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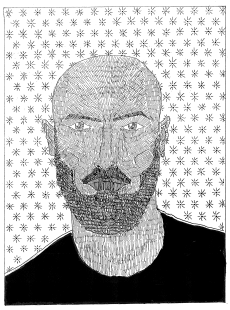
Guest Editorial



Thinking retrospectively about my Ph.D. I feel I am one of those lucky chaps who had three advisors: two were official and one unofficial. The person I interviewed for this issue is the unofficial one, namely Pierpaolo Battigalli. Full Professor of Microeconomics and Game Theory at Bocconi University, Fellow of the Econometric Society, charter member and Fellow of the Game Theory Society, it is difficult to understate the importance of Pierpaolo in the field of game theory. A pioneer of epistemic game theory, along his career his work, which touched many different topics, has always been driven by the desire for clarification of foundational issues.

Pierfrancesco Guarino
Alpen-Adria-Universität Klagenfurt

Features

Interview with Pierpaolo Battigalli

Pierfrancesco Guarino : Let's start from the beginning. Do you have some memories of your first encounters with mathematics?

Pierpaolo Battigalli : Yes. They were not particularly related to my family, but my father was an engineer so he helped me a little bit with math. However, there was essentially no need, because I was always good in math.

Pf.G : Did you attend the Liceo Scientifico or the Liceo Classico? *[ed. Until 2003, in Italy there were four different kinds of Licei, which were considered the most advanced type of Italian Secondary Schools: the Classico and the Scientifico were the most prominent ones.]*

PB : The Liceo Scientifico. I didn't want to spend all the time studying ancient Greek and Latin *[ed. As it happened in the Liceo Classico]* and – again – I was good in math.

Pf.G : We are talking about the 70s. You were born and raised in Milan, which from a political standpoint was in a turmoil. Did this environment had an influence on your choice to focus on social sciences?

PB : Yes, it had a very strong influence. I started high school in '75 and the year I finished it coincided with the end of the political turmoil. When I started, in my high school there were a lot of political activities with basically only one non-leftish political organization. I didn't have definite political ideas, but coming from a bourgeoise family, whose political orientation was – broadly speaking – classically liberal, there was a reaction. So, during my second year, Mario Gilli *[ed. future coauthor of Pierpaolo]*, who was in my class, convinced me to

help him to set up a local branch of a right off-center political organization that already existed in several schools in Milan. I accepted and it turned out that in our last high-school year, namely in '79-'80, we were both elected in the school council.

PF.G : So, you liked math and social sciences. When did you see for the first time the connection between these two fields?

PB : Because of this political activism, we were trying to organize events. Thus, we organized a series of lecturers held by Professor Dubini, a professor of Economics of the Catholic University. She presented mostly some macroeconomic models, but I learned that it was possible to use math to study economics in an analytical way. But then the approach, not just to economics, but to social sciences in general, came from two parts: by attending the meetings of my organization and the meetings and events of the liberal party, where they were occasionally telling us about liberal thinkers. Thus, I learned about von Hayek and Popper and I read "The Open Society and Its Enemies". Also, I found out that Bocconi was offering a new degree "Economics and Social Sciences" and I decided to enroll in it. It was not just standard political economics: although it was mostly economics, there was a strong interdisciplinary flavour. To give you an idea: during the first year, of course you had to study Calculus, Microeconomics, Macroeconomics, and Law, but also Epistemology. Better, we were supposed to study it... because actually nobody knew how to teach it, so it was idiosyncratic self-study, which came naturally to me since I was a Popperian and – after "The Open Society and Its Enemies" – I had read "Conjectures and Refutations" and also many other books on epistemology, including works by Kuhn and Lakatos. Thus, overall this degree was very attractive to me: it was inspired by the London School of Economics and implemented under the auspices of the late Innocenzo Gasperini.

PF.G : In your undergraduate thesis you formalized the notion of conjectural equilibrium. When did the topic pop up in your mind?

PB : Around 1984. I had heard about game theory just from individual studies, from reading books and works on the methodology of economics. So I understood that basically when you want to have a model where everything you want to explain is a function of the actions of the economic agents, that is formally a game. I was also very interested in what were called disequilibrium models. Thus, I went to the office of Aldo Montesano, who was teaching a course called "Mathematical Economics", to ask a title for the thesis and I told him that I wanted to study game theory, because I wanted to study these models and I understood that I had to study game theory to study them. And this is how my thesis started. I studied a lot: general equilibrium theory; the book "General competitive analysis" of Arrow and Hahn; I studied almost cover to cover "The Theory of Games and Economic Behavior" by Von Neumann and Morgenstern, plus a short book on game theory, the one by the late Russian mathematician Vorobev.

PF.G : How did you encounter the paper on conjectural equilibria by Frank Hahn?

PB : Piero Tedeschi, who was back then an assistant and is now professor at the Catholic University, when we were students still fascinated by the rational expectation approach, well before my thesis, told us "Don't be fascinated by the rational expectation approach; rather, read the work of Frank Hahn on conjectural equilibrium." So I read his papers and I was impressed the most by the non-mathematical one, which was his inaugural lecture "On the Notion of Equilibrium in Economics" for the Cambridge academic year of 1973. So my interest shifted from something in which eventually I would have used game theory to do macroeconomics and disequilibrium to a methodological thesis where I was saying "Look, you want do this well? Then you have to use game theory". So I was enthusiastic about game theory as a language, but I was very much aware of Hahn's criticism of the traditional equilibrium concept. So, when I had my first encounter with Nash Equilibrium, I had the same attitude and I asked myself: why Nash? So, basically I shifted from a methodological thesis relying on game theory to a methodological thesis about game theory, in which I was criticizing the main solution concept.

PF.G : Did you read some articles on GT?

PB : Yes. I had been told to read Kreps and Wilson's "Sequential Equilibria", Kohlberg and Mertens' "On the Strategic Stability of Equilibria", and Perry and Grossman's "Perfect Sequential Equilibrium". So, I didn't read much, but I read those. By reading Kohlberg and Mertens, I was fascinated by the forward induction example and I thought that was exactly how I had to think about reasoning in games. Also, I thought that I could solve this by an iterated deletion procedure. Thus, I understood that in order to provide a satisfactory formal mathematical definition of Hahn's equilibrium concept I had to do what he didn't want to do, namely to use game theory, and that in order to formalize this in a satisfactory way I had to do something where everything I wanted to explain had to be a function of actions. So I used the notion of extensive form game and I gave a formal definition of what I called conjectural equilibrium for finite games in mixed strategies. Then I showed that some conjectural equilibria that are also Nash – because I was of course generalizing Nash – don't make any sense. So, I wanted to have conjectural equilibria with conjectures that made sense. And the way in which I defined conjectures that made sense was by means of an iterated deletion of conjectures: in the case of simultaneous move games this procedure corresponded to Rationalizability, even if back then I was unaware of the existence of both papers from 1984 by Bernheim and Pearce in which this solution concept had been introduced.

PF.G : Talking about Rationalizability and forward induction, in your paper "On Rationalizability in Extensive Games" on the *Journal of Economic Theory* (1997), you provided an alternative definition – by now considered the standard one – of Pearce's Extensive Form Rationalizability and you derived a result known as Battigalli's theorem, which proves the outcome equivalence of backward and forward induction in generic games with perfect information. How did you arrive to that fundamental game-theoretical result?

PB : This result has a very convoluted story: in few words, back then I was not the only one who had this result in mind. I wrote the original paper in the spring of 1990 during a three months visiting period in Tel Aviv. Later on I realized the paper had a problem: Faruk Gul and Phil Reny provided a counterexample to a lemma that I used in the proof. Thus, I revised the paper and obtained the solution during my first year in Princeton in 1994. However, I was extremely unsatisfied with my so-

lution. Indeed, I had to borrow the line of proof from Reny's paper "Backward Induction, Normal Form Perfection and Explicable Equilibria", which employed tools from algebraic topology: this made me really unhappy, since I felt this was a deeply algorithmic result, which did not have to be proved that way. So I was very pleased years later when I read "The Order Independence of Iterated Dominance in Extensive Games" by Jing Chen and Silvio Micali, which had an entirely algorithmic argument for a result that implies mine as a corollary; much in the same way in which I was happy to see the proofs of my result obtained by Aviad Heifetz and Andrés Perea in their paper "On the Outcome Equivalence of Backward Induction and Extensive Form Rationalizability" and by Andrés Perea alone in his paper "Why Forward Induction Leads to the Backward Induction Outcome: A New Proof for Battigalli's Theorem".

PF.G : How do you see the epistemic characterization of Extensive Form Rationalizability that you provided by means of the Strong Belief Operator in "Strong Belief and Forward Induction Reasoning", published on *Journal of Economic Theory* in 2002, written along with Marciano Siniscalchi?

PB : That work is intertwined with the paper on infinite hierarchies of conditional probability systems and the corresponding type structures *[ed. "Hierarchies of Conditional Beliefs and Interactive Epistemology in Games",* Journal of Economic Theory *(1999)]*. Thus, first I built that theory and then I introduced the strong belief operator as an epistemic operator. But, before building the formal apparatus, obviously I had the intuition, since I had worked on these different notions of rationalizability that capture forward induction reasoning in what Faruk Gul once called – while we were both in Princeton – my *pre-epistemic* work. Actually, the intuition is already in my undergraduate dissertation.

PF.G : How did you actually decide to study mathematical logic and modal logic?

PB . It is really one specific episode: I attended two presentations of the same paper by Salvatore Modica and Aldo Rustichini of what became their first paper on unawareness [ed. "Awareness and Partitional Information Structures", *Theory and Decision* (1994)]. Modica's presentation was the first one I attended and in that occasion he said how he stumbled upon the idea behind the paper by reading a work by John Geanakoplos and that, when he mentioned the topic to Rustichini, Aldo said "We have to study modal logic, so let's read Chellas' book". So I bought and read that book. Also, I had already a correspondence with Giacomo Bonanno.

PF.G : There are a lot of people that have no access to you, but that would like to learn the methodology that you apply. The question is: what is the way in which you like to do and teach game theory?

PB : There are some maxims I wrote for a festschrift in honour of Sergei Artemov in 2012. I still subscribe to statements such as "Explicit is better than implicit", "Knowledge and Belief are different", "Strategies cannot be chosen; only actions can be chosen". Given this, I try to let students understand that if you want to do economics or other social sciences in a formal way from the ground-up you have to use game theory, because this means using models where everything you want to explain is a function of exogenous variables and agents' actions. Although traditional game theory uses Nash Equilibrium and its refinements, I try to teach my students that the main reason why they have to study these refinements is because other people use them and they need to know them to understand the literature, but this does not mean they are intrinsically important. What in my view is intrinsically important is what we can give an interesting foundation to, where a foundation is not necessarily in terms of – say – *Common Belief in Rationality*, but is something where you state explicitly and formally assumptions about rationality and how players thinks about each others, and you derive whatever you derive from those assumptions in terms of behavior or other things such as first-order beliefs. So this is the way in which I teach game theory and this is how I want my students to learn game theory. However, in order for them to learn game theory in this way, I also teach them something else, namely a foreign language, the language of mathematics. So I teach them that mathematics is first and foremost a body of knowledge that allow us to understand a lot of things, but it is also a foreign language that has to become their first language. So, overall, this is how I teach, something that can be found in my textbook, which I am slowly writing *[ed. part I is stabilized and can be downloaded from Pierpaolo's webpage*. Thus, this is like my gospel and I am a preacher *[laughs]*, a lucky one since I have access to some brilliant students.

PF.G : If you have to pick three articles written by you that represent your work, which ones would you pick?

PB : Without hesitation "Strong Belief and Forward Induction Reasoning" on the *Journal of Economic Theory* (2002). Concerning the others, I would go for "Self-Confirming Equilibrium and Model Uncertainty" on *The American Economic Review* (2015), and "Dynamic Psychological Games" on the *Journal of Economic Theory* (2009).

PF.G : Thus, in the end, economist or philosopher?

PB : Given my knowledge, I am an economist. But in terms of interests and innate abilities, I should have been a philosopher.

PF.G : Final question: will we win the Champions League this year? [Pierpaolo and I are both Juventus' supporters]

PB : No.

News

The 3rd triennial international conference of the German Society for Philosophy of Science 25-27 February 2019

The 3rd triennial international conference of the German Society for Philosophy of Science (GWP) took place at the Universitaet zu Koeln from the 25th to the 27th of February. It was organized and hosted by the Philosophisches Seminar of the Universitaet zu Koeln. The conference covered all fields of Philosophy of Science and was attended by philosophers of many European and Non-European countries. There were six parallel sessions and six invited talks, two of which were sponsored by Springer and De Gruyter. 101 talks and six plenary lectures were given in total, with 140 papers and nine symposia proposals having been submitted. The talks provided an insight into the work of experienced philosophers and those in the early stages of their careers alike. The ensuing discussions were universally polite and gave an opportunity for well-meaning critique and inspiring comments. The six plenary lectures were given by Kaerin Nickelsen (LMU Muenchen), C. Kenneth Waters (University of Calgary), Erik J. Olsson (Lund University), Katherine Hawley (University of St Andrews), Martin Carrier (Universitaet Bielefeld) and Michael Strevens (New York Uni-

versity). In the first talk "Interactions and Interdependencies: Philosophy of Science and History of Science as Friends with Benefits (or more)" Kaerin Nickelsen argued for more cooperation between philosophers and historians of science, using the discovery of photosynthesis as an example of the historical contingency of scientific progress. While philosophers emphasize analyticity and tend to take a normative stance towards methodology, historians point out the historical particularity of each event and describe it in greatest possible detail. C. Kenneth Waters position in "Scientific Metaphysics of Hierarchy" was that there are hierarchies in nature, but no overarching structure. In his words: "Its a mess out there." He discussed possible distributions of structure in nature on a scale from micro to macro and illustrated his point in detailed analyses of biological hierarchies from molecular genetics to trophic levels in ecology. "Explicationist Epistemology and Epistemic Pluralism" by Erik J. Olsson presented an explicationist account of knowledge inspired by Carnap. Olsson discussed the ability of explicationism to tackle the Gettier problem and compared this take to Alstons theory of epistemic desiderata. In "Who Speaks for Science?" Katherine Hawley dealt with the way in which scientific findings are presented in the media, focussing primarily on the formulation of headlines. Usually it is unclear if they contain either existential or universal quantification, thus inviting misunderstandings and wrong conceptions about groups. Martin Carriers talk posed the question: "How does Good Science-Based Advice to Politics Look Like?" Rather than keeping non-epistemic values out of the process, they should be presented as alternatives but kept clearly separate from the facts, Carrier argued. Finally, Michael Strevens analysed the different meanings of "Necessity in Scientific Explanation". He made a case for the distinction of necessity pertaining to causality and necessity in a metaphysical sense. GWP.2019 was supported by Springer, Deutsche Forschungsgemeinschaft (DFG), De Gruyter, Universitaet zu Koeln, German Society for Philosophy of Science (GWP), and Duesseldorf Center of Philosophy of Science (DCLPS). Their contributions are very welcome. The Local Organizing Committee at the Universitaet zu Koeln was chaired by Andreas Huettemann. The other members were Ursula Heister, Michael Hicks, Elisabeth Muchka, Jan Koester, Liane Lofink and Martin Voggenauer. It was assisted by the GWP Committee under the President Gerhard Schurz (Heinrich Heine University Duesseldorf), Uljana Feest (University of Hannover), Alexander Gebharter (University of Groningen), Thomas Reydon (University of Hannover) and Christian Feldbacher-Escamilla (Heinrich Heine University Duesseldorf). The conferences of the GWP have been growing steadily over the last nine years and the GWP.2022 conference is already being planned.

Michael Koerner

Calls for Papers

HaPoC 2019: 5th International Conference on the History and Philosophy of Computing: , deadline 30 April.

Folk Psychology: Pluralistic Approaches: special issue of *Synthese*, deadline 15 May.

Imprecise Probabilities, Logic and Rationality: special issue of *International Journal of Approximate Reasoning*, deadline 1 June.

Nancy Cartwright's Philosophy of Science: special issue of *Theoria*, deadline 1 November.

What's Hot in ...

Medieval Reasoning

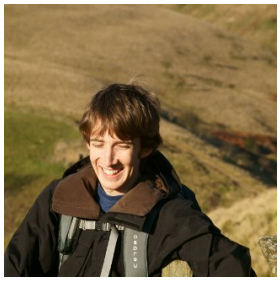
At the end of May, UCLA will host the first *Pan-American Symposium on the History of Logic*[=PASHL]. For four days (24-28 May), experts on different logical and philosophical traditions – from Antiquity to the early 20th century – will meet to discuss about the notion(s) of *Validity throughout History*. It is not the first time that I write about this upcoming conference, but this is a pet project of mine and I hope that our readers will forgive me for my self-indulgence. Besides, truth is, I think it is going to be an exciting event, not only for the parties directly involved, but for historians of logic and rationality in general, because we are trying to propose something new in the way we do the history of logic and, hopefully, influence the possible routes that future research should explore. I should thank profusely the UCLA Department of Philosophy and the Centre for Medieval and Renaissance Studies for sponsoring, financing and hosting this meeting. I also cannot thank enough my co-organisers (Calvin Normore and Milo Crimi) for their time and the hard work that they have devoted to this. But I would also like to use the space at my disposal to say something more about where the PASHL comes from, what it is meant to be, and where we would like for it to go. The idea stemmed from three different consideration. (1) As of now, most of the academic events in the history of logic are hosted in European universities – which is understandable, at the very least because on that side of the Pond there is a somewhat stronger emphasis on the history of philosophy and sciences within the standard curricula. However, this means that most grad-students and early career researchers from outside the Old Continent – underfunded as they are – have a hard time attending any of those meetings. The intention of this conference is to offer them a closer alternative. (2) In recent years, we have noticed a tendency towards a "metaphysical turn" in larger scale conferences in the history of logic, relegating the technicalities of the "old logical stuff" to small workshops. While this is not a problem per se, as much as a mirror of the current research trends and academic interests, it puts a misleading emphasis on the non-logical stuff that is indeed part of traditional logic, however it is clearly not so dominant a part as some non-specialist might be let believe by looking at those conferences' programmes or skimming through the proceedings. We have tried to put the emphasis back on the logic in the history of logic. What better starting point than the notion of validity? (3) Historians of logic tend to be locked in their own subfield bubbles and their interactions are sporadic and limited to closely related traditions, usually with a heavily Western focus. While this a widespread problem throughout the history of philosophy, it is particularly puzzling and urgent in the history of logic since very often contemporary logicians have advanced claims of eternality and universality. We have tried to create a larger space for dialogue and comparison, across time and space. So we will have talks on medieval Latin logic alternating with papers in Ancient, Byzantine, Arabic, Sanskrit, and Early Modern logic – not by focusing on questions of transmission and

reception, but on a fundamental conceptual issue. This spirit of openness and inclusivity has motivated our scientific choices and our invitations, with the intent of having a generational dialogue as well. Our hope is to create a biannual appointment in the Americas and to help make our discipline more comparative, more dialogical and more aware of what is going on in its many subfields. We have a magnificent line-up (that you can find here: and no registration fee; so if you are in the area, feel free to come by.

Graziana Ciola, Durham University

Graziana Ciola
Durham University

Uncertain Reasoning



You're rushing towards a hospital ward carrying two gas canisters. Inside one canister is a gas that will cure Dragon Pox, and that has no discernable side effects. Inside the other canister is a gas that will cure Greyscale but causes bad eczema. In the ward you're rushing towards are two patients, one with Dragon Pox, the other with Greyscale: two diseases that will be fatal without treatment. You only have time to pump one of the gases into the ward; what should you do? If you pump in the Dragon Pox cure, then one patient – the patient with Greyscale – will die and the other – the patient with Dragon Pox – will be cured. Let's assign these outcomes utilities of 0 and 1 respectively. On the other hand, if you pump the Greyscale gas, then the Greyscale patient will survive but have bad eczema for a few days (utility 0.9) and the Dragon Pox patient will die (utility 0). You don't know which patient is which, so you assign probabilities as follows:

- the probability that Tim has Dragon Pox is 0.5,
- the probability that Tim has Greyscale is 0.5,
- the probability that Jason has Dragon Pox is 0.5 and
- the probability that Jason has Greyscale is 0.5.

Let's look at the prospects for Tim and for Jason. Tim's prospects if you pump in the Dragon Pox gas are $0.5 * 0 + 0.5 * 1 = 0.5$, and likewise for Jason. And for the Greyscale gas case we have $0.5*0.9+0.5*0 = 0.45$ for both Tim and Jason. On this way of looking at things, it appears that the prospects for both patients are better if you pump the Dragon Pox-curing gas and that seems like a reason to do that rather than the other option. However, we might instead consider things a different way: we know we have one of each kind of patient, and if we consider the prospects for Dragon Pox patient and Greyscale patient then things are less clear. If we pump the Dragon Pox gas, then Dragon Pox patient survives (utility 1) but Greyscale patient dies (utility 0); if we pump the Greyscale gas, then Dragon Pox patient dies (utility 0) and Greyscale patient survives but has bad eczema (utility 0.9). No option dominates the other on this way of designating the people involved. So what should you do? This puzzle comes from Anna Mahtani's recent paper "The Ex Ante Pareto Principle" (*Journal of Philosophy* 2017). How you designate the people who might suffer or gain by your actions affects how good or bad an act looks. Uncertainty about which person seems to be causing a serious puzzle.

It is widely acknowledged that what an agent believes about a person, say, can depend on how that person is designated. So Lois Lane believes Superman can fly, but she does not believe that Clark Kent can fly, even though Superman and Clark Kent are the same person. Mahtani's example shows that the same sort of issue arises in decision making contexts: the same issue can infect our assessment of the goodness of an act when there's uncertainty about how different ways of describing the people match up. The importance of how possible beneficiaries or victims of our actions are designated also shows up in the large literature on "identified lives" versus "statistical lives" (Cohen, Daniels and Eyal "Identified Versus Statistical Lives" OUP 2015).

Experimental economics and behavioural psychology have also shown that when assessing what one ought to do, we tend to be sensitive to how the outcomes of the acts are framed (the so-called framing effect). For example, if there are 600 people at risk, it seems better to save 200 than to let 400 die, even though these are, in effect, the same outcome (Tversky and Kahneman "The Framing of decisions and the psychology of choice". *Science* 1981).

The standard "set of possible worlds" framework for belief and decision struggles to accommodate the above kinds of "intensionality" of belief and value. What are we to think? Can we just dismiss framing effects and identification bias as cognitive mistakes? If that's the case, what is the correct way to designate the patients in Mahtani's example such that we get "the correct" answer? Or do the above examples show that we need a more nuanced theory of rationality that permits agents' beliefs and desires to be intensional? It may be obvious from previous columns that I am firmly in the latter camp. But what such a thoroughly intensional foundation for degrees of belief would look like, I do not know.

Seamus Bradley
Philosophy, University of Leeds

Mathematical Philosophy

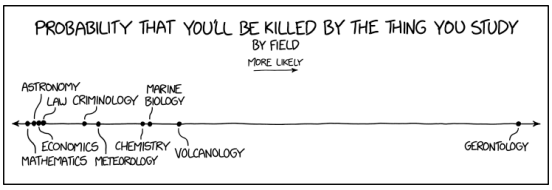
In my last column, I launched Formal Epistemology of Medicine as a new research program that analyses epistemic issues arising in medical epistemology by examining the interaction of methodological, social and regulatory dimensions in medicine. This mainly consists of the foundational analysis of causal and statistical inference in medicine, with a view at the strategic dimensions of scientific interactions in fields of enquiry characterized by information asymmetry and vested interests. This column is therefore devoted to the so called "reproducibility crisis" (see Open Science Collaboration (2015) Estimating the reproducibility of psychological science. Science 28 Aug 2015: Vol. 349, Issue 6251), which invested not only the social sciences, but also biomedical research, and is drifting the entire scientific enterprise into a general crisis of trust (Edwards, M. A., & Roy, S. (2017). Academic research in the 21st century: Maintaining scientific integrity in a climate of perverse incentives and hypercompetition. Environmental Engineering Science, 34(1), 51-61). This state of affairs has led to the launch of various initiatives aiming to foster transparency and honesty in the sciences; see for instance "Sense

about Science" and the All Trials campaign (see also the Catalogue of Biases). "Metascience" studies have instead focused on monitoring misconduct by developing tools for the identification of patterns of bias in datasets and bodies of evidence: e.g. Munafò, M. et al. (2017). A manifesto for reproducible science. Nature Human Behaviour, 1(1), 0021; Etz A, Vandekerckhove J (2016). A Bayesian Perspective on the Reproducibility Project: Psychology. PLoS ONE 11(2): e0149794).

Other proposals more directly address the structure of scientific ecosystems and offer research policy recommendations. The "Open Science" movement appeals to data sharing, making codes and research protocols accessible, as well as imposing preregistration of studies as a prerequisite for publication in journals (see e.g. Nosek, B. et al. Contestabile, M. (2015). Promoting an open research culture. Science, 348 (6242), 1422-1425). These measures should, among other, restrict the "researchers degree of freedom" (Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. Psychological science, 22(11), 1359-1366), or the so called "Garden of Forking Paths" (e.g. p-hacking or HARKing). However while advocating for more data curated science (accuracy of measurements, curated replications), Koole and Lakens (2012) lament that scientists are not motivated enough to make replications (Koole, S. L., & Lakens, D. (2012). Rewarding replications: A sure and simple way to improve psychological science. Perspectives on Psychological Science, 7(6), 608-614). More generally, the metascience community is becoming increasingly aware that methodological standards cannot by themselves constitute a decisive solution to the problem of distorted data, and that incentives to scientific misconduct should be better investigated and be taken into account when designing research policies (see for instance Smaldino, P. E., & McElreath, R. (2016). The natural selection of bad science. Royal Society Open Science, 3(9), 160384).

On their side, philosophers of science (Alvin Goldman, Philip Kitcher, Helen Longino, Wendy Parker, Miriam Solomon, Michael Strevens, Kevin Zollman, to name but a few) have been long drawing our attention to the social dimensions of science and the communication dynamics of various structures of information flow in scientific ecosystems. Although these contributions cast light on important aspects related to the impact of evidence in the scientific community – and the forces distorting it –, they do not formally address the epistemic dynamics related to evidence signaling from knowledge producers to consumers of scientific information and decision makers (e.g. agencies, policy makers etc.). This is a phenomenon which has been modeled in game theory by Bayesian games, especially the more recent generation of "Bayesian persuasion games" with noisy data acquisition stages, in which the sender is uncertain about the state of the world but has the (usually costly) option to obtain a noisy (i.e. probabilistic) signal about it. The introduction of noisy data results in models where both the sender and the receiver hold probabilistic beliefs about the state of the world and each other's type. Thus, the sender's delivery of imperfect information (probabilistic evidence) to the receiver is "adaptive" in the sense that it is tailored to the goal of having the receiver act in his preferred way, in light of probabilistic beliefs about the receiver's type. However, a sophisticated receiver, i.e. one who is aware of the sender's incentives and preferences, may adjust her belief revision in light of probabilistic belief that she has about the sender's motivations, represented by the sender's types (Henry, E., (2009). Strategic disclosure of research results: the cost of proving your honesty. Economic Journal 119: 1036-1064; see Harsanyi, J, (1967-1968). Games with Incomplete Information Played by Bayesian Players. Parts I, II, and III, Management Science 14: 159-182, 320-334, 486-502 for the original characterization of Bayesian equilibrium. For Bayesian equilibrium refinements for sequential games, see, for example, Kreps, D., M., Wilson, R. (1982). Sequential equilibria. Econometrica 50(4): 863-894 and Fudenberg, D., J. Tirole (1991). Perfect Bayesian equilibrium and sequential equilibrium. Journal of Economic Theory 53: 236-260). Since this literature is broadly interested in identifying the conditions under which the sender is incentivized to provide truthful information to the receiver, it may come to the aid for rescuing science from its crisis of distrust. Within my ERC project, we are developing a model of statistical inference which takes into account such strategic dimensions with the aim to formally address various kinds of biases (see also Osimani, De Pretis, Radzvilas, Stambaugh. Science as a Signaling Game: Statistical Evidence in Strategic Environments, forthcoming; Radzvilas, Stambaugh. Optimal Lying and Lie-detection in Bayesian Persuasion Games with Costly Information Acquisition and Truth-Proportional Beliefs, forthcoming; Landes J., Osimani B. Varieties of Error and Varieties of Evidence in Scientific Inference, forthcoming; Boem, F., Bonzio S., Osimani B., Sacco A. The Cochrane case: an epistemic analysis ondecision-making and trust in science in the age of information, forthcoming). We welcome philosophers, scientists and policy makers interested in joining us and contributing to this new research program. We are organizing seminars and workshops on the topic and everyone is invited to get in touch with us for more detailed information. Please also consider submitting your abstract to the MuST&PSE joint conference, which is devoted to statistical reasoning and scientific error from diverse viewpoints.

Barbara Osimani
Munich Centre for Mathematical Philosophy



Events

May

EMaTC: Episodic memory and Temporal cognition, University of Antwerp, 2–3 May.
ExLog: Explaining Explanation Using New Developments in Logic, Belgium, 6–8 May.
SSR: Science Self-regulation: Between Marketization, Bureaucratization, and Professionalization, Belgium, 9 May.
BMiPoS: Mental Imagery and Bayesian Models in Philosophy and Cognitive Science, Belgium, 9 May.
BtB: Beyond the Brain. Reconceptualizing Mental Disorders, University of Edinburgh, 9–10 May.
BaR: Workshop on Belief and Reasoning, Union College, New York, 16 May.
TiPoB: Recent Trends in the Philosophy of Biology, Bilkent University, 17–18 May.
OiSR: Objectivity in Social Research, University of Bergen, 23–24 May.
PPoMK: Philosophical Perspectives on Medical Knowledge, University of Genoa, Italy, 28 May.
LogiDis: Workshop on Logical Disagreements, University of Bergen, 28–29 May.

June

Logic and Metaphysical Commitment, Israel: 13–14 June,
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Tractable Probabilistic Modeling, Long Beach, California: 14–15 June,
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Prob&Stats: Conference in Probability and Statistics, University of Exeter, 18–21 June.
SECC: Scientific Explanations, Competing and Conjunctive, University of Utah, Salt Lake City, 26–28 June.

July

PoSE: Perspectives on Scientific Error, LMU Munich, 1–4 July.

Courses and Programmes

Courses

SSA: Summer School on Argumentation: Computational and Linguistic Perspectives on Argumentation, Warsaw, Poland, 6–10 September.

Programmes

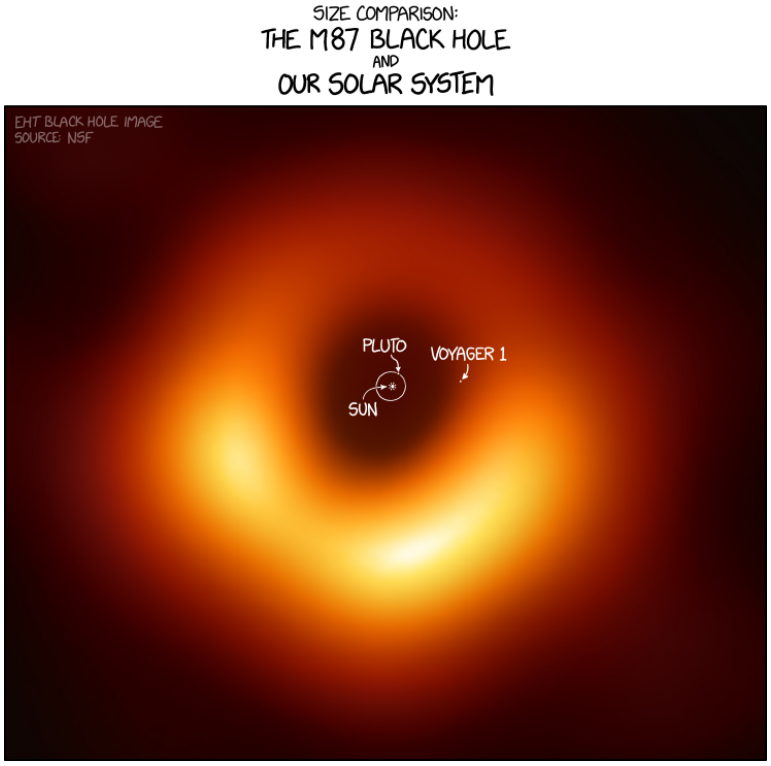
APhil: MA/PhD in Analytic Philosophy, University of Barcelona.
Master Programme: MA in Pure and Applied Logic, University of Barcelona.
Doctoral Programme in Philosophy: Language, Mind and Practice, Department of Philosophy, University of Zurich, Switzerland.
Doctoral Programme in Philosophy: Department 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

Post doc: in Philosophy of Science, University of Southern Denmark, open until filled.
Post doc: in History and Philosophy of Science, Stanford University, California, 15 May.

Studentships

PhD position: in Individual and Collective Reasoning, University of Luxembourg, open until filled.
PhD position: in Metaphysics of Quantum Objects, University of Geneva, open until filled.