The Reasoner

Volume 10, Number 6
June 2016

thereasoner.org
ISSN 1757-0522

Contents

Editorial

"What does probability mean?" This question echoes in the entire scientific work on foundations of probability, evidence theory and mathematical statistics produced by Glenn Shafer. 2016 is the 40th anniversary of the publication of "A Mathematical Theory of Evidence" and I thought that *The Reasoner*'s readers might be interested in getting a glimpse on the personal steps that led its author from Dempster-Shafer rule of combination, to his game-theoretic foundations of probability. Glenn Shafer obtained his PhD in mathematical statistics from the University of Princeton in 1973; after teaching Statistics at Princeton, he returned to Kansas in 1976 to teach Mathematics at the University of Kansas. In 1984, he moved from Mathematics to Business at Kansas. He joined the Rutgers Business School in 1992 and served as its dean from 2011 to 2014. The Dempster-Shafer theory has been extensively applied in engineering and artificial intelligence as well as in accounting, and "A Mathematical Theory of Evidence" is one of the most cited books in the history of statistics, also widely so outside the discipline.

Glenn Shafer's interest in the meaning of probability soon led him to investigate probability also from an historical and philosophical perspective: his papers on Martingales belong to this strand of research, as well as the ones on the history of the term "random variable" and on the work of Russian statisticians of the last century.

Shafer's theory of belief functions was developed to overcome the limitations faced by Bayesian confirmation theory in situations where probabilities are "constructed" while gathering the evidence—in this, he has been inspired among others by William Fishburn Donkin's idea that theories of probability are about how we should distribute our belief—this kind of inferential situation characterizes risk detection and causal assessment of harms, as opposed to classical hypothesis testing, and this made my research trajectory meet his work. So I decided, together with Thomas Augustin (Statistics Department at LMU), to invite him to Munich for a Lecture Series. This was hosted by the Munich Center for Mathematical Philosophy and the Statistics Department at LMU and gave philosophers, statisticians, as well as historians of statistics and mathematicians a great opportunity to interact with him and around his work. This interview is meant to continue this discussion with a wider audience.

Barbara Osimani
Munich Center for Mathematical Philosophy

Interview with Glenn Shafer

Barbara Osimani: How has your interest in the history of probability and statistics influenced your theoretical work on the various topics you have investigated?

Glenn Shafer: Early in my doctoral study in mathematical statistics, I discovered that seventeenth and eighteenth century writers had ways of thinking about probability on which I could build to develop alternatives to some prevailing dogmas. I was particularly intrigued by Hooper, Bernoulli, and Lambert, who used non-additive probabilities in much the same way as I used them in my 1973 dissertation and 1976 book, and by Bayes, whose argument for what we now call the definition of conditional probability revealed a fundamental issue that can limit the applicability of what we now call Bayesian reasoning. These authors did not help me mathematically, but their writings helped me develop philosophical positions that would never have emerged had I learned only from my contemporaries. Moreover, the experience of studying them has taught me to try to learn from older authors whenever I am puzzled by my contemporaries' philosophical assumptions.

Your question has made me reflect on how I came to study these early authors. I have always been interested in history, but I also had the good fortune to have three brilliant teachers who shared this interest. At Berkeley, I was introduced to the history of probability by a course taught by Florence Nightingale David, the pioneering author of *Games, God, and Gambling*. At Harvard, I took Art Dempster's course on the history of statistics and read historically rich chapters of his proposed book on statistical inference, unfortunately never completed. At Princeton, I participated in a very exciting seminar on the history of probability offered by Ivo Schneider, then a young visitor at Princeton and later a distinguished historian of science at Munich.

I was fortunate to encounter the history of probability and statistics at a time when it was a new world to conquer. The field has benefited from an immense amount of work since 1969, when I began my study of statistics, and most of this work has been done by individuals whom I know personally. Some of the figures whom we now consider part of that history were also still on the scene when I entered. Jimmie Savage and Bruno de Finetti were still publishing new work. During my first semester as a doctoral student at Berkeley, I attended Jerzy Neyman's seminar. For students of probability and statistics today, the history of the field must seem more intimidating, not an arena where they themselves can readily discover new insights that others have overlooked.

BO: What did you learn about Bayesian and non-Bayesian reasoning from reading Hooper, Lambert and Bernoulli (and Bayes himself) and how does this relate to the 'Dempster-Shafer rule of combination'?

GS: Discussions of Bayes's posthumous essay on probability, published in 1763, usually focus on his use of a uniform distribution to represent ignorance about an unknown probability. But as I argued in my 1982 paper on the topic ("Bayes's two arguments for the rule of conditioning", *Annals of Statistics* 10, 1075–1089), we should also think about his arguments for Bayesian updating: if at first we have probabilities P(A), P(B), and P(A&B), and we subsequently learn that B has happened, then we should change our probability for A from P(A) to P(A&B)/P(B). Nowadays we lead students to think that this is true almost by definition; we call P(A&B)/P(B) the "conditional probability of A given B" and write P(A|B) for it. But the notion of conditional probability was invented only in the late 19th and early 20th centuries. Bayes realized that we need an argument for changing our probability after we learn that B has happened, and he also realized that different arguments are needed depending on whether B is decided before or after A. If we are playing a game in which B is to be decided first, and the game defines the probability for A after B has happened or failed, then the relation P(A&B)=P(B)P(A after B has happened) follows from arguments made by Bayes's predecessors, especially Abraham De Moivre. But if A is to be decided first and we learn of B's happening without knowing whether A has happened or failed, then a different argument is needed, and the one that Bayes gave does not hold water.

This does not mean that Bayesian arguments are always wrong. It does mean that they depend on additional assumptions or judgements that are not being made explicit and may or may not be convincing. One way of filling the gap is to say that we must condition not only on B's having happened but also on the fact that we have learned B. In other words, we are working with a larger probability model than we at first thought or admitted, and in this larger model we had prior probabilities for what things we would learn and in what order we would learn them.

Another way of filling the gap is to make the additional judgement, after we have learned B, that certain probabilities (namely, the odds for a bet on A that was to be called off if B did not happen) remain valid. We can express this by saying that we judge our learning B to be independent of the uncertainties assessed by these probabilities. Or, using the game-theoretic form of Cournot's principle, we can say that learning B does not change our judgement that these probabilities cannot be beat.

When I first encountered the non-Bayesian arguments by Hooper, Lambert, and Bernoulli, probably in Schneider's seminar at Princeton, I immediately recognized them as examples of Dempster's rule of combination. (See my "Non-additive probabilities in the work of Bernoulli and Lambert", *Archive for History of Exact Sciences* 19 309–370, 1978.) George Hooper's rules for concurrent and successive testimony, published in the *Philosophical Transactions of the Royal Society of London* in 1699 (21 259–365) and in earlier more theological writings, have a simplicity that makes them particularly elegant examples of Dempster's more general rule. Hooper's rule for concurrent testimony tells us that if two independent witnesses testify to the same event, and we give them probabilities p and q of being individually reliable, then the probability that both are wrong is (1-p)(1-q), and so we can claim a degree of belief 1- (1-p)(1-q) that the event happened. His rule for successive testimony says that if we give witness A probability p of being reliable, we give witness B probability q of being reliable, and B says that A said that an event happened, then we can claim a degree of belief pq that the event happened. These arguments are non-Bayesian, and the degrees of belief involved are non-additive: the probabilities p and q that we give to the reliability of witnesses A and B, respectively, justify belief p and q in what they say being true but justify no confidence at all (i.e., zero belief) in it not being true.

In my 1973 dissertation and my 1976 book, *A Mathematical Theory of Evidence*, I studied *belief functions*, which are mappings that assign possibly non-additive degrees of belief to whole collections of propositions or events. Degrees of belief that are additive and hence qualify as probability measures also qualify as belief functions. Dempster's rule is a general rule for combining belief functions that are based on distinct bodies of evidence, involving independent uncertainties, to obtain a belief function based on the pooled evidence. Ever since it was formulated, first in papers Dempster published in the 1960's and then in my work in the 1970's, people have asked for more explanation of the notion of "independence" involved. Advocates of the Bayesian approach often pair this inquiry with the claim that their approach is clearer because they have definitions of independence and conditional probability. As I have already explained, I see the matter differently. As I see it, Bayesian conditioning is not a matter simply of applying a definition; it involves an constitutive judgement, a judgement about the independence of uncertainties that goes beyond what is initially given. Dempster's rule generalizes Bayesian conditioning and relies on the same kind of constitutive judgement.

This point has motivated much of my work over the past forty years. My conviction that Bayesian arguments involve the same kind of judgements that Dempster's rule requires led to my study of Bayes's posthumous article and to my subsequent work on conditional probability, constructive probability judgement, and causality in probability trees, and this led in turn to my collaboration with Vladimir Vovk on game-theoretic probability.

BO: This leads us to *Probability and Finance: It's only a Game!* co-authored with Vladimir Vovk. This book proposes a mathematical foundation of probability which fits into its diverse philosophical interpretations (subjective, frequentist). In contrast to the measure-theoretic approach, it does not rely on the establishment of a complete probability distribution over all relevant parameters from the outset. What advantages does this offer?

GS: Since the 1930's, and especially since the Ernest Nagel's *Principles of the Theory of Probability* in 1939, it has been a commonplace that Kolmogorov's axioms can be interpreted and used in different ways. They can be interpreted as axioms for objective probabilities, as axioms for degrees of belief, or as axioms for logical degrees of support. The game-theoretic foundation for probability Vovk and I proposed in our 2001 book deepens this idea by giving a richer structure that can be used in these various ways. We formalize the betting picture with which probability theory began (in the work of Pascal and Huygens, for example) in terms of modern game theory, obtaining a flexible game that typically has three players: (1) Forecaster, who prices gambles, (2) Skeptic, who decides how to gamble, and (3) Reality, who decides the outcomes. We can use games of this form in many ways. In my Munich lectures, I discussed a number of possibilities that Vovk and I and others have explored in our book and in subsequent articles:

1. If the role of Forecaster is played by a theory (quantum mechanics, for example), we can take the role of Skeptic and test the theory by trying to multiply the capital we risk by a large factor; it turns out that the usual statistical tests can be represented in this way.

2. To make probabilistic forecasts, we can put ourselves in the role of Forecaster and play to beat Skeptic's tests.

3. We can take the role of Forecaster in order to state prices that express our belief, perhaps saying that we are willing to offer these prices to all comers (this may be dangerous, because surely there are others who know more), or perhaps more cautiously saying merely that we do not think we can beat them if we take the role of Skeptic.

4. We can put an actual financial market in the roles of Forecaster (the day's opening prices being Forecaster's move) and Reality (the day's closing prices being Reality's move), so that the hypothesis that Skeptic, the investor, cannot multiply his capital by a large factor becomes a testable version of the efficient market hypothesis.

5. We can imagine that the probabilistic predictions are successful (inasmuch as Skeptic cannot beat them), but that the game is being played out of our sight, so that we see only some aspects of Reality's moves. This provides a way of thinking about causality: statistical relationships among events and variables can be explained by conjectured regularities in the unseen probabilistic predictions, and to the extent that these regularities are stable, they can be thought of as causal.

This picture does not contradict Kolmogorov's measure-theoretic axioms. Rather, it generalizes them. The most valuable result of the generalization is the richer context and sense of freedom it gives in applications. We know in our bones that games arise in all kinds of ways. We know that we can make them up. Thinking about different ways we can use a formal game frees us from any sense that we need to find the *true* meaning of probability.

As the question points out, the game-theoretic framework does not require us to assign probabilities to all the information that enters into the game as it proceeds. Many applied problems involve large amounts of information (the statistician's independent variables, the engineer's control variables, the physicist's measurement decisions, even a forecaster's predictions) to which it is not natural to assign probabilities. This information can be brought into the measure-theoretic framework only if it we pretend that it was determined in advance. The game-theoretic framework is therefore more comprehensive in its view of applications, in the sense that it can bring more of an applied problem inside its picture.

The game-theoretic framework can also lead to a wider acceptance of the variety of modes of probability judgement. In articles in the 1980s (see especially "Languages and designs for probability judgement", with Amos Tversky, in *Cognitive Science* 9 309–339, 1985), I suggested that a Bayesian analysis of evidence should be seen not as a normative exercise but as an argument that assesses given evidence by fitting it to a scale of examples, and that a belief-function analysis is similar, except that it uses different examples. This becomes more concrete and persuasive when the examples are games.

BO: How did the idea for the book emerge at all?

GS: In the 1980's Vovk was working in Moscow on the algorithmic theory of probability and randomness deficiency. Around 1991, he began to correspond with Phil Dawid and with me, as he saw common themes in the work of the three of us. For one thing, we were all trying to bring time back into the foundations of probability. Vovk considered Dawid's *prequential principle*, which requires statistical testing of successive probability predictions to depend only on actual predictions and outcomes, to be very important, and he was also interested in my work on probability trees and in my informal use of games to explain the unity of probability. At the time, I was mainly occupied with the theory of causality using probability trees that I later described in my book *The Art of Causal Conjecture* (1996), but I found the articles Vovk sent me fascinating and wanted to understand them better. We corresponded extensively, and in May of 1994 I visited him for a week in Moscow to discuss his ideas. In June of 1995, Steffen Lauritzen invited Vovk, Dawid, myself and others to a seminar at Aalborg at which I presented the main results of The *Art of Casual Conjecture* and Vovk presented some of his results. Already in 1991, Vovk had given a purely game-theoretic account of the strong law of large numbers, and by the time of the Aalborg meeting he had shown that Lindeberg's central limit theorem could also be interpreted game-theoretically. He had put these results in written form, but not in a style that I or many others could follow. According to Vovk's recollection, Lauritzen suggested that the two of us write a book together. As I recall, it was the following summer, as Vovk was arriving in the United States for a year at the Institute for Advanced Study in the Behavioral Sciences and my wife and I were about to leave for a year's sabbatical in Paris, that I agreed. Many of the book's ideas are closely related to what I had been doing with probability trees, and the book uses some of the language I had used for probability trees (e.g., *situation, cut, path*), but in the five years that we took to complete the book, I became convinced that the game-theoretic language lends itself better to a general and unifying picture.

BO: Why should one bet for your game-theoretic approach to probability, and how much would you set the odds?

GS: As I argued in my Munich lectures, the game-theoretic approach goes back to Blaise Pascal. We can also see it in the twentieth-century work of Jean Ville, Per Martin-Löf, and Claus-Peter Schnorr. So it has already been around for a good while. Surely it is a safe bet that it will not disappear. The modern formulation in *Probability and Finance: It's only a Game!* has sufficient mathematical depth and broad enough applications (to testing, forecasting, causality, finance, and the interpretation of subjective probability) that it will also endure.

The game-theoretic formulation generalizes the measure-theoretic formulation attributed to Kolmogorov and Doob. Will it become as widely used among mathematicians, engineers, management scientists, economists, statisticians, philosophers, and computer scientists as the measure-theoretic formulation? This is partly a sociological question, and the answer will surely vary by field. Vovk's work on game-theoretic probability in continuous time is ongoing, and other mathematicians have joined him in developing it. Time will tell how useful and attractive it will be as a framework for investigating new mathematical questions, modeling prices in continuous time, and so on. The eventual degree of interest among mathematicians will, of course, influence scholars in other fields. I am sure that engineers and management scientists will find our forecasting method useful, but it will be just one among probabilistic and non-probabilistic methods. Within economics the framework may be recognized as one more nonstandard way of thinking about probability, but the field's current odd but standard picture, according to which economic agents make decisions by expected utility relative to a true objective probability distribution that only the economist does not know, will continue its reign, because it is so productive of new theory. The intellectual foundations of mathematical statistics are more in flux. I think game-theoretic probability will find some role in statistics over the next couple decades, but we cannot expect it to resolve fully the deep tension between the objective and subjective aspects of probability that have characterized the field since Laplace.

News

Evidence, Inference, and Risk, 31 March–2 April

The ninth volume of the Munich-Sydney-Tilburg conference (MuST 9) took place at the University of Munich (LMU) in the period between the 31st of March and the 2nd of April 2016. This annually repeating conference, originally established by Stephan Hartmann and Maurice Schouten in 2008, is a joint undertaking between the Sydney Center for the Foundations of Science (SCFS), the Tilburg Center for Logic and Philosophy of Science (TiLPS) and the Munich Center for Mathematical Philosophy (MCMP).

The main aim of this year's volume of the conference series was to gather philosophers, natural and social scientists and statisticians in order to examine the theoretical and methodological issues involved in evidence evaluation, statistical inference and causal inference. The title of this year's volume and, accordingly, the thematic framework of the conference was "Evidence, Inference, and Risk", already indicating the strong interdisciplinary character of the meeting.

Keynote speakers of the conference were Lisa Bero (University of Sydney), Julian Reiss (University of Durham), Glenn Shafer (Rutgers Business School) and Jon Williamson (University of Kent). In total, 29 talks were given by people from a big variety of universities from many different countries. Highlights of the conference included (besides many others) the talk of Glenn Shafer on "Probability Judgement", in which he demonstrated how the idea of betting, historically underlying the mathematical theory of probability, is connected to the assessment of evidence via game-theoretic probability as well as a talk held by Julian Reiss defending minimalism of assumptions in statistical inferences and pointing out that most of the methods used in modern statistics are based on hardly-justifiable assumptions about the data generating process.

Moreover, quite a number of speakers covered topics belonging to the intersection of Philosophy, Pharmacology and Statistics. Specifically, contributors presented an approach for modeling decision making under causal uncertainty, taking explicitly into account the fundamental difference from ordinary statistical uncertainty (Danial Malinsky), a plea against fishing for significance in situations in which several statistical models seem to be appropriate to analyze the data in a biomedical study (Anne-Laure Boulesteix) as well as a contribution proposing a pluralistic approach for modeling the assessments of harms in an evidence amalgamation framework (Barbara Osimani).

The MuST conference was the grand finalé of a three week visit (15th of March–2nd of April) by Prof. Glenn Shafer to the MCMP and the Department of Statistics of the LMU. During this visit, Prof. Shafer also held a series of lectures on "Game Theoretic Probabilities" (15–21 March) and participated in (and contributed to) a workshop on the history of statistics jointly organized by the MCMP and the the Department of Statistics of the LMU (22–23 March). The first part of the lectures was guided by the book "Probability and Finance: It's Only a Game!" (Wiley, 2001) by Shafer and Vladimir Vovk and gave a very nice and closed overview on this alternative approach to formalizing probability. In the later lectures, Shafer demonstrated how classical subjective and objective interpretations of probability can be embedded in the game theoretical framework as well as how the theory can be applied in decision making, finance and in the modeling of causality. All lectures were accompanied by tutorials held by Shafer himself.

Additionally, also the two day workshop on the history of statistics had a lot to offer. Besides Shafer's contribution on the historical roots of the name "random variable", there were presentations on how the history of Munich's statistics department can be re-constructed by applying "oral history" techniques (Thomas Augustin and Rudolf Seising), on the influence of Wolfgang Stegmüller on the foundation of the Department of Statistics in Munich (Almond Stöcker), on the use of inverse probabilities in Laplace's work (Hans Fischer), on the history of statistical tests (Uwe Saint-Mont) and on the applicability of causal approaches for analogical inferences (Wolfgang Pietsch).

MuST 9: Evidence, Inference, and Risk, organized by Mark Colyvan, Paul Griffiths, Stephan Hartmann, Barbara Osimani and Jan Sprenger, was supported by the European Research Council (Grant 639276 and Grant 640638).

With MuST 9 having been a great pleasure for all participants, we are looking forward to the next volume of the conference!

Christoph Jansen
LMU Munich

Calls for Papers

A Hundred Years of Donald Davidson. His influence on Contemporary Philosophy: Special issue of *Argumenta*, deadline 30 June.

The Background of Constitutive Rules: Special issue of *Argumenta*, deadline 10 November.

What's Hot in ...

Uncertain Reasoning

In *Mathematics and Plausible Reasoning: Patterns of plausible inference*, G. Polya introduces random mass phenomena along the following lines. Consider raindrops falling on an ideally squared pavement, and focus on just two otherwise identical stones called Left and Right. It starts raining (conveniently one drop at a time) and we start recording the sequence of Left and Right according to which stone is hit by each raindrop. In this situation we are (reasonably) unable to predict where the next raindrop will fall, but we can easily predict that in the long run, both stones will be wet. This, Polya suggests, is typical of random mass phenomena: "unpredictable in certain details, predictable in certain numerical proportions to the whole".

The fact that we can often make reliable predictions on some aggregate, but fail to draw from this obvious conclusions on the individuals, has profound implications not only for the foundations of probability, but also for its practical applications. In medicine, for instance, this is quite the norm. In the absence of further information, what does the fact that a certain side effect of, say statins, is known to affect 1 in 100 patients say about you suffering from it? Problems like this raise the more general question: what is the extent to which forecasts on some aggregate can reliably inform us about its individuals? This question, and its philosophical underpinnings, are tackled by P. Dawid (2016: On Individual Risk, *Synthese*, available online).

Here's a motivating example from the paper

> Aharoni et al. (2013) tested a group of released adult offenders on a go/no go task using fMRI, and examined the relation between task-related activity in the anterior cingulate cortex (ACC) and subsequent rearrest (over 4 years), allowing for a variety of other risk factors. They found a significant relationship: ... whereas subjects with a high ACC activity had an estimated 31 % chance of rearrest, subjects with low ACC activity had a 52% chance [and conclude] "These results suggest a potential neurocognitive biomarker for persistent antisocial behavior". A newly released offender has low ACC activity: how should we judge his chance of rearrest?

To set the stage, statistician Phil Dawid compares a number of alternative interpretations of probability, which are distinguished according to whether they see probability as an attribute of individuals or groups. The resulting categories are called *individualist* and *groupist* respectively. This contrast is clearly related to the traditional 'subjective' vs 'objective' divide, though it is not equivalent to it. Whilst "individualist" theories promptly suggest a subjective (personal) interpretation, the term "groupist" suggests an intersubjective rather than fully objective view of probability.

The central part of the paper illustrates Dawid's ideas about how groupist and individualist theories relate, and does so by considering "Group to individual" (G2i) and "individual to Group" (i2G) bridges. An example of the former is provided by a suitably reintepreted version of de Finetti's representation theorem, which entitles the individualist not to worry about taking "expert valuations" arising from groupist frequencies—so long as they arise from exchangeable observations. A rather detailed explanation of this and its implications is provided by the author. If the (G2i) direction can be seen as a reinterpretation of known results, the (i2G) direction is where the author's contributions lie. Suppose that individual risks are known (or taken as primitive) how do they lead to 'group' frequencies? The answer is inspired by the property known as *calibration*, which boils down to the idea that individual estimates of risk should be in line with the way uncertainty resolves. The precise details of *which kind of calibration* suits best the purpose are discussed by the author in the final part of the paper. The desideratum is to have a calibration principle which is capable of providing sufficiently refined forecasts. To achieve this it is suggested, naturally enough, that all available information is used for calibration purposes. This eventually leads to a rather remarkable level of intersubjectivity which can be seen to bridge the "individual to Group" gap. I refer interested readers to Dawid's paper for more details.

Hykel Hosni
Philosophy, University of Milan

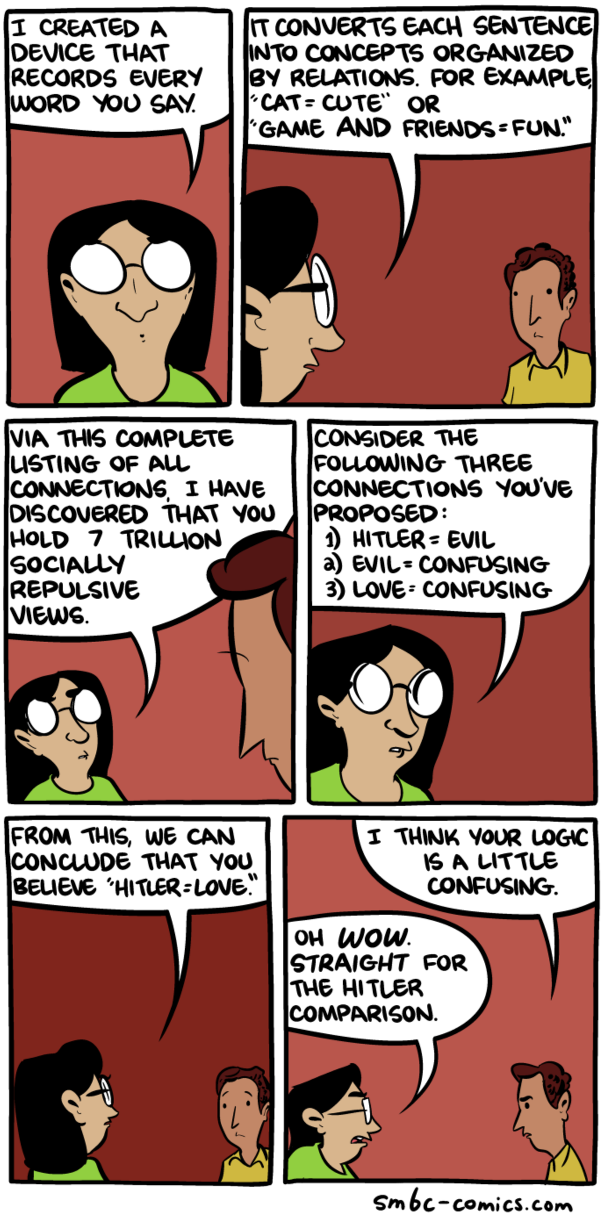
Evidence-Based Medicine

An editorial in *Nature* last month pointed out that although a large number of breastfeeding mothers take some form of medication, little research is carried out to determine whether the medicines a breastfeeding mother may take are in fact safe. Despite this, new mothers are often advised to breastfeed on the basis of evidence which suggests a link between breastfeeding and various desirable health outcomes. (There is a discussion of some of this evidence in another article in *Nature*.)

Should new mothers be encouraged to breastfeed if they are taking some form of medication? In the editorial, Janet Woodcock, the director of the Center for Drug Evaluation and Research at the FDA is quoted as saying: 'I had never received one word of information on that situation'. One possible of explanation of this is that until recently children tended to be excluded from clinical trials. And even though a number of health organizations now encourage paediatric clinical trials, there remain challenges to including children in clinical trials, challenges that result from ethical and other considerations (see, e.g., this paper on paediatric clinical trials). The editorial discusses a recent workshop in which researchers put forward a number of suggestions to overcome these challenges.

One suggestion is the following: 'Ethical questions can be addressed though careful study design, and by paying attention to the benefits of the extra monitoring for both individual babies and for mothers'. Another suggestion is to raise the profile of the problem by more clearly labelling drugs which have not been established as safe for use by breastfeeding mothers. The idea is that this may motivate co-operation in the relevant clinical trials. A further suggestion would be to consider a wider range of evidence than just the evidence that results from clinical trials, e.g., evidence that comes from basic science research. The editorial concludes with such a suggestion, and with the slogan: 'But basic researchers can contribute, too'. That slogan would make a good bumper sticker, if the EBM+ consortium ever consider a line of merchandise.

Michael Wilde
Philosophy, Kent



Events

June

T&PR: Workshop on Theoretical and Practical Reasoning, University in Leipzig, Germany, 2–4 June.
MCMP5: Five Years MCMP: Quo Vadis, Mathematical Philosophy?, Ludwig Maximilian University of Munich, 2–4 June.
DPSNM: Does the philosophy of psychiatry need a metaphysics?, Storey Institute, Lancaster, 3 June.
HLM: Hierarchical Linear Modeling, University of Connecticut, 6–10 June.
IDıS: Infinite Idealizations in Science, Ludwig Maximilian University of Munich, 8–9 June.

GEM: Ground, Essence and Modality, Helsinki, 8–10 June.

TT&P: Type Theory and Philosophy, University of Kent, Canterbury, 9–10 June.

R&MSDT: Reasons and Mental States in Decision Theory, London School of Economics, 9–10 June.

PoI: Workshop on the Philosophy of Information: The Role Of Data In Biomedical Sciences, University of Ferrara, Italy, 13–14 June.

EUT: Epistemic Utility Theory, University of Bristol, 13–15 June.

S&F: Spacetime and Fundamentality, Switzerland, 17 June.

PLFOM: Plural Foundations : Plural Logic and the Foundations of Mathematics, University of Leeds, 17 June.

CPW: Causation and the Physical World, University of Cologne, 17–18 June.

EBM+: New frontiers for evaluating evidence in medicine, University College London, 20 June.

SWE: Stockholm Workshop in Epistemology, Stockholm University, 20 June.

RML: Reliable Machine Learning in the Wild, New York City, 23 June.

CE: Chance Encounter, University in Groningen, Netherlands, 23–24 June.

MI: Mechanistic Integration and Unification in Cognitive Science, Warsaw, Poland, 23–26 June.

BDM: Workshop: Introduction to big data manipulation using Hive, University of Manchester, 24 June.

GLMM: Generalized Linear Mixed Models, University of Connecticut, 27–28 June.

RCS: Reasoning in Conceptual Spaces, Amsterdam, 28–29 June.

CFA: Causation: Foundation to Application, Jersey City, New Jersey, 29 June.

BD&DL: Big Data and Deep Learning in High Performance Computing, Porto, Portugal, 30 June.

July

CPR: Contemporary Perspectives on Reductionism, Prague, 30 June–1 July.

AAL: Australasian Association for Logic Conference, Melbourne, 30 June–2 July.

PM: Perspectival Modelling: Pluralism and Integration, University of Edinburgh, 2–3 July.

IH&PoS: Integrated History and Philosophy of Science, University of Edinburgh, 3–5 July.

SDH: Webinar: An introduction to survey data on health, online, 4 July.

BSPS: The British Society for the Philosophy of Science Annual Conference, University of Cardiff, 7–8 July.

SRAI: Statistical Relational Artificial Intelligence, New York City, 11 July.

FOMUS: Foundations of Mathematics: Univalent Foundations and Set Theory, Bielefeld, Germany, 18–23 July.

NRA: Knowledge, Reasons, and Action, Erlangen University, Germany, 21–22 July.

Courses and Programmes

Programmes

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

Master Programme: MA in Pure and Applied Logic, University of Barcelona.

Doctoral Programme in Philosophy: Language, Mind and Practice, Department of Philosophy, University of Zurich, Switzerland.

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

Master Programme: in Statistics, University College Dublin.

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

Master Programme: in Artificial Intelligence, Radboud University Nijmegen, the Netherlands.

Master Programme: Philosophy and Economics, Institute of Philosophy, University of Bayreuth.

MA in Cognitive Science: School of Politics, International Studies and Philosophy, Queen's University Belfast.

MA in Logic and the Philosophy of Mathematics: Department of Philosophy, University of Bristol.

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

MA in Logic and Philosophy of Science: Faculty of Philosophy, Philosophy of Science and Study of Religion, LMU Munich.

MA in Logic and Theory of Science: Department of Logic of the Eotvos Lorand University, Budapest, Hungary.

MA in Metaphysics, Language, and Mind: Department of Philosophy, University of Liverpool.

MA in Mind, Brain and Learning: Westminster Institute of Education, Oxford Brookes University.

MA in Philosophy: by research, Tilburg University.

MA in Philosophy, Science and Society: TiLPS, Tilburg University.

MA in Philosophy of Biological and Cognitive Sciences: Department of Philosophy, University of Bristol.

MA in Rhetoric: School of Journalism, Media and Communication, University of Central Lancashire.

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

MRes in Methods and Practices of Philosophical Research: Northern Institute of Philosophy, University of Aberdeen.

MSc in Applied Statistics: Department of Economics, Mathematics and Statistics, Birkbeck, University of London.

MSc in Applied Statistics and Datamining: School of Mathematics and Statistics, University of St Andrews.

MSc in Artificial Intelligence: Faculty of Engineering, University of Leeds.

MSc in Cognitive & Decision Sciences: Psychology, University College London.

MSc in Cognitive Systems: Language, Learning, and Reasoning, University of Potsdam.
MSc in Cognitive Science: University of Osnabrück, Germany.
MSc in Cognitive Psychology/Neuropsychology: School of Psychology, University of Kent.
MSc in Logic: Institute for Logic, Language and Computation, University of Amsterdam.
MSc in Mind, Language & Embodied Cognition: School of Philosophy, Psychology and Language Sciences, University of Edinburgh.
MSc in Philosophy of Science, Technology and Society: University of Twente, The Netherlands.
MRes in Cognitive Science and Humanities: Language, Communication and Organization: Institute for Logic, Cognition, Language, and Information, University of the Basque Country (Donostia San Sebastián).
Open Mind: International School of Advanced Studies in Cognitive Sciences, University of Bucharest.

Jobs and Studentships

Jobs

Lectureship: in Statistics, University College Dublin, deadline 7 June.
Lectureship: in Philosophy of Physics, University of York, deadline 7 June.
Junior-Professorship: in Mathematical Statistics, University of Göttingen, deadline 9 June.
Assistant Professor: in Practical Philosophy, Utrecht University, deadline 12 June.
Research Fellow: in New Directions in Philosophy of Mind, University of Cambridge, deadline 14 June.
Research Associate: in Statistical Genetics, University of Leicester, deadline 15 June.
Lecturership: in Statistics, University of Kent, deadline 17 June.

Studentships

PhD position: in Machine Learning for Geosciences, University of Valencia, Spain, deadline 1 June.
PhD position: in Medical Statistics, University College London, deadline 5 June.
PhD position: in Philosophy of Science, Leibniz University of Hannover and Bielefeld University, deadline 5 June.
PhD position: in Mathematical Statistics, Lund University, deadline 9 June.
PhD position: in Philosophical Logic, University of Bergen, deadline 10 June.