# The Reasoner

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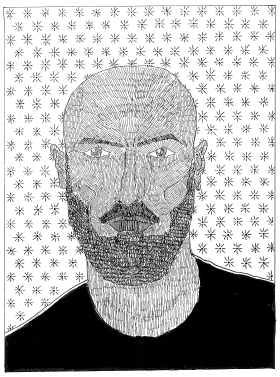
## Contents



## Guest Editorial

When Hykel asked me to be the Guest Editor for this issue of The Reasoner, I was honoured. The honour gradually changed towards delight when I was asked to interview Elias Tsakas, since he was one of my supervisors during my PhD. Elias is associate professor at the School of Business and Economics of Maastricht University and his work ranges from evolutionary game theory, to epistemic game theory, to decision theory, with incursions in logic and experimental economics. Still, the easy-to-spot common thread across his production is the desire to answer challenging conceptual questions by using formal tools.



Pierfrancesco Guarino
Maastricht University

## Features

### Interview with Elias Tsakas

Pierfrancesco Guarino: First question, how did you get into math?

Elias Tsakas: I was privileged to grow up in an environment where there were a lot of math books. My father was a math teacher and, even if we never formally studied mathematics together in the form of standard tutoring, we would sit on the couch in the evenings, he would give me riddles and we would play around. The parts of mathematics that I found most interesting as a kid were combinatorics and elementary probability. I still remember when he told me the famous story of how Gauss, at a very young age, came up with a method to sum all numbers from 1 to 100: that was the first time I realised that abstract reasoning can be more powerful and efficient than brute force. Later on, during high school, I was involved in math competitions and I always found this very pleasant. Finally, the last thing that drove me towards math was euclidean geometry, which is something that was extensively present in our curricula.

Pf. G: So now I see from where it comes your tendency to – quite literally – draw concepts.

E. T.: Indeed. My father always told me that you understand something if you can draw it and this is often a very difficult

exercise given the natural bidimensional and tridimensional restrictions we have to face.

Pf. G: Then you did your Bachelor in economics, your Master in math, and then you moved back to economics. What happened?

E. T.: Actually, I ended up doing economics almost by accident, since I liked math but – back then – I did not like physics and chemistry (even if now I enjoy reading physics) and the only school track that would allow me to study math without doing those other subjects was the one that led to economics and business schools. Thus, that's how I ended up doing economics. Back then I had no idea what kind of problems could be addressed in economics and I simply thought that by doing economics I was being trained to become an accountant. But once I started studying it, I realised that the span of topics that economics discusses is extremely broad.

Pf. G: Were you always interested in microeconomics?

E. T.: Somehow, I found much more appeal in microeconomics, but this was again accidental. Actually, one of the things I was good at coming from high school was differential calculus, and the way in which microeconomics is taught emphasises it, thus it really came natural to me. By the time I graduated I knew that I wanted to stay in academia, but I didn't really know whether the right path to do that was via micro theory. So I ended up applying to a Master program at the math department in the University of the Aegean, which is located in Samos, the birthplace of Pythagoras. This was a program on mathematical modelling, open to people with different backgrounds, such as engineering or social sciences (something which – ex post – made it more interesting than a standard math program. And this is when I started appreciating the math behind micro theory. As a result, even if this was not a famous school, this ended up being my ticket to the academic path, since this was the right environment for me at that stage and I managed to get all the tools that eventually facilitated my academic development.

Pf. G: After that you ended up doing a PhD in Goteborg and what happened is that first you worked on evolutionary game theory and then you gradually moved towards epistemic game theory. How did your interest in game theory start? What was behind this path?

E. T.: I really wanted to do my PhD in Scandinavia for two reasons: I wanted to go to a school with a US-style education (that is with structured coursework), something which Scandinavian schools offer, and I had realised during my previous stay in Sweden that I needed to live in a cultural and social environment that gives you the freedom and flexibility to work on your research without thinking about practical problems. So, this is how I ended up in Goteborg. During my second year I took a course taught by Mark Voorneveld on bounded rationality. That really interested me, so I approached Mark to talk about the topic, and I eventually asked him if he was willing to supervise my work and he agreed. Thus, I started to commute regularly to Stockholm (where Mark had, and still has, his position) and, by being in that environment, I found myself hooked up by evolutionary game theory, both because Mark was interested in it and also because Jürgen Weibull was around, which made it a natural topic of interest. Concerning my work on epistemic game theory, it all started as the result of reading Aumann's seminal paper "Agreeing to disagree". At first I was thinking about how agreements can arise in large groups, and eventually I got interested in other problems within epistemic game theory, such as epistemic characterizations of solution concepts and the mathematical models underpinning belief hierarchies. This happened towards the end of my PhD and was largely inspired by many talks I had with Amanda Friedenberg, both when we overlapped for a semester in Berkeley and later on during a visit to her home department in St. Louis. This was the time when I started to have a better understanding of what epistemic game theory does and what its general aims are.

Pf. G.: The next step were a Maastricht position and a Marie Curie fellowship. What were your thoughts back then?

E. T.: Funny enough, few years before me entering the job market, the American Economic Association allowed the candidates to signal their two preferred destinations and, in my case, one of the two was Maastricht, mainly driven by the very large number of theorists in the school (something which was and still is – actually, to an even larger extent – a very scarce commodity). Thus, when Maastricht made me an offer, I pretty much had to accept. Of course, Marie Curie was a milestone since it gave me the freedom to set up my own agenda at a very early stage of my personal development. The limited amount of teaching that came with it, allowed me enough research time, to also make tenure comfortably which I now appreciate greatly.

Pf. G.: Thus, during the last years, you started to diversify your results, with publications in experimental economics.

E. T.: Yes, indeed. Part of my aim at those early stages was to understand experimental methodologies and what is behind testing theories, in order to eventually be able to do better theory. This was eased by the fact that we have a very strong experimental group in Maastricht to whom I could relate. Along the way, interacting with experimentalists and writing myself experimental papers made me appreciate applied and experimental work much more than I originally did as a PhD student. But my relation towards experimental economics has not changed: I still mainly see it as a way to do better theory.

Pf. G.: It seems that now you are also broadening your interests by adding decision theory to your agenda. How does this relate to your previous work on epistemic game theory? What is your relation by now with epistemic game theory?

E. T.: First of all, let me explain what is my current of view on epistemic game theory, which is something that has shaped over the years. Epistemic game theory has fulfilled its original aim, which was partly to provide foundations for existing solution concepts that game-theorists traditionally introduced as black boxes that make predictions (Nash equilibrium being a typical example). This was a very important problem and – to some extent – we managed to provide collectively, as a scientific group, an answer. Now, having solved these issues, we find ourselves at a crossroad, asking what to do with all the machinery that we have developed at the early years of the discipline. Actually, concerning this, I see different ways in which epistemic game theory and related tools will/can be useful in the future. One way is in its relation with experimental economics, and in particular in contributing to the study of applied problems such as for instance "how to elicit higher order beliefs" or "how many steps of iterative reasoning people undertake",

and so on. This is something that we already see in the literature. Indeed, there are already more and more papers that take this direction. The second possible way is in connection with AI. Indeed, epistemic game theory studies how rational übersophisticated agents with unbounded computational resources would reason about the behaviour of others before making decisions themselves. Of course, from an applied point of view, these models are too ideal, since they assume way too much on the side of the agents' reasoning abilities. As a prime example, most epistemic game theory models assume that decision makers form infinite chains of beliefs, of the form "I believe that my opponent will do x", and "I believe that my opponent believes that I will do y", and so on. But, at the same time, it is not unreasonable to think that in few decades we will find ourselves in environments where important decisions will be taken by machines, and my conjecture is that all these models will turn out to be useful for predicting the behaviour of such decision-makers, who quite likely will closely approximate the über-sophisticated agents that one finds within our epistemic game theoretic models. Thus, in a sense, I conjecture that epistemic game theory will become very relevant for exactly the same reasons that are behind its present criticism. Concerning decision theory, I started to think about it because I wanted to have a new intellectual challenge. Indeed, on the top of what I said before about epistemic game theory, as it happens with many streams of the literature (e.g. the refinement literature), pretty much the same mathematical tools are used over and over again and I wanted to be exposed to new tools and new proof techniques. Furthermore, and this is related to what I said about experimental economics, decision theory is extremely appealing to me since it is much easier to produce testable hypotheses. Of course, this is the case since we have very nice axiomatic characterizations of the different decision-theoretic models, but also, even more importantly, decision theory does not have to deal with the complexity of strategic uncertainty. What I mean is that testing game theoretical statements with actual subjects often produces very noisy data, exactly because game-theoretic predictions often depend on higher order beliefs (e.g., on what I believe that the opponent believes that I believe that he will do). This complexity is not present in decision-theoretic environments, where it is often much simpler to describe the sources of uncertainty.

Pf. G.: Last question, maybe the toughest one. You mentioned the word "prediction": your predictions about Champions League final, World Cup, and NBA finals (ndr. Elias is an avid sport aficionado).

E. T.: World Cup: let Messi win one (laughs), partially wishful thinking. NBA finals is easier: Golden State. Champions League: gut feelings, Bayern Munich.

## Note on *The Significance of the New Logic*

As analytic philosophy is becoming increasingly aware of and interested in its own history, the study of that field is broadening to include, not just its earliest beginnings, but also the mid-twentieth century. One of the towering figures of this epoch is W.V. Quine (1908-2000), champion of naturalism in philosophy of science, pioneer of mathematical logic, language enthusiast and world traveller, trying to unite an austerely physicalist theory of the world with the truths of mathematics, psychology, and linguistics. Quine is best known among philosophers for his attack on the distinction between analytic truths and synthetic truths. He argued logic, language and mathematics are continuous with natural science, not separable from it. All truths depend on what words mean and on what the physical world is like; these are inextricably linked in scientific explanation.

Quine's posthumous papers, notes, and drafts revealing the development of his views in the forties have recently begun to be published. So have careful historical-philosophical studies of the evolution of his philosophical views, especially his key doctrine that mathematical and logical truth are continuous with the truths of natural science, not analytically true in virtue of meaning alone. But no Quine scholars thus far have included any discussion of Quine's fourth book on logic and its philosophy. This book constituted Quine's farewell to logic as he embarked on an assignment in the US Navy in 1942, uncertain he would return and deeply anxious about the state of the world. In a letter to his friend and mentor Carnap, Quine described this book as marking a crucial change in his thinking on meaning and analyticity Quine (1943: Letter to Carnap, in R. Creath (ed.) *Dear Carnap, Dear Van*, University of California Press, 299). Why have English-speaking philosophers have neglected such an interesting source until now? The answer is that they were unable to read it, because Quine wrote his book, Quine (1944: *O Sentido da Nova Lógica*, São Paulo: Martins), in Portuguese, during a visiting professorship at São Paulo.

Quine, who loved languages, spoke fluent German as a result of his association with Carnap and the Vienna Circle, and had previously picked up a little Portuguese on sabbatical in the Azores. He faced a steep learning curve trying to introduce Brazilians to 'the new logic', the modern mathematical logic used by philosophers of mathematics and science, positivists, logicists, and pragmatists to interpret contemporary developments in science and mathematics. With support from his Brazilian assistant Vicente Ferreira da Silva, Quine managed to use his book to explain the new logic's applications to logicism, transfinite mathematics, the incompleteness of arithmetic, proof theory, set-theoretic and semantic paradox, formal theories of truth, and new approaches to ontology. But after Quine returned from the war, his views had shifted and an English translation never materialised, apart from a few pages translated by Quine himself which he had published as a journal paper, Quine (1943: 'Notes on Existence and Necessity', *Journal of Philosophy*, 40, 113-127).

As a Anglophone post-doc specialising in philosophy of logic and Quine scholarship, I collaborated with Prof. Walter Carnielli, professor of logic at the University of Campinas, and Dr William Pickering, an American linguist bilingual in English and Portuguese on the first full English translation of *O Sentido da Nova Lógica*, supported by two postdoctoral grant (CAPES and FAPESP). The book is now published by Cambridge University Press.

Besides the translation of Quine's book, the volume also contains my accompanying historical-philosophical essay, Janssen-Lauret (2018: 'Willard Van Orman Quine's Philosophical Development in the 1930s and 1940s', in *The Significance of the New Logic* (ed. and tr. W. Carnielli, F. Janssen-Lauret, and W. Pickering), Cambridge University Press, xiv-xlvii). I explain the significance of the book for the history of Quine's views on analyticity. Quine's important work on impurely designative occurrences of terms, such as 'Giorgione' in 'Giorgione is so-called because of his size', dates from *The Significance of the New Logic*. I also argue that the book contains

equally crucial developments in Quine's philosophy of logic and views on ontology. Another key Quinean doctrine first articulated in this book is the virtual theory of sets, given a weighty role in Quine's later work on set theory, Quine (1963: *Set Theory and Its Logic*, Harvard University Press). Quine also made several of the well-known arguments of Quine (1948: 'On What There Is', *Review of Metaphysics*, 2(5), 21-38), or clear precursors of them, for the first time in *The Significance of the New Logic*, Janssen-Lauret (2018: xxx-xxxiii). From a historical point of view, the book reveals the influence of philosophers other than Carnap, including Whitehead and Russell, Tarski, and notably Frege, to whom Quine attributes the 'essential content' of the work on impure reference Quine (2018: 85).

**W.V. Quine *The Significance of the New Logic*, ed. and tr. Walter Carnielli, Frederique Janssen-Lauret, and William Pickering, with an accompanying essay by Frederique Janssen-Lauret, Cambridge University Press, 2018**

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# News

## Calls for Papers

Pluralistic Perspectives on Logic: special issue of *Synthese*, deadline 1 June.

Agency and Rationality: special issue of *MANUSCRITO*, deadline 30 June.

Formalization of Arguments: special issue of *Dialectica*, deadline 31 July.

Reliability: special issue of *Synthese*, deadline 11 November.

Instrumentalism about Epistemic Rationality: For and Against: special issue of *Synthese*, deadline 30 October.

# Dissemination Corner

## The Logic of Conceivability

The Logic of Conceivability (LoC) project aims to address a limitation of the standard treatment of intentional states based on possible worlds semantics: the so-called *problem of logical omniscience*. I here motivate how the formal tools so far developed in the LoC project - when combined with tools and techniques from epistemic logic - can help to tackle this problem. Let us first take a step back and briefly mention what epistemic logic is about, and what causes the problem of logical omniscience in such logics to arise.

Epistemic logic is an umbrella term for a species of modal logics whose main objects of study are *knowledge* and *belief* - intentional states of particular importance in reasoning. As a field of study, epistemic logic uses modal logic and mathematical tools to formalize, clarify, and solve the questions that drive (formal) epistemology, and its applications extend not only to philosophy, but also to theoretical computer science, artificial intelligence, and economics. Initiated by Hintikka's *Knowledge and Belief: An Introduction to the Logic of the Two Notions* (1962) - inspired by insights in von Wright's *An Essay in Modal Logic* (1951) - research in epistemic logic has widely advanced based on the formal ground of normal modal logics and standard possible world semantics based on (relational) Kripke structures as they provide a natural, mathematically elegant yet still relatively easy way of modelling epistemic logics. However, as already flagged by Hintikka, the standard Kripke semantics possesses features that make the notions of knowledge and belief it implements too strong, leading to the problem of logical omniscience: the agents represented know/believe all logical truths, and know/believe all logical consequences of what they know/believe. These agents are obviously highly idealized reasoners, far away from having realistic cognitive powers and bounds. While such epistemic systems can be defended to work well for derivative attitudes such as what one *ought to know* given what one knows, what one *potentially knows* given a certain body of information etc., they do not provide a satisfactory formalism for arbitrary *non-omniscient* agents and the knowledge attitude *per se*. Thus, we ask: what is the *logic* of the *knowledge attitude per se for arbitrary agents*? This is one of the questions that have been keeping Peter, Franz, and myself busy in the last couple of months.

The formal theory of aboutness and subject matter Peter surveyed last time in the issue of February, 2018 is of great help here. To be more specific: Franz and Peter have been developing in several LoC outputs - Franz's *Aboutness in Imagination* (2017: Philosophical Studies) and *Simple Hyperintensional Belief Revision* (2018: Erkenntnis), and Peter's *Theories of Aboutness* (2017: Australasian Journal of Philosophy) - a theory of propositional content that supplements the truth set of a sentence with its *subject matter* or *topic* as a component of its meaning. Intuitively then, knowing what a sentence *A* means boils down to knowing *what it is about*, i.e., *having grasped its topic*, and *what it says about that topic*. This more refined account of content - when taken on board together with the claim of Seth Yalcin's claim that epistemic states are topic sensitive (2016: *Belief as Question Sensitive*, Philosophy and Phenomenological Research) - helps us to break some patterns of strong logical closure that lead to the problem of logical omniscience. The resulting logic, for example, can account for some hyperintensional distinctions: one can know that "2+2=4" without knowing that "equilateral triangles are equiangular" although they are true at exactly the same possible worlds, namely all of them. Another intuitive example for the failure of strong closure goes as follows. "If 113 guests attended the ball, then the number of guests is prime" is a logical truth, yet one can know that "113 guests attended the ball" without knowing that "the number of guests is prime": *one can grasp claims about the number 113 without being able to grasp claims about primeness*.

The topic-sensitivity of epistemic states does not seem to explain the entire logical omniscience story, especially with respect to the failure of closure under *known* implications though.

Another explanatory factor we are drawn to - as, e.g., Lewis (1982: *Logic for Equivocators*, Noûs, Volume 16, No. 3, 431-441), Stalnaker (1984: *Inquiry*, MIT Press), Fagin & Halpern (1988: *Belief, Awareness, and Limited Reasoning*, Artificial Intelligence, Volume 34, No. 1, 39-76), Yalcin (2016) have for belief - is that an agent's knowledge state is fragmented across various 'frames of mind'. We store information in different fragments of our minds and, sometimes, it is hard to put two and two together and get to know what is entailed by what we know: Jones knows that Mary lives in New York, that Fred lives in Boston and that Boston is north of New York. Yet Jones fails to infer the obvious: that Mary will have to travel north to visit Fred (Braddon-Mitchell & Jackson, 2007: *The Philosophy of Mind and Cognition: An Introduction*, Blackwell Publishers, p. 199). Fragmentation of *belief states* (rather than knowledge states) can further account for the fact that an arbitrary agent can hold mutually inconsistent beliefs in non-interacting frames of minds: one might believe that monotremes lay eggs yet also believe that only non-mammals lay eggs.

Combining topic sensitivity and fragmentation of epistemic states results in a logic of the knowledge attitude per se for arbitrary, non-omniscient agents that complements, if not competes with, some of the previous proposals to solve the problem of logical omniscience in epistemic logic using, for example, *impossible worlds semantics* and *awareness structures*.

One note regarding the *dynamics* of knowledge and belief. The above mentioned logical framework naturally expands the array of dynamic attitudes Dynamic Epistemic Logic (DEL for short) studies. Roughly speaking, DEL extends (the so-called static) epistemic logics by inclusion of operators that describe informative events that lead to changes in knowledge and belief of the agents in question. Arguably, in a framework that makes subject matter of sentences part of their meanings and epistemic states topic sensitive, the dynamic knowledge update and belief revision operators should capture not only the changes in the intension but also the topic of the agents' epistemic state. This is what we are after now: *topic-sensitive dynamic epistemic logics for arbitrary agents*.

Further details and results are to follow as LoC outputs, please stay tuned!

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# What's Hot in ...

## Medieval Reasoning

At the beginning of May I was in St Andrews for an excellent workshop on Medieval Logic and its Contemporary Relevance (URL: https://www.st-andrews.ac.uk/arche/event/medieval-logic-and-its-contemporary-relevance/2018-05-01/). Scholars from different backgrounds – including historians of medieval logic, historians of medieval philosophy, logicians and philosophers of logic – came together to discuss several central aspects of the medieval logical tradition and of its study, bringing to the table a variety of points of view, approaches, answers and (above all) questions. Not only were all these talks informative about their own specific subjects within the scope of medieval theories, but they were also enlightening about contemporary logic and philosophy of logic. The QAs were particularly useful and exciting because they turned out to be one of those rare occasions of examining a number of familiar medieval theories under different lights and approaches, asking different types of questions and arriving to different answers equally supported and compatible with each other. Overall, this encounter of different backgrounds, interests and methods in an open dialogue was truly representative of what the study of medieval reasoning – intrinsically intersectional between historical, philosophical and logico-mathematical concerns – could and should be at its best.

Even if self-reflection and self-justification is a common (and almost peculiar) philosophical practice, in particular those scholars who work in grey areas of this sort, in between different disciplines, often find themselves addressing some general background questions that, while they might look symptomatic of an anxiety for legitimation, concern the nature of their work, their intellectual aims, and how those aims should be pursued. Why do we care about Medieval Logic? Why should others care? Who are those others? It was surprisingly reassuring to find out that everyone in the room routinely asked themselves these questions too. Sara Uckelman's talk on "The Ways in Which We Can Learn from Medieval Logic" (URL: medievallogic.wordpress.com/2018/05/03/why-do-we-study-medieval-logic/) addressed these matters and was excellent food for thought. Sara presented a twofold distinction mapping the ways of looking at medieval logic and reasoning: on the one hand, medieval logic is a subject holding intrinsic interest, that we study to learn about it, while aiming for a historically sound reconstruction; on the other hand, medieval logic is something we can learn from to address our contemporary concerns and clarify our own practices by transposing these theories within our own framework. Either approach is perfectly legitimate and has its practitioners: historians of philosophy will often be on the one side, logicians and philosophers of logic usually on the other. Yet, more often than not, the line is not so neatly drawn, nor should it be: not only is there a possible third way going down the middle, but it would also be a way that leads to both goals. For example, some awareness of the analogous contemporary debates would be helpful to a historian working on medieval logical theories, in order to better grasp their peculiarity and their continuity with our own contemporary concerns. Moreover, a historically minded logician could possibly find a goldmine of ideas and techniques in those dusty manuscripts by treating carefully those aspects of medieval logic that may not seem immediately transposable into our contemporary frameworks. Perhaps, not only is it possible to meet on this middle ground, but this could be the best space for a genuine and highly fruitful conversation between disciplines, of which this workshop was the perfect example.

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## Mathematical Philosophy

According to a recent Nature news feature by Baker (2016: 1,500 scientists lift the lid on reproducibility, Nature 533, 452–454), a survey of over 1,500 scientists revealed that 90% believed there to be a "reproducibility crisis" or "replication crisis" in scientific methodology: scientists have been unable to replicate much of the published experimental findings in some fields, especially social psychology and cancer biology—though others are affected—even well-known findings, leading to community-wide discussion on scientific research and publishing. In the case of social psychology, at least, Paul Meehl (1967: Theory-Testing in Psychology and Physics: A Methodological Paradox, Philosophy of Science 34, 103–115) had been warning about methodological problems in accepted research methods since the 1960s, but these anomalies have only begun to reach a point of crisis since around 2011. According to Nelson, Simmons, and Simonsohn (2018: Psychology's Renaissance, Annual Review of Psychology 69, 511–534), this came about through the confluence of several events, including the following:

1. Daryl Bem's (2011: Feeling the future: Experimental evidence for anomalous retroactive influences on cognition and affect, Journal of Personality and Social Psychology 100, 407–425) publication of conventional experimental evidence for the outlandish claim that present cognitive function can be influenced by future events. How could such work have been possible?

2. Well-known psychologists Diederik Stapel, Lawrence Sanna, and Dirk Smeesters were investigated for research misconduct—the latter two arising from work by Simonsohn (2013: Just Post It: The Lesson From Two Cases of Fabricated Data Detected by Statistics Alone, Psychological Science 24, 1875–1888) himself—leading to their resignations at their respective universities and the retraction of a range of research articles due to data fabrication and other dubious methodological practices. How could researchers not have discovered these earlier?

3. Simmons, Nelson, and Simonsohn (2011: False-Positive Psychology: Undisclosed Flexibility in Data Collection and Analysis Allows Presenting Anything as Significant, Psychological Science 22, 1359–1366) and John, Loewenstein, and Prelec (2012: Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling, Psychological Science 23, 524–532), respectively, revealed how easy and accepted it was to engage in "p-hacking" or questionable research practices (QRPs) that all but guarantee an experiment turns up a statistically significant finding. How could the community turn a blind eye to these obviously unreliable practices?

4. The Open Science Collaboration's "Estimating the reproducibility of psychological science" (2015: Science 349, aac4716) was a massive effort to replicate one hundred psychology studies. Depending on one's understanding of "replication," only between 36–47% of the studies replicated the originals. What does this say about the strength of evidence for effects published in typical studies?

As Kuhn observed, times of crisis in a science are opportunities for philosophical reflection, and this case is rich with mingled conceptual, mathematical, and modeling questions ripe for mathematical philosophers to pluck, ones whose answers could have a profound effect on policy and the practice of science. Indeed, talks at the 2017 meetings of the BSPS (by Osimani and Landes, Bird, and myself) and EPSA (by Sprenger and Romano, Radder, and myself) saw some philosophers bringing attention to these subjects, and the 2018 UK Experimental Philosophy conference has selected as its theme "Reproducibility and Replicability in Psychology and Experimental Philosophy." At the Minnesota Center for Philosophy of Science's official discussion group, the spring terms of 2017 and 2018 have been devoted to replication in general and in psychology, respectively, with active participation by psychologists and statisticians in addition to philosophers. (This column arises, in part, from my reflections on the developments of this last reading group.)

Among the many interesting questions, I will focus on just three. First: What is reproducibility/replication? Broadly, one can distinguish the use of these terms as they apply to experimental and statistical methodology, on the one hand, and as the nominalization of a success term, on the other. In the former case, an experiment or observational study's ability to be replicated in one sense requires a clear articulation of which features of the study are relevant for its evidential value. In another sense, it is closely related to concepts of triangulation and robustness, but more work needs to be done—could these features, say, be described in terms of causal models? As the nominalization of a success term, it refers to, e.g., one study *replicating* another. Various options for this have been proposed based on one's statistical framework, but their formal properties and justifications have not been studied systematically. For example, most are not symmetric: study A may replicate study B, but not vice versa. So, if replication is considered as evidential, the total evidence might depend on which study was conducted first! Using a prediction interval criterion, as proposed by Patil, Peng, and Leek (2016: What Should Researchers Expect When They Replicate Studies? A Statistical View of Replicability in Psychological Science, Perspectives on Psychological Science 11, 539–544), solves this, but only when one adopts symmetric intervals. Can replication depend on such a convention and still have evidential significance?

This leads directly to the second question: Why does reproducibility/replication matter? Many of the influential articles by scientists on this subject invoke simplistic platitudes, such as that "reproducibility is a defining feature of science" or "a core principle of scientific progress," (OSC, 2015: aac4716-1) that whither under the slightest criticism. But offering a more substantive defense depends, of course, on what one takes reproducibility/replication to *be*. Endorsing it for methodology does not entail doing so for it as a success term, or vice versa. The former of course ought to be connected with the debates around triangulation and robustness, while the latter faces prima facie incompatibility with mainstream (e.g., Bayesian) accounts of evidence and confirmation from philosophy of science. The reason for this, simply put, is that judging an experiment to have "replicated" or not may be too coarse of a way to assess the total evidence available for a hypothesis.

Third: What should be done about it? The scientific literature abounds in proposals, such as making research more transparent through standardization and pre-registration of one's statistical methods and lowering the significance threshold for null-hypothesis significance testing from 0.05 to 0.005, the latter by Benjamin et al. (2018: Redefine statistical signifi-

cance, Nature Human Behaviour 2, 6–10). But there is also recognition, e.g., by Zwaan et al. (2017: Making Replication Mainstream, Behavioral and Brain Sciences, 1-50), that the problems may be largely institutional in character: the economy of research incentivized in the sciences generally discourages researchers (especially early-career ones, as pointed out by Everett and Earp (2015: A tragedy of the (academic) commons: interpreting the replication crisis in psychology as a social dilemma for early-career researchers, Frontiers in Psychology 6, 1152)) from spending time on replication studies and levies few consequences for some QRPs and p-hacking. How might these incentives be modified to create better functioning science? Here, agent-based models could offer insight into how these proposals might affect research and explain why the crisis is much more acute in some sciences than others.

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## Uncertain Reasoning

2018 is the 30th anniversary of the publication of two books that should be familiar to uncertain reasoners. I'm going to use my column this month to celebrate them. These are both books that I happen to have consulted in my research in the last couple of months: but that I have have had cause to use them recently is some indication that they remain important touchstones for their respective topics.

The first book published in 1988 that I would like to celebrate is Peter Gärdenfors' *Knowledge in Flux*. This book synthesised (and extended) the rapidly growing theory of belief revision. Given a knowledge base (a consistent logically closed set of sentences of a propositional language) how ought you to change your beliefs on learning some new information? Several different belief change operators are studied and connections drawn between them. Given the subject matter and the density of theorems in the book, it is surprisingly readable. I am sure that the state of the art has moved on in the past thirty years, but as an introduction to the basics of belief change, this is still a great book.

The second of the two books I'd like to celebrate this month is David M. Kreps' *Notes on the theory of choice*. The theory that Kreps develops in this little book is not new to him: axiomatic choice theory had been around for about fifty years by the time Kreps published this book. But Kreps does an amazing job of summarizing and explaining the theories of von Neumann and Morgenstern, Anscombe and Aumann and Savage in just under 200 pages. Again, what's remarkable about this book is how readable it is. It covers a lot of mathematical ground, but in a pleasingly conversational style. Now, there are better books for getting at the philosophical problems surrounding this topic (for example John Broome's *Weighing Goods* or Richard Bradley's *Decision Theory with a Human Face*) but for a grounding in the mathematics of axiomatic choice theory, Kreps is still my favourite.

I'm sure there are other books whose anniversaries deserve to be celebrated, and perhaps I will celebrate some more books in the future.

Seamus Bradley
Philosophy, University of Tilburg

## Evidence-Based Medicine

A new outbreak of Ebola virus in the Democratic Republic (DR) of Congo has spread for the first time to a large city of 1 million people, Mbandaka. This makes the outbreak more dangerous as there is an increased possibility of transmission now that the outbreak has 'gone urban' and more people are in closer proximity to one another. The 2014-16 outbreak in West Africa killed approximately 11,300 people, but there is hope that controlling the current outbreak will be easier and a similar death toll can be avoided. This is because of the increased readiness of local and global health organisations to respond with appropriate control measures, as well as the presence of new and effective vaccines. One very promising measure is a vaccine developed in response to the '14-16 outbreak that targets the most deadly form of the virus, the 'Zaire' strain. Like all medical interventions, vaccines must be tested for efficacy and licenced for use by relevant national and international regulatory agencies. Vaccines, as I have noted in a previous column (March 2018), are however a special case due to mutating strains of virus causing problems with testing for efficacy, and with the high level of risk that comes with not responding quickly enough to rapidly spreading epidemics. In the Ebola case, efficacy has been tested for on a fairly stable strain, but this 'Zaire' strain vaccine has not been licenced for use yet. This may not be of too much concern as the trial that tested the vaccine reported 100% efficacy in a sample of approximately 5000 people. With such a large sample and effect size, irrespective of the particular methodology used or any uncertainty surrounding potential biases, it seems like this vaccine just works. Hence why even without licencing approval, the WHO has sent it to the DR Congo for potential use if the outbreak continues to proceed as the risk of not reacting seems to be outweighed by how well the vaccine works. The outbreak is still a major problem, but it is encouraging that the development and testing process seems to have worked in this case in the way it is intend to: going from identification of problem to efficacious intervention in only a few years, through the collective efforts of national and international organisations.

While the presence of such an efficacious vaccine is encouraging, it has been acknowledged that it would be better if instead of reacting to outbreaks, more effort was spent trying to prevent outbreaks. Transmission of Ebola through animal stock makes outbreaks difficult to predict due to the need to track which animals are carrying the virus and where they are travelling. Having vaccines ready for any outbreak is one way to prevent, but the possibility of being able to predict where disease may appear is more appealing as a source of prevention. Such efforts have been discussed recently in the context of efforts to

combat Cholera (see the WHO initiative '*Ending Cholera: A Global Roadmap to 2030*' for more details). Cholera is still a major publich health concern across Sub-Saharan Africa, as well is in parts of the Carribrean, Middle-east and Asia, with most efforts to control it being reactive in nature. Furthermore, control efforts are expensive as the interventions are not pills or injections, but sustainable water supply, sanitation, and hygiene (WaSH) infrastructure. Targeting these interventions to the right places before an epidemic breaks out is thus of paramount importance in areas where resources are scarce to begin with. One way of doing this is to have ever more detailed disease mappings, which can be used to show where Cholera incidience is highest in more fine-grained detail than country or region wide statistics. One recent study attempts to do this through 'Big Data' techniques, using 279 data sets covering 2283 locations in 37 countries, to identify areas at the 'district' level (20km x 20km) where targeted WaSH interventions will have the most effect on the most people. However, disease mapping alone cannot give the full picture needed to successfully prevent outbreaks. The maps constructed from large data sets need to be combined with epidemiological, environmental, and genomic information, to specify even more precisely where to target interventions and where not to. For example, information about the social lives and connections of individuals in high risk areas can combined with molecular data about the rate at which the Cholera bacteria spreads (or even inhibited by viral bacteriophages) to inform whether certain areas are 'cold' spots in epidemic regions. This would target interventions with even greater precision to 'hot' rather than 'cold' spots, saving resources for those areas that will need them the most. It is obvious here that, as in most complex public health cases, a combination of diverse kinds of evidence can build a better picture of what and where interventions are needed.

Cholera has long history of public health efforts being directed at its eradication, and hopefully with these new efforts the WHO's targets can be achieved. Ebola on the other hand is a relatively new public health concern, and one much more limited in geographic scope. This does not mean that the same sort of techniques currently being applied to the Cholera case cannot be utilised to predict and prevent Ebola outbreaks in future. The actions of local and global organisations in this current outbreak offer some hope that the right amount of attention will be directed to what is still a severe and dangerous concern.

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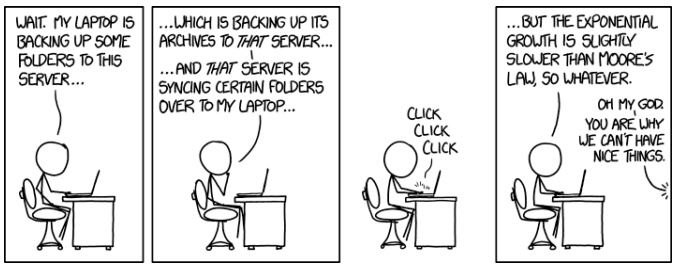



# Events

## June

EoM: Epistemology of Metaphysics Workshop, University of Helsinki, 1 June.
PNaS: Philosophy, Knowledge, and the Sciences, Polo Universitario di Pistoia, Italy, 4–5 June.
SSiB2: Science Studies in Budapest 2, Budapest, 5 June.
PoMMSE: Philosophy of Medicine Meets Social Epistemology, Hanover, Germany, 7–8 June.
A&R: Agency and Rationality, University of Campinas, Brazil, 14–16 June.
ExPow: Explanatory Power, University of Geneva, 14–15 June.
SaT: Space and Time: An Interdisciplinary Approach, Vilnius University, Lithuania, 15 June.
HaSE: Workshop on History and Scientific Explanation, KU Leuven, Belgium, 15–16 June.
ExPlura: Workshop on Explanatory Pluralism, University of Birmingham, 19 June.
RiPTW: Reasoning in a post-truth world: a look at dual-process models, Utrecht, the Netherlands, 20–21 June.
AtC: Approaches to Contradictions, University of Leeds, 22 June.
CMP: Computational Modeling in Philosophy, The Munich Center for Mathematical Philosophy, 22–23 June.
Logical Geometry and its Applications: Vichy, France, 25 June.
EGEC: Edinburgh Graduate Epistemology Conference, University of Edinburgh, [.25–26 June
K4U: Deliberation Workshop, Newcastle University, 27 June.

## July

AAoL: Australasian Association of Logic Meeting, Victoria University of Wellington, 6–7 July.
ESoN-EF: The Epistemic Significance of Non-Epistemic Factors, University Osnabrück, Germany, 12–14 July.

## August

TaE: Workshop on Time and Explanation, Milan, 20–21 August.

## September

PLP: The 5th Workshop on Probabilistic Logic Programming, Ferrara, Italy, 1 September.
WAW: Warsaw Argumentation Week, Warsaw, Poland, 6–16 September.

# Courses and Programmes

## Courses

LUCG: Logic, uncertainty and games, Como, 9–13 July.
SIPTA: 8th School on Imprecise Probabilities, Oviedo, 24–28 July.
SSA: Summer School on Argumentation: Computational and Linguistic Perspectives on Argumentation, Warsaw, Poland, 6–10 September.

## 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.
Doctoral Programme in Philosophy: Department of Philosophy, University of Milan, Italy.
LogiCS: Joint doctoral program on Logical Methods in Computer Science, TU Wien, TU Graz, and JKU Linz, Austria.
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 and 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.
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.
Research Master in Philosophy and Economics: Erasmus University Rotterdam, The Netherlands.

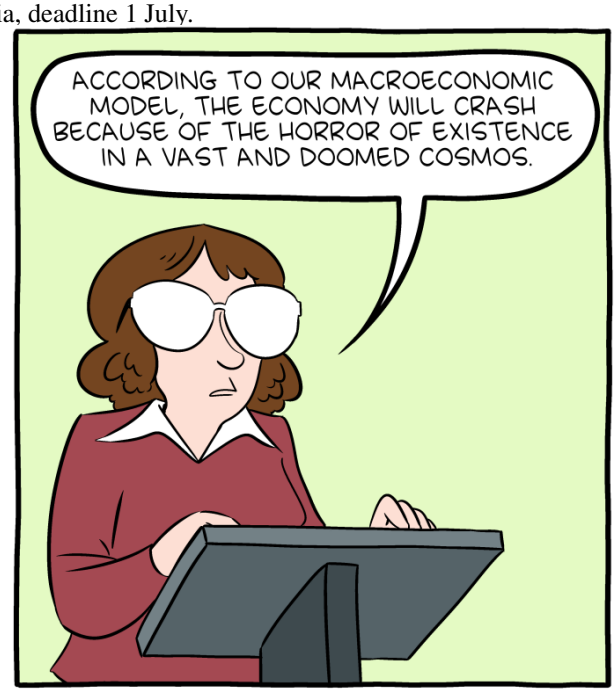
# Jobs and Studentships

## Jobs

Lecturer: in Statistics, University College Dublin, deadline 5 June.
Senior Lecturer: in Theoretical Philosophy, Philosophy of Science/Epistemology, Stockholm University, deadline 7 June.
Senior Lecturer: in Theoretical Philosophy, Logic, Stockholm University, deadline 7 June.
Fellow: in Philosophy of Science, University of Bern, deadline 16 July.

## Studentships

PhD position: in The Epistemology of Disagreement in Philosophy, University of Tartu, deadline 1 June.
PhD positions: (8) in Philosophy and Human Sciences, University of Milan, Italy, deadline 11 June.
PhD positions: in Logical Methods in Computer Science, Austria, deadline 1 July.




The fact that this never happens is proof that economic models don't ***really*** assume all agents have perfect information.