadline 1 June.

PHD POSITION: in Statistics and Probability, Durham University, deadline 19 June.

PHD POSITION: in Bayesian Learning Methodology, University of Leicester, deadline 26 June.

PHD POSITION: in Cognitive Irrationality, University of Basel, deadline 30 June.

PHD POSITION: in Scientific Inferences, Tilburg University, deadline 10 July.