

---

# THE REASONER

---

VOLUME 1, NUMBER 8

DECEMBER 2007

[www.thereasoner.org](http://www.thereasoner.org)

## CONTENTS

§1 Editorial

§2 Features

§3 News

§4 Introducing ...

§5 Events

§6 Jobs

§7 Courses and Studentships

1 of integrating philosophers into research groups. Yet, I wasn't so glad to hear that my impression was indeed confirmed—namely that the practice of (at least) the social sciences is neglecting the 'theory' of scientific reasoning. This is something that goes beyond the development of complex statistical models or pushing buttons to run powerful statistical software. This is something that ought to precede scientific practice and that has to do with the very *foundations* of scientific reasoning. It is therefore a very fortunate circumstance that the newborn [Centre for Reasoning](#) at the University of Kent is setting up an [MA on reasoning](#). Hopefully the program will not just attract reasoning-oriented philosophy students, but also young practising scientists willing to improve their reasoning skills.

As mentioned above, we launch this month the section *Introducing ...*, which contains short introductions to key terms, authors and texts in logic. Selected pieces will be published monthly and then collected in a volume edited by Jon Williamson and by myself. We encourage Reasoners to send us feedback on those entries. Key terms, authors and texts are meant to be short pieces—between 50 and 1000 words—for first year undergraduate students. If you wish to collaborate, contact us at [thereasoner@kent.ac.uk](mailto:thereasoner@kent.ac.uk). All information is available on the [Key Terms](#) webpage.

Pieces in this issue span a variety of topics and disciplines, from the theory and practice of scientific reasoning, to mathematical arguments, data mining, etc. I would like to ask then, perhaps naively, whether we can define such a thing as 'scientific rigour', whether this is something relative to the domain of application or to

## §1

### EDITORIAL

It is always a pleasure to collaborate with *The Reasoner*, but particularly this time, as guest editor of the December issue. This is for two reasons—the first being that I had the opportunity to conduct an interview, and the second being that we are launching the section *Introducing ...*

As for the interview, I decided to ask two social scientists questions about scientific reasoning. Interestingly, their points of view were different enough to be expressed in separate answers, but similar enough to be put together in a single piece. Being (or trying to be!) myself a philosopher working on the epistemology and methodology of the social sciences, I was glad

culture, or whether logic is invariably at its basis. It would be interesting to receive Reasoners' experienced opinion on that.

Finally, I wish to renew the call for reports on conferences, workshops or any other kind of event related to reasoning that you attended or organised. We thank those who already contributed—it is indeed a valuable service to the scientific community to circulate ideas.

Federica Russo

Philosophy, University of Louvain

## §2

### FEATURES

#### Interview with Michel Mouchart and Guillaume Wunsch

Michel Mouchart and Guillaume Wunsch are emeritus professors, respectively, of statistics and econometrics and of demography at the University of Louvain (Belgium). Given their lifelong careers as practising scientists, I asked them about reasoning, notably about *scientific* reasoning.

Federica Russo: Before asking you a few questions about scientific reasoning in theory and practice, I'd like you to tell our Reasoners a few words about your academic background and on your area of expertise.

Michel Mouchart: At the undergraduate level, I studied commercial sciences and thereafter theoretical economics. I also earned a PhD in economics on econometric issues. In between, I worked 3 years in Santiago (Chile): 1 year as a teacher in statistics and O.R. at CIENES (Centro Interamericano de Enseñanza de la Estadística) and 2 years at the University of Chile. After the PhD I was appointed professor at UCL (Belgium) in statistics and econometrics. Progressively I became more and more interested in mathematics, and since my PhD my research interests varied between theoretical statistics and econometrics, with a rather strong flavor of mathematics, and applied micro-econometrics, working mainly on modal choice for the home-work trip, along with other mobility problems, and energy consumption at the household level. This applied research focused on modeling individual—or, household—behavior and raised up a growing interest for further research on the problem of how to build a statistical model.

Guillaume Wunsch: After obtaining degrees in political science and in economics at the University of Louvain in Belgium, I studied demography at the University of Paris and then went off to London with a British Council scholarship. Afterwards, I worked for some time at the United Nations Population Division

in New York, and eventually went back to Louvain—after obtaining my doctoral degree in demography—as a professor of methods of demographic analysis. I am now an emeritus professor, still actively involved in research however. My research was initially focused mainly on demographic methods and on morbidity and mortality. I quickly became aware that there were many *explanation-seeking why questions*, as Hempel would say, in my field of research and I became interested in causality and causal reasoning, even writing a book some twenty years ago on causal research in the social sciences.

FR: Do you think there is such a thing as “scientific reasoning”, or is it a highly specialised, domain-relative task?

MM: This question on scientific reasoning suggests two associated questions: How do scientists reason today? and Why do they use that way of reasoning? For the “How” question, my feeling is that scientific reasoning is based on two ingredients: firstly, a systematic recourse to standard boolean logic, and secondly, a continuous feedback mechanism “observation-modeling-observation-modeling ...”. As a matter of fact, the object of science is a continuous endeavor to try to understand, i.e. to make sense of observations. For this reason I tend to maintain a methodological unity of science even if the context may be dramatically different among different scientific disciplines. For the “Why” question, I think that the motivation behind is the need for communication: if my reasoning were not recognized as scientific, I could not be sure that my colleagues would understand me, and they possibly wouldn't pay much attention to my reasoning. This also means that scientific reasoning is relative to a culture, evolving in time and space.

GW: We all reason, I hope, in ordinary life but scientific reasoning follows a series of guidelines that do not always pertain to ordinary or common reasoning, though they are probably common to all sciences. For example, evidence-based medicine observes a set of well-known criteria concerning the quality of evidence and of methodology that can also be followed outside of medicine, in social science or psychological research for example. The observational methods vary of course from one science to another and the tools of analysis too, but the general scientific rules of reasoning are valid e.g. for history as well as for chemistry I believe.

FR: Jan L.A. van Snepschent once said: “In theory, there is no difference between theory and practice. But, in practice, there is”. Do you think the same holds for scientific reasoning?

MM: One way of considering that issue is to notice that a theory aims at being “general” whereas any application is specific to a particular case. Alternatively a theory is, by nature, simpler than a real application. The conscientious scientist should therefore evaluate

whether such a difference is due to a flaw in the theory and/or in the practice or whether it may reflect a difference of scope.

GW: Take the example of causal reasoning. Many criteria for the diagnosis of causation and the assessment of evidence can be found in the literature. For example are the observed variables in the correct temporal order? Is the relation between presumed cause and effect a strong one? How does the effect differ according to a variation of the cause? Is the presumed causal relation observed within different sub-populations? Etc. Though I apply these criteria to a large extent in my applied research, I hardly use them in my everyday life. Psychological research has indeed shown that common reasoning relies heavily on one's beliefs and on such Humean criteria as contiguity and co-occurrence that would be largely insufficient for scientific causal reasoning.

FR: Prof. Mouchart, once you told me that practising scientists are like chefs—they cook all their life long and they start worrying about dietary issues only at the end of their career. Why is it so, in your opinion? Prof. Wunsch, do you share this analogy?

MM: I am not going to question what I once said! I see two reasons for this state of affair. Firstly, experience actually accumulates with dissatisfaction for more and more unresolved questions. After some time it becomes natural to think “that's enough, I should really try to elaborate more on these questions”. Another reason is that questioning scientific practice is probably not professionally rewarding: colleagues do not like to be questioned and journals are not eager to publish such kind of contributions. A definite privilege of aging is that one feels less pressure for publishing and enjoys more liberty for selecting research interests.

GW: When you are older and retired, you have the time to think about more fundamental issues relating to one's scientific practice. When you are younger, you have to comply to the “publish or perish” motto, meaning that you cannot lose your time writing about fundamental problems that no scientific journal would publish anyway, especially if you have a heavy teaching load and administrative tasks in addition to your research. Science has become too often a “fast food” business, to come back to the cooking analogy. Good science, like a good Burgundy wine, needs time to mature.

FR: Do you think that curricula (master and PhD) in quantitative social science, which is, broadly, your area of expertise, pay too much attention to the theory or to the practice of scientific reasoning? How can a balance be restored? Does it have to be restored at all?

MM: It seems to me that the situation is actually very different from one department to another. In broad terms, a department or a teacher may not provide a better teaching than her(his) actual experience in that field.

GW: When I was teaching demography some years ago, we had a course given by one of my colleagues—Hubert Gérard—on theorising. Not on population theories but on the theoretical procedures for developing theories in population studies, moving from background knowledge and the conceptual framework to the empirical model. This course was however dropped when my colleague reached retirement age. Every student in the social sciences now knows how to obtain results using very complicated statistical models, as statistical software has become so user-friendly. Unfortunately, most social science students do not understand these models and do not know the conditions under which they should be used. Even worse, they often do not know why they are using these models for the problem at hand—some methods just seem to be fashionable. This seems to me a question of too much emphasis on practice and not enough on theory!

FR: A last question. We, that is the three of us, have been working together for about three or four years now. In theory, your collaboration with philosophers has been very fruitful and useful, or at least this is my impression. But in practice?

MM: The vast majority of my published papers have been co-authored and I always chose simultaneously the topics of my research and the persons with whom I was willing to collaborate; for this issue a congenial relationship was an essential ingredient. This time I have repeated such a strategy (to my greatest satisfaction!), taking into account my answer to the fourth question above.

GW: I often say half-jokingly that a philosopher should be on every research team, whatever the field. For philosophers on the one hand, it is a good way of experiencing how science is actually done, far from the arm-chair view of scientific practice unfortunately so common in philosophy of science papers. For the practising scientist on the other hand, it is a way of confronting his / her practical interests to broader concerns: What is actually the problem to be solved? Why is one using this approach instead of another? What for? Has one satisfied the conditions for a good explanation? Etc. In my viewpoint, in conducting scientific research, the philosopher should be a Guardian Angel of the Scientist.

## **Automating the Design of Data Mining Algorithms**

In essence, data mining consists of extracting useful knowledge (or patterns) from real-world datasets—see e.g. Witten & Frank (2005: *Data Mining: Practical Machine Learning Tools and Techniques*, 2nd Ed., Morgan Kaufmann). It can also be viewed as a form of automated reasoning, where the data mining system has

to reason about which pieces of knowledge or patterns better describe regularities in the data.

Data mining is an inter-disciplinary field, mainly involving several areas of computer science—notably machine learning, a sub-area of artificial intelligence, and database systems—and statistics. This short article focuses on data mining from a machine learning perspective. According to Langley (1996: *Elements of Machine Learning*, Morgan Kaufmann): ‘...if machine learning is a science, it is a science of algorithms.’ Under this perspective, the basic goal of a data mining scientist or researcher is to discover the best possible algorithm for analyzing real-world datasets. This basic goal can be pursued in different ways, in particular by designing a new data mining algorithm or analyzing existing data mining algorithms in order to identify their pros and cons. In this article we focus on the former type of data mining research, i.e., the design of new data mining algorithms.

In general, in a data mining research project, the main output of the project is a new data mining algorithm. That algorithm can then be implemented as a computer program in any chosen programming language. From the point of view of a data mining user, once the new algorithm has been implemented in a computer program, the process of analyzing her/his data is now a partially automated process. We say “partially” because in practice the user still needs to do some preliminary high-level analysis of the data in a pre-processing stage, as well as to do some analysis of the results of the data mining program in a post-processing stage, and these basic steps of data pre-processing, data mining, and discovered-knowledge analysis may need to be repeated several times. The iterative application of these steps constitutes the so called “knowledge discovery process”. In any case, the “core” of the knowledge discovery process, the extraction of knowledge or patterns from the data, is considerably automated by running a given data mining program on the target dataset.

Interestingly, however, there is a certain irony here about what is being automated. From the user’s perspective, the actual extraction of knowledge or patterns from the target dataset has been considerably automated, but the actual process of designing a data mining algorithm is still essentially a purely manual process. In other words, the data mining research community as a whole is working hard to automate the data analysis tasks of users, but they are not practising what they preach!, in the sense that they are not automating the design of data mining algorithms. This short article proposes a new level of automation in data mining, where the design of a data mining algorithm, usually considered a “creative act” of human experts, is also automated. In addition to being an interesting research direction by itself, from the point of view of artificial intelligence and automated reasoning, there are more im-

portant technical motivations to automate the design of data mining algorithms.

First, it is well known that no data mining algorithm is the best across application domains. In practice, when one wants to obtain the best possible knowledge for a given dataset in an important application domain, one empirically tries to apply several different data mining algorithms to the data, and simply selects the best algorithm, i.e., the algorithm producing the best result (e.g. the highest predictive accuracy in the case of the classification task). This empirical approach is normally necessary because it is not feasible to ask a data mining expert to design a brand new data mining algorithm tailored to the current dataset. Given the ubiquitous application of data mining in science, industry, etc, the number of datasets being mined or to be mined in the near future exceeds by far the number of experts in the design of data mining algorithms. However, once a computational system is available for automatically creating new data mining algorithms, one can simply run the system in a way that it automatically creates a data mining algorithm tailored to the dataset being mined—see Pappa (2007: *Automatically Evolving Rule Induction Algorithms with Grammar-Based Genetic Programming*, PhD Thesis, Computing Laboratory, University of Kent).

A second and related reason is that, despite the large diversity of data mining algorithms available in the literature, since (almost) all of them have been manually designed, they inevitably incorporate human biases and preconceptions. A machine-created data mining algorithm could in principle have a very different bias, complementing (instead of replacing) the application of existing data mining algorithms.

It is not possible to describe a system that automatically creates new data mining algorithms in this short article, but such a system is described in Pappa (2007) and Pappa & Freitas (2006: ‘Automatically evolving rule induction algorithms,’ In: Proc. ECML-2006 (17th European Conf. on Machine Learning), Lecture Notes in Artificial Intelligence 4212, pp. 341-352, Springer). The system described in these references automatically creates one type of data mining algorithms—namely, rule induction algorithm—but the basic idea could be extended to automate the design of other types of data mining algorithm.

To conclude, this paper suggests that researchers start to seriously consider the automation of the science of data mining, i.e., the automation of the process of designing (or discovering) the best possible data mining algorithm for the target problem.

Alex A. Freitas

Computing Laboratory, University of Kent

Gisele L. Pappa

Computing Laboratory, University of Kent

## Truth and truthmakers: A reply to Bourne's negation

In a recent paper, Craig Bourne (2004: "Future contingents, non-contradiction, and the law of excluded middle muddle", *Analysis* 64(2): 122-128) attempted to make good on Aristotle's alleged position about future contingents, namely: to preserve the law of excluded middle (hereafter: LEM):  $p \vee \neg p$ , while rejecting bivalence: not every sentence is either true or false. Łukasiewicz (1920: "O logice trójwartościowej", *Ruch Filozoficzny* 5: 170-171) attempted to do so with his non-bivalent logic  $\mathcal{L}_3 = \{1, 1/2, 0\}$ ; but the normal behaviour of negation here:  $\neg(1/2) = 1/2$ , implied that excluded middle was again not valid: LEM is not true when  $p$  is neither true nor false.

How to preserve such an alleged 'law' without bivalence? Bourne claims that

The solution rests on the following observation: it is the definition of  $\neg$  that causes the trouble. Thus we should stop trying to patch up the obvious deficiencies in Łukasiewicz's system (...) and deal with the root directly. For not only does Łukasiewicz's definition of  $\neg$  create the difficulty, I see no reason to think that it is correct, and thus altering it is not fudging it. (Bourne 2004: 124)

Bourne's position turns out to be both appealing and formally efficient: positing that  $\neg(1/2) = 1$  helps to have the expected result for excluded middle:  $(p \vee \neg p) = (1/2 \vee \neg(1/2)) = (1/2 \vee 1) = 1$ , thus relating a non-normal negation to Aristotle's alleged position. Bourne presents his solution as both plausible and convenient:

The justification for the  $\neg(1/2) = 1$  entry is as follows: given that  $p$  is indeterminate, then it isn't the case that  $p$ ; so to say that it is not the case that  $p$  is clearly to say something true. Thus, there is no justification for holding that the negation of a proposition can only be true if that proposition is false, as in Łukasiewicz's system. (ibid.)

I do see one such justification, however appealing and convenient Bourne's matrix may be: the point is that Bourne seems to make a confusion between two distinct senses of 'truth' regarding what *is* and what is *said* to be; the former concerns *truthmakers*, i.e. *that* which makes a sentence true, whereas the latter concerns *truth-bearers*, i.e. the sentence itself. Assuming that a sentence (or propositional content) expresses a fact and its particular utterance by a speaker results in a statement, we say that:

- a truthmaker is a fact or 'state of affairs' expressed by a sentence (e.g., that the sea-fight will occur tomorrow),

whereas

- a truth-bearer is a sentence uttered by a statement (e.g. "the sea-fight will occur tomorrow", a speaker says).

Although 'truth' may be variously assigned to sentences or statements, it only relates to truthmakers for Aristotle, in the sense that any sentence is 'true' only if it matches with an actual case. Admittedly, truth-values may be used iteratively, as when we say that it is 'the case' that something is so or not. If someone asserts that a given sentence expresses a falsity, then asserting it to be false may be said to be 'the case' by means of a that-clause: "That  $p$  is false is the case". This is a clear-cut difference between two distinct senses of truth-values, depending upon whether they are about states of affairs or sentences expressing them.

Now when Bourne assumes  $\neg(1/2) = 1$  in order to state iteratively that it is the case that  $p$  is not the case, what is 'true' is stating that the sentence  $p$  is not true, and not  $p$  itself. Following Bochvar (1938: "On a three-valued calculus and its application to analysis of paradoxes of classical extended functional calculus", *Matematičeskij Sbornik* 4: 287-308), Bourne seems to have mixed two distinct senses of negation in his assumption: an *internal* sense and an *external* one, where internal negation is a *sentence-forming* operator on sentences while external negation is a *statement-forming* operator on sentences; the latter can be marked by an assertion operator  $A$ , with  $A$  for asserting (the truth of)  $p$  and  $\neg Ap$  for not asserting  $p$ . So while Bourne noted that his matrix was the same as Bochvar's logic of assertion, he didn't equally note that the logical form of 'his' LEM would result in  $(p \vee \neg Ap)$  rather than  $(p \vee \neg p)$  or  $(Ap \vee \neg Ap)$ . Is  $(p \vee \neg Ap)$  still LEM, given its logical form?

Such a discrepancy is syntactically marked by Bourne between two distinct scopes of negation with respect to a future tense operator  $F$ : "It will be the case that Dr Foster does not go to Gloucester" ( $F\neg p$ ), as opposed to "It won't be the case that Dr Foster goes to Gloucester" ( $\neg Fp$ ) (126). The difference in scope may be rendered in a temporal square of oppositions where  $F\neg p$  and  $\neg Fp$  are contrary and contradictory with respect to  $Fp$ , respectively. Bourne wanted to show that any adequate formulation of LEM for future contingents would result in  $(Fp \vee \neg Fp) = (1/2 \vee \neg(1/2)) = 1$ , where the negated formula is not a sentence but its statement. That Bourne equates  $\neg(1/2)$  with 1 purports to mean that whoever urges not to believe something untrue tells the truth. But if so, his matrix makes a confusion between two sorts of 'truth', i.e. *being* true and *telling* the truth.

A least precondition would be to have one univocal sense of 'truth' in one and the same matrix, so that: either 1 and 0 only concern sentences and  $\neg(1/2)$  does not

yield 1 but 1/2, because being ‘true’ relies on truthmakers and intends to express an actual state of affairs; or 1 and 0 only concern statements and the initial value 1/2 become vacuous, so that bivalence is restored. In a nutshell, Bourne’s many-valued matrix is either misleading or irrelevant.

Fabien Schang  
Philosophy, University of Nancy

## A constructive definition of the intuitive truth of the Axioms and Rules of Inference of Peano Arithmetic

There is a remarkable, albeit unremarked, consequence of Turing’s seminal 1936 paper (‘On computable numbers, with an application to the Entscheidungsproblem,’ Proceedings of the London Mathematical Society, ser. 2. vol. 42 (1936–7), pp. 230–265). It admits a constructive definition of *what* is meant *intuitively* by the assertion that the axioms and rules of inference of a first-order Peano Arithmetic are *intuitively* true under its standard interpretation in Tarski’s sense (1936: ‘The concept of truth in the languages of the deductive sciences,’ in *Logic, Semantics, Metamathematics, papers from 1923 to 1938*, Hackett).

Specifically:

**Meta-theorem:** If a formula, eg.  $[R(x_1, x_2, \dots, x_n)]$ , is a theorem of a first-order Peano Arithmetic, then there is a Turing-machine, T, such that, given *any* set of natural numbers  $(a_1, a_2, \dots, a_n)$  as input, T will compute the arithmetical proposition  $R(a_1, a_2, \dots, a_n)$  as TRUE in a finite number of steps.

**Proof:** Consider the PA-axioms:

- A1:**  $[(x_1 = x_2) \rightarrow ((x_1 = x_3) \rightarrow (x_2 = x_3))]$ ;
- A2:**  $[(x_1 = x_2) \rightarrow (x'_1 = x'_2)]$ ;
- A3:**  $[0 \neq x'_1]$ ;
- A4:**  $[(x'_1 = x'_2) \rightarrow (x_1 = x_2)]$ ;
- A5:**  $[(x_1 + 0) = x_1]$ ;
- A6:**  $[(x_1 + x'_2) = (x_1 + x_2)']$ ;
- A7:**  $[(x_1 \star 0) = 0]$ ;
- A8:**  $[(x_1 \star x'_2) = ((x_1 \star x_2) + x_1)]$ ;
- A9:** For any well-formed formula  $[F(x)]$  of PA:

$$[(F(0) \rightarrow (\forall x)(F(x) \rightarrow F(x')))] \rightarrow (\forall x)F(x).$$

Now, each of the PA axioms can intuitively be seen to be Turing-computable as *always* TRUE in the following, definitional, sense:

**D1:** A total number-theoretical relation,  $R(x_1, x_2, \dots, x_n)$ , when treated as a Boolean function, is Turing-computable if, and only if, there is a Turing-machine T such that, for *any* given natural number sequence,  $(a_1, a_2, \dots, a_n)$ , T will compute  $R(a_1, a_2, \dots, a_n)$  as either TRUE, or as FALSE.

**D2:** If  $[R]$  is an atomic formula  $[R(a_1, a_2, \dots, a_n)]$  of PA, then  $[R]$  is Turing-computable as TRUE/FALSE for the natural number input  $(a_1, a_2, \dots, a_n)$  if, and only if, the arithmetical relation  $R(a_1, a_2, \dots, a_n)$  is Turing-computable as TRUE/FALSE on the natural number input  $(a_1, a_2, \dots, a_n)$ .

**D3:** The PA-formula  $[\neg R]$  is Turing-computable as TRUE for the natural number input  $(a_1, a_2, \dots, a_n)$  if, and only if,  $[R]$  is Turing-computable as FALSE for the natural number input  $(a_1, a_2, \dots, a_n)$ .

**D4:** The PA-formula  $[R \rightarrow S]$  is Turing-computable as TRUE for the natural number input  $(a_1, a_2, \dots, a_n)$  if, and only if, either  $[R]$  is Turing-computable as FALSE for the natural number input  $(a_1, a_2, \dots, a_n)$ , or  $[S]$  is Turing-computable as TRUE for the natural number input  $(a_1, a_2, \dots, a_n)$ .

**D5:** The PA-formula  $[R]$  is Turing-computable as *always* TRUE if, and only if,  $[R]$  is Turing-computable as TRUE for *any* given natural number input  $(a_1, a_2, \dots, a_n)$ .

**D6:** The PA-formula  $[\neg R]$  is Turing-computable as *always* TRUE if, and only if,  $[R]$  is Turing-computable as FALSE for any given natural number input  $(a_1, a_2, \dots, a_n)$ .

**D7:** The PA-formula  $[(\forall x_i)R]$  is Turing-computable as TRUE if, and only if,  $[R]$  is Turing-computable as TRUE for *any* given natural number input  $(a_1, a_2, \dots, a_n)$ .

**D8:** The PA-formula  $[\neg(\forall x_i)R]$  is Turing-computable as TRUE if, and only if  $[(\forall x_i)R]$  is *not* Turing-computable as TRUE.

Thus if we assume, for instance, that axiom **A1** is intuitively true in the Tarskian sense—i.e. that the PA-formula,  $[(x_1 = x_2) \rightarrow ((x_1 = x_3) \rightarrow (x_2 = x_3))]$ , interprets as an arithmetical relation,  $(x_1 = x_2) \rightarrow ((x_1 = x_3) \rightarrow (x_2 = x_3))$ , which holds for *any* substitution of natural numbers for the variables contained in it—then it follows that **A1** interprets as an arithmetical relation that is Turing-computable as *always* TRUE.

Similar arguments hold for the axioms **A2** to **A8**.

Next, if we assume that the Induction axiom, **A9**, is intuitively true in the Tarskian sense, then, again, we have that the arithmetical relation expressed by:

$$(F(0) \rightarrow (\forall x)(F(x) \rightarrow F(x'))) \rightarrow (\forall x)F(x)$$

is Turing-computable as always TRUE, since:

(a) If  $F(0)$  is Tarskian-true intuitively, then  $F(0)$  is Turing-computable as *always* TRUE;

(b) If the arithmetical relation,  $(\forall x)(F(x) \rightarrow F(x'))$ , is Tarskian-true intuitively—i.e.,  $(F(x) \rightarrow F(x'))$  holds for *any* given natural number  $x$ —then  $(F(x) \rightarrow F(x'))$  is Turing-computable as *always* TRUE;



(c) If  $F(0)$  is Turing-computable as *always* TRUE, and  $(F(x) \rightarrow F(x'))$  is Turing-computable as *always* TRUE, then  $F(x)$  is Turing-computable as *always* TRUE.

Further, the following rules of Inference in PA preserve Turing-computability:

**Modus Ponens:**  $[B]$  follows from  $[A]$  and  $[A \rightarrow B]$ ;

**Generalisation:**  $[(\forall x)A]$  follows from  $[A]$ .

In other words, if we assume under the standard interpretation that:

Whenever the PA-formulas  $[A]$  and  $[(A \rightarrow B)]$  interpret as Tarskian-true, then the PA-formula  $[B]$  interprets as Tarskian-true;

then:

$[B]$  interprets as *always* Turing-computably TRUE if  $[A]$  and  $[(A \rightarrow B)]$  interpret as *always* Turing-computably TRUE.

Similarly, if we assume under the standard interpretation that:

If the PA-formula  $[A]$  interprets as Tarskian-true, then so does the PA-formula  $[(\forall x)A]$ ;

then we have the tautology:

$[A]$  interprets as *always* Turing-computably TRUE if  $[A]$  interprets as *always* Turing-computably TRUE.  $\square$

The remarkable feature of this interpretation is that if  $'[(\exists x)R(x)]'$ —formally defined as  $'[\neg(\forall x)\neg R(x)]'$ —is PA-provable, we cannot conclude that there must exist a natural number  $n$  such that the arithmetical proposition  $R(n)$  holds. We can only conclude that  $R(n)$  is not Turing-computable as always FALSE.

Now, PA is  $\omega$ -consistent if, and only if, there is no PA-formula such as  $[F(x)]$  for which:

- (i)  $[\neg(\forall x)F(x)]$  is PA-provable, and,
- (ii)  $[F(n)]$  is PA-provable for any given numeral  $[n]$  of PA.

So, unless we assume that PA is  $\omega$ -consistent,  $R(n)$  may still be false for any natural number  $n$ !

The question arises: Is PA  $\omega$ -consistent?

Bhupinder Singh Anand  
Mumbai

## Neo-Fregean Unnatural Numbers

A considerable programme of research, recently, has been concerned to provide a foundation for Arithmetic based on Hume's Principle, together with suitable definitions (see for instance, Hale, B. and Wright, C., 2001: *The Reason's Proper Study*, Clarendon, Oxford). Hume's Principle says that the number of F's is the same

as the number of G's just so long as the F's and the G's can be put into one to one correspondence—in short:

$Nx:Fx = Nx:Gx$  iff the F's are 1-1 with the G's.

Together with an initial definition of zero, such as

$Nx:(x \neq x) = 0,$

and subsequent definitions of the natural numbers, in the style

$Nx:(x = 0) = 1,$

$Nx:(x = 0 \vee x = 1) = 2,$

etc., the principle has been taken to allow the proof of all arithmetical facts (see the appendix of Wright, C., 1983: *Frege's Conception of Numbers as Objects*, Aberdeen University Press, Aberdeen.)

The only major hurdle to the completion of this project has seemingly been the 'Caesar Problem', since no guidance is given by Hume's Principle to establish the truth or falsity of identities other than those of the form 'the number of F's = the number of G's'. Specifically, identities of the form

$Nx:Fx = \text{Caesar},$

cannot be evaluated, and this troubled Frege, since he wanted functions like 'the number of F's =  $x$ ' to be defined for all arguments. But it would seem to be possible to rule out the latter kind of identity on grammatical grounds, by showing that names of people are of a different type, or category from names for numbers.

The intensity of interest in this Neo-Fregean line of research, and the concentration on the Fregean difficulty with it, is rather a puzzle, however. For it is quite easy to see, given a moment's critical reflection, that one to one correspondence between the F's and the G's does not establish the intended interpretation of ' $Nx:Fx$ ', and ' $Nx:Gx$ '. If the F's can be put into one to one correspondence with the G's, then not only is the number of the F's the same as the number of the G's, but also any function of one of those numbers has the same value as the same function of the other number. So there are, in addition to the above, a whole host of comparable, true equivalences like

$(2 + Nx:Fx) = (2 + Nx:Gx)$  iff the F's are 1-1 with the G's,

etc., and quite generally

$f(Nx:Fx) = f(Nx:Gx)$  iff the F's are 1-1 with the G's, where 'f' is any numerical function of one variable. Writing ' $f(Nx:Fx)$ ' as ' $Mx:Fx$ ', we therefore get true equivalences of the same form as Hume's Principle, showing that the right hand side of that principle does not enable one to distinguish the interpretation of ' $Nx:Fx$ ' as *the number of F's* from an interpretation in terms of any function of that number. The difficulty ramifies, of course, since the above definitions of zero and the following natural numbers merely produce  $f(0)$ ,  $f(1)$ ,  $f(2)$ , etc., for some indeterminate function 'f'.

What, in place of Hume's Principle, will ensure that the interpretation is the intended one? Clearly it would

be better, for a start, if one could obtain a series of identities on the left of the form

$$Nx:Fx = n,$$

where 'n' is a standard numeral. For instance, this could be achieved if the one to one correlation was made with some standard series of sets whose numbers of elements are known. Thus following the idea behind Frege's definitions of the natural numbers we might try saying:

$Nx:Fx = n$  iff the F's are 1-1 with the natural numbers from 0 to n-1.

But while this might be true, it will not serve as a definition of the natural numbers on account of its circularity. Moreover, it requires a prior understanding of what terms may be substituted for the variable 'n', which was the sort of issue that gave rise to the Caesar Problem in the first place.

There is no way out, therefore, but to presume a conventional set of terms for the numerals, and base the definition on appropriate properties of sets of them. In fact we do not have to look far for a suitable definition, once we remember the intimate connection between numbers and the elementary process of counting. For in counting some things we generate a one to one correspondence between those things and what are surely *the paradigm items* that have a number, namely the numerals. So we can get a definition of *the number of F's*, using a schematic variable 'n', in the following way:

$Nx:Fx = 0$  iff there are no F's,

$Nx:Fx = n$  iff the F's are 1-1 with the successive non-zero numerals up to 'n'.

This is not a 'logicist' account of the natural numbers, of course, since the subsequent derivation of the properties of the natural numbers is not based on pure logic but instead on formal properties of an arbitrarily given numeration system. However, it saves what is most valuable in the Fregean and Neo-Fregean accounts of the natural numbers—the reliance on one to one correspondences—while automatically solving the Caesar Problem.

Hartley Slater

Philosophy, University of Western Australia

## On Moral Responsibility and The Reflective Endorsement View

In her recent article, 'Moral Responsibility Without Libertarianism' (2006: *Nous* 40, 307-330), Lynne Rudder Baker has argued the sufficiency of what she calls the Reflective Endorsement View, which she presents as follows:

Letting \* denote an attribution to the agent of a first-person perspective (the capacity to appreciate that a desire is hers, without recourse to any name or description (2006: 315)).

RE A person is morally responsible for a choice or action X, if X occurs and:

- (i) S wills X.
- (ii) S wants that she\* will X.
- (iii) S wills X because she\* wants to will X, and
- (iv) S would still have wanted to will X even if she had known the provenance of her\* wanting to will X.

This argument can be summed up as follows: "S would have still wanted to will X even if she\* had known the [deterministic] provenance of her wanting to will X" (Baker 2006: 317). Baker claims that the Reflective Endorsement View captures the intuitions that both compatibilists and libertarians have regarding the dignity of the person. She assumes that once an agent reflectively endorses her desires as her own, even if they have causes that are beyond her control, we still intuitively hold her responsible for her actions.

In order to justify her argument Baker introduces the following example:

Example 1: Bobby Frank Cherry was recently convicted of a bombing that killed four black Sunday-School girls in a church in Birmingham in 1963. Let us suppose that as a proud white supremacist Cherry (a) willed to participate in the bombing (b) wanted to will to participate, and (c) willed to participate because he wanted to (d) he would still be proud of his participation, and would participate again, even though he knew that his wanting to will to participate had been caused by his racist upbringing (along with other factors beyond his control). Baker concludes that because he satisfies (i)–(iv) of RE we should find him morally responsible for his action.

If Cherry is determined by F, such that he could not do otherwise with respect to his actions, and if he identifies with his desire to murder, then it is impossible to discredit the fact that his desire and identification with it are also determined by F. If it is determined that he will do X at  $t_1$ , and if forming the second-order desire to X is integral to the actual performance of X, then his identification with the second-order desire to X must also be determined (for otherwise F would not be effective). The same is true of any other factors that are essential to Cherry's performance of X. Thus, we must also accept that his second-order desire is determined. If this is the case, then it was never possible that he not identify with his desire to murder in this instance.

It is condition (iv) of RE that Baker believes overcomes any difficulties that stem from the deterministic nature of the world. This is because if the agent still endorses his desires, given that he knows that the world is determined, he is responsible for his ensuing actions under RE. However, I think that what must be conceded



is that even if Cherry Reflectively Endorses his desire to murder, he could never do other than murder and Reflectively Endorse his desire to do so in a determined world due to factors F.

In order to illustrate the implications of my preceding analysis I will now construct a second example. Example 2: Let us further suppose that Cherry is aware that his desires have a deterministic provenance and identifies with them irrespective. However, Cherry proceeds to spend considerable time pondering the nature of determinism and comes to the conclusion that not only are his desires determined, but even his endorsement of those desires is determined. Cherry is dismayed by the implications of this fact and feels that in a significant sense his desires are not his own, and he is reluctant to take ownership of desires over which he cannot exercise the power to do otherwise. He would like to be able to have the capacity to endorse desires other than those that it has been determined he will endorse. It is my claim that because of this fact he no longer identifies with his desires in a sufficiently robust sense for him to be held morally responsible for them and the actions they give rise to.

Example 2 illustrates how even if an individual would identify with his desires if he knew he had a deterministic provenance, it is still possible that he would not identify with that endorsement if he knew that the endorsement itself was determined. If this was the case, then he would be exempt from moral responsibility under condition (iv) of RE. In light of this example, we can see that the initial intuition that led Baker to assume that clause (iv) of RE is essential to moral responsibility in fact gives rise to an additional clause, which is not as easily satisfied:

- (v) *S* would still have wanted to will *X* even if she had known the deterministic provenance of her\* Reflective Endorsement of her identification with wanting to will *X*.

The greatest problem with Baker's Reflective Endorsement View is that it is too strong. I think that we intuitively come to the conclusion that many people would be appalled if they knew that their identification with their desires and actions, and their Reflective Endorsement of that identification is determined. Should we therefore hold that they should not be held morally responsible for their actions? Should we withhold our morally reactive attitudes for those few who are perfectly content to have desires that are determined and are happy to know that their Reflective Endorsement of those desires originate in factors beyond their power to affect? Baker's thesis leads to the conclusion that many of the instigators of actions that we commonly understand to be morally deficient, cannot be held morally responsible for their actions. This seems a bizarre conclusion for someone who believes that compatibilism is

capable of encompassing our everyday intuitions about moral responsibility and the dignity of the person.

Richard H. Corrigan  
Philosophy, University College Dublin

## The Trouble with Pollock's Principle of Agreement

A recent exchange between Pollock and Fogdall focused on the plausibility of Pollock's *Principle of Agreement* (*The Reasoner* 1(3) 2007: 7–8, and 1(4) 2007: 6–8). Although Pollock fared well in the debate, the plausibility of the Principle of Agreement is incidental to Fogdall's initial point. To set things back on track, I will return to Fogdall's original question: "does direct inference *require* Pollock's Principle of Agreement?"

I agree with Fogdall that direct inference does not require the Principle of Agreement. In fact, I will go a little further and argue that the Principle of Agreement leads to very unattractive consequences in the presence of other theses maintained by Pollock. To make my point, I will sketch the role of the Principle of Agreement in Pollock's theory of direct inference, and then describe the unattractive consequences of the principle.

Where  $\rho(Y|X)$  is the proportion of members of *X* that are members of *Y*, Pollock's Principle of Agreement states:

For All sets *A* and *B*, and all  $r > 0$ : if *B* is infinite, then  $\rho(\rho(A|X) \approx_r \rho(A|B) \mid X \subseteq B) = 1$ .

The Principle of Agreement is central to Pollock's theory of direct inference. The impact of the principle stems from Pollock's definition of nomic probability. Where '**F**' and '**G**' denote the sets of physically possible *F*s and *G*s, the nomic probability of *F*s on *G*s (written "prob(*F*|*G*)") is defined by the identity  $\text{prob}(F|G) = \rho(\mathbf{F}|\mathbf{G})$ . The key principle of Pollock's theory of direct inference is derived from the Principle of Agreement. The key principle is as follows:

**Non-Classical Direct Inference:** If *F* is projectable with respect to *G*, then ' $\mathbf{H} \subseteq \mathbf{G} \ \& \ \text{prob}(F|G) = r$ ' is a defeasible reason for ' $\text{prob}(F|H) = r$ '.

*Non-Classical Direct Inference* does not allow one to directly assign *single-case* probabilities to singular propositions. Pollock's theory incorporates such assignments via a definition of single-case probabilities in terms of nomic probabilities. The definition identifies an agent's single-case probability for a singular proposition, *F<sub>c</sub>*, with the nomic probability of *F* conditional on the property of being *c* under conditions **K**, where **K** is the conjunction of the set of propositions that the agent is warranted in accepting. The exact property

conditioned on is the property  $x = c \ \& \ \mathbf{K}$ :

**Definition:**  $\text{PROB}(Fc) = \text{prob}(Fx \mid x = c \ \& \ \mathbf{K})$ .

Now that I have set out the basic elements of Pollock's theory, let us consider the unattractive consequences that derive from the Principle of Agreement.

The problem is simply that the Principle of Agreement sometimes leads to higher than desirable single-case probabilities. To see the problem note that if the Principle of Agreement is true, then statements of the form ' $\rho(\mathbf{F}|\mathbf{G}) = 1$ ' do not imply the truth of corresponding universal generalizations,  $(\forall x)(Gx \supset Fx)$ . A consequence of this is that there are cases where Pollock's theory of direct inference allows an agent to conclude that  $\text{PROB}(Fc) = 1$  (based on the premises that  $Gc$  and  $\rho(\mathbf{F}|\mathbf{G}) = 1$ ), when it is *epistemically possible* that  $c$  is *not F* (i.e.,  $c$  is *not F* is consistent with what the agent believes). The preceding puts Pollock's theory of direct inference on a collision course with *classical decision theory*. Classical decision theory prescribes that an agent who assigns probability *one* to a proposition,  $p$ , should behave *as if* there is no risk that  $p$  is false. While the prescription is reasonable, it leads to irrational behavior when paired with Pollock's theory of direct inference. Simply: it is irrational to behave as if there is no risk that  $p$  is false in a situation where *not p* is epistemically possible. Whatever the limitations of classical decision theory, it is clear in this case that the trouble originates from the Principle of Agreement.

If we accept that the Principle of Agreement is true, then it seems that we must reject some other component of Pollock's theory of direct inference. The alternatives are to reject the thesis that it is statements of nomic probability that underlie direct inference or adopt a new definition of nomic probability that does not connect nomic probabilities with *proportions* in a way that leads to the applicability of the Principle of Agreement. Whatever route is taken, the goal is the same, namely: disconnect the statistical statements that serve as premises for direct inference from the Principle of Agreement. To put the matter lightly: direct inference does not require the Principle of Agreement.

Paul Thorn

Philosophy, Arizona

### §3

## NEWS

### The Reasoning Club

This month sees the launch of [The Reasoning Club](#), a network of institutes and centres whose research focusses on the study of reasoning, inference and method.

The goals of the Reasoning Club are to foster the exchange of ideas between researchers working on reasoning-related topics, and to develop a sense of community for researchers spread geographically and spread across disciplines. The Club achieves these goals by facilitating free movement of researchers between institutes.

If you are affiliated to a member institute then you may travel freely between member institutes. You just need to inform the host institute of the dates of your stay.

Founding members of the Reasoning Club include the [Centre for Reasoning](#), University of Kent, UK; the [Institute for the History and Philosophy of Science and Technology](#), Paris, France; the [Centre for Logic and Philosophy of Science](#), Vrije Universiteit Brussel, Belgium; and the [Tilburg Center for Logic and Philosophy of Science](#), Tilburg, Netherlands. The Club is keen to welcome more members in the future. If your institute would like to be involved, please [let me know](#).

Member institutes will of course continue to welcome visitors from other organisations. Please see the individual institute web pages for details of the application procedures for academic visitors.

See <http://www.kent.ac.uk/reasoning/club/> for more information about the Reasoning Club.

Jon Williamson

Philosophy, Kent

### History, Philosophy, and Didactics of Mathematics, 1–4 November

From November 1 to November 4, 2007, the Institute of Philosophy in Bonn, Germany, hosted this year's Novembertagung on the History, Philosophy, and Didactics of Mathematics. The event was co-organized with the Seminar for Mathematics and its Didactics in Cologne.

The Novembertagung is an annual, international meeting aiming especially at master students and doctoral students in the history, philosophy and didactics of mathematics to discuss genuine work in progress. Speakers want to benefit from fresh conceptual and methodological comments on their presented ideas from an unbiased audience of young researchers.

One of today's central tasks for all three branches, history, philosophy, and didactics of mathematics, is to clarify the nature of mathematics by shifting actual mathematical practice, and the development of mathematical knowledge and understanding into the focus of research. This year's conference theme "Mathematical practice and development throughout history" was supposed to emphasize the need for a fruitful combination of the multiple tools from history, philosophy and didactics of mathematics to this end—the interplay of

these bordering disciplines appears as a most promising perspective for an adequate understanding of mathematics as a human endeavor. Altogether, we had 23 presentations from various branches of history, philosophy, and didactics of mathematics, including the invited lecture, which is traditionally held by a senior researcher; this year's invited speaker was Professor Leo Corry (Tel Aviv), who gave a talk on results from his research on the development of number theory (for particular details on speakers and abstracts please visit the conference homepage at <http://www.novembertagung.uni-bonn.de>).

A number of the presentations led to the issue of how general or significant our findings about mathematical practice and development can be, depending on our methodology. One question that was intensively discussed was the validity of rather general models of mathematical practice and development built on historical material. A further topic discussed in this regard was the role of empirical research from e.g. sociology, cognitive science, and psychology, for an adequate understanding of the nature of mathematics.

The participants also agreed on the location and dates of the Novembertagung 2008, which will now be held in Holbæk, Denmark, from November 5–9, 2008. The organizers will be Uffe Thomas Jankvist (Roskilde), Laura Turner, and Henrik Kragh Sørensen (both Aarhus). See <http://www.henrikkragh.dk/novembertagung> for the announcement.

What should be stressed in a closing remark is the special importance of interdisciplinarity at an event where young researchers have the possibility to present, as comments and suggestions from an interdisciplinary audience at an early stage of their investigations have greatest profitable impact on their ongoing work.

Eva Wilhelmus  
Philosophy, Bonn University

Ingo Witzke  
Didactics of Mathematics, Cologne University

## Philosophy of Information and Logic, 3–4 November

The First Workshop on the Philosophy of Information and Logic was held at the Philosophy Centre, at the University of Oxford, November 3–4 2007. The Workshop was a huge success, and we are happy to announce that it will most likely become a regular annual event. The proposal at this stage is for the second Workshop to be held in Amsterdam, sometime in late 2008. The Workshop was an official event of the IEG and was generously supported by the Faculty of Philosophy.

The aim of the workshop was strongly interdisciplinary in focus. Researchers from both philosophy and computer science departments were brought together for a weekend of intense interdisciplinary research. With nearly forty participants coming from New Zealand, North America, and Europe, the presenters included: Samson Abramsky (Oxford): *Towards Informational Dynamics*, Patrick Allo (Brussels): *A Two-Level Approach to Logics of Data and Information*, Alexandru Baltag (Oxford): *Reasoning about Informational Dynamics: A Dynamic Logic Approach to Epistemology*, Johan van Benthem (Amsterdam-Stanford): *Logic and Information (At Least) Three Views*, Marcello D'Agostino (Ferrara): *Is Propositional Logic Really Uninformative*, Luciano Floridi (Hertfordshire-Oxford): *Logical Fallacies as Informational Shortcuts*, Mark Jago (Nottingham): *Logical Information is Vague*, Edwin Mares (Wellington): *General Information*, Giuseppe Primiero (Ghent): *Becoming Informed*, Mehrnoosh Sadrzadeh (Southampton): *Okham's Razor applied to Reasoning about Information Flow*, Sebastian Sequoiah-Grayson (Oxford): *A Positive Information Logic*, Sonja Smets (Brussels): *Dynamic Logic meets Quantum Information*, and Chris Timpson (Oxford): *A Budget of Informations*.

The content of the presentations was diverse within the area itself. Topics ranged from the algebra and topology of quantum information states, logics of information flow in various contexts (a recurring theme being the information flow involved in logical reasoning procedures themselves), dynamic information states in multi-agent reasoning settings, to the nature of the informational turn in logic itself.

Selected Workshop proceedings will appear in a dedicated issue of *Synthese* (special section: *Knowledge, Rationality and Action*). The Workshop presentation slides, as well as photos of the event, will be available at the Workshop website: <http://www.philosophyofinformation.net/workshop/>.

We would like to express our sincerest thanks to everyone who took part in and supported the Workshop, and thus helped to make it such a successful event!

Sebastian Sequoiah-Grayson  
Philosophy, Oxford

Luciano Floridi  
Philosophy, Hertfordshire & Oxford

## Calls for Papers

**EVOLUTIONARY INTELLIGENCE**: Special Issue on Artificial Immune Systems, deadline 1 December 2007.

#### INTRODUCING ...

*The Reasoner* would like to publish very short introductions to key terms, people and texts in logic and reasoning. Selected pieces will also be published in a book “Key Terms in Logic” by Continuum. If you would like to contribute, please contact [TheReasoner@kent.ac.uk](mailto:TheReasoner@kent.ac.uk)

**HYBRID LOGIC:** Special Issue of the Journal of Logic, Language and Information, deadline 1 March 2008.

**MACHINE LEARNING IN SPACE:** Special Issue of the Machine Learning Journal, deadline 31 March 2008.

**CONDITIONALS AND RANKING FUNCTIONS:** Special issue of Erkenntnis, [franz.huber@uni-konstanz.de](mailto:franz.huber@uni-konstanz.de), deadline 31 May 2008.

#### §4

#### INTRODUCING ...

In this section we introduce a selection of key terms, texts and authors connected with reasoning. Entries will be collected in a volume *Key Terms in Logic*, to be published by Continuum. If you would like to contribute, please [click here](#) for more information. If you have feedback concerning any of the items printed here, please email [thereasoner@kent.ac.uk](mailto:thereasoner@kent.ac.uk) with your comments.

#### Peirce, Charles Sanders (1839-1914)

An American philosopher who coined the term “pragmatism” and its associated maxim of ascertaining the meanings of concepts from the consequences of employing them. A pioneer in semiotics—the theory of signs and signification—Peirce also initiated the use of a number of basic methods in modern logic, including the use of truth tables and inclusive, instead of exclusive disjunction, the treatment of relations as classes of ordered pairs, ordered triplets, and by extension, ordered n-tuples.

Logic—particularly the logic of relations—forms the basis for Peirce’s work in semiotics, which adopts a triadic notion of signifier, signified, and (ideal) observer; it justified his ongoing search for metaphysical Categories, inspired by Kant; and its employment as a rigorous method in Duns Scotus and Ockham is a paradigm for Peirce’s own inquiries into the fixation of belief.

Keen to explicate the contrast between inductive and deductive forms, Peirce distinguished a third type of inference—“abduction” (or “retroduction”)—which by its very nature is not formalizable and is today often associated with inference to the best explanation.

Like Frege, Peirce held an essentially antipsychological view of logic, but also held that logical thought

could be normatively subsumed under an aesthetic category of what is ultimately “satisfactory.”

Kevin S. Decker

Eastern Washington University

#### Ontology

##### PHILOSOPHICAL ONTOLOGY

A branch of Western philosophy having its origins in ancient Greece in the work of philosophers such as Parmenides, Heraclitus, Plato, and Aristotle. Philosophical ontology is concerned with the study of what is, of the kinds and structures of objects, properties, events, processes, and relations in every area of reality. The term ‘ontology’ derives from the Greek ‘ontos’ (‘being’ or ‘what exists’) and ‘logos’ (‘rational account’ or ‘knowledge’). From the philosophical perspective, ‘ontology’ is synonymous with ‘metaphysics’ as classically conceived. This philosophical sense of the term is what Jacob Lorhard had in mind when he coined the term ‘ontology’ (ontologia) around 1613, and this is also why Bailey’s 1721 Oxford English Dictionary defined ontology as ‘an Account of being in the Abstract’.

##### DOMAIN ONTOLOGY

A representation of the things that exist within a particular domain of reality such as medicine, geography, ecology, or law, as opposed to philosophical ontology, which has all of reality as its subject matter. A domain ontology provides a controlled, structured vocabulary to annotate data in order to make it more easily searchable by human beings and processable by computers. The Gene Ontology Project is an example of a domain ontology that attempts to provide a taxonomy and controlled vocabulary for genes and gene products. Domain ontologies benefit from research in formal ontology, which assists in making communication between and among ontologies possible by providing a common language and common formal framework for reasoning.

##### FORMAL ONTOLOGY

A discipline which assists in making communication between and among domain ontologies possible by providing a common language and common formal framework for reasoning. This communication is accomplished by (at least) the adoption of a set of basic categories of objects, discerning what kinds of entities fall within each of these categories of objects, and determining what relationships hold within and amongst the different categories in the domain ontology. Formal ontology draws heavily from the logic and methodology of philosophical ontology. Through the work of thinkers such as Edmund Husserl, Roman Ingarden,

Barry Smith, and Patrick Hayes, formal ontology is increasingly being applied in bioinformatics, intelligence analysis, management science, and in other scientific fields, where it serves as a basis for the improvement of classification, information organization, and automatic reasoning.

Robert Arp

Biomedical Ontology, University at Buffalo

## §5 EVENTS

### DECEMBER

**NIPS:** Neural Information Processing Systems—Natural and Synthetic, Hyatt Regency Vancouver, 3–6 December.

**DESCARTES:** Philosophie naturelle, Philosophie de l'esprit, University of Provence Aix-Marseille 1, 6–9 December.

**LOGIC AND PHILOSOPHY OF SCIENCE:** Institut d'histoire et de Philosophie des Sciences et des Techniques, Paris, 7 December.

**WORKSHOP:** International Workshop on Applied Bayesian Statistics, EpiCentre, Massey University, Palmerston North, New Zealand, 10–14 December.

**ENQUIRY, EVIDENCE AND FACTS:** British Academy, London, 13–14 December.

**NORMS AND CONTENTS:** Geneva, 14 December.

**METAPHOR:** Third International Symposium of Cognition, Logic and Communication, Riga, Latvia, 16–18 December.

**WHAT IF? SO WHAT!:** Interdisciplinary approaches to counterfactual reasoning, Erasmus University of Rotterdam, The Netherlands, 17–20 December.

**SYMPOSIUM:** Reflections on Type Theory, Lambda Calculus and the Mind, Celebrating Henk Barendregt's 60th birthday, Radboud University Nijmegen, The Netherlands, 17 December.

**POWERS, CAUSATION AND LAWS:** St. John's College Durham, 17–18 December.

### JANUARY 2008

**ISAIM:** Tenth International Symposium on Artificial Intelligence and Mathematics, Fort Lauderdale, Florida, 2–4 January.

**3RD IMS AND ISBA MEETING:** The third joint international meeting of the IMS (Institute of Mathematical Statistics) and ISBA (International Society for Bayesian Analysis), Bormio, Italy, 9–11 January.

**PERSPECTIVES ON TRUTH:** University of Nottingham, 11–12 January.

**GRADUATE CONFERENCE:** 1st Cambridge Graduate Conference on the Philosophy of Logic and Mathematics, St. John's College, Cambridge, 19–20 January.

**BAYESIAN BIostatISTICS:** Houston, Texas, 30 January – 1 February.

### FEBRUARY

**FoIKS:** Foundations of Information and Knowledge Systems, Pisa, Italy, 11–15 February.

### MARCH

**RELATIVISM AND RATIONAL REFLECTION:** 10th Annual Pitt-CMU Graduate Student Philosophy Conference, University of Pittsburgh, 1 March.

**ARTIFICIAL GENERAL INTELLIGENCE:** The First Conference on Artificial General Intelligence, Memphis, Tennessee, 1–3 March.

**SCIENCE AND PSEUDOSCIENCE:** University of Birmingham, UK, 15 March.

**CONSTRAINT-SAC:** Track on Constraint Solving and Programming, at the 23rd Annual ACM Symposium on Applied Computing, Fortaleza, Brazil 16–20 March.

**CAUSATION: 1500-2000:** King's Manor, University of York, 25–27 March.

**UNCLOG:** International Workshop on Interval / Probabilistic Uncertainty and Non-Classical Logics, Ishikawa, Japan, 25–28 March.

### APRIL

**RELMICS10-AKA5:** 10th International Conference on Relational Methods in Computer Science & 5th International Conference on Applications of Kleene Algebra, Frauenwörth, Germany, 7–11 April.

**REDUCTION AND THE SPECIAL SCIENCES:** Tilburg Center for Logic and Philosophy of Science, 10–12 April.

**FLOPS:** Ninth International Symposium on Functional and Logic Programming, Ise, Japan, 14–16 April.

**WORKSHOP:** XVIII Inter-University Workshop on Philosophy and Cognitive Science, Madrid, [luis.fernandez@filos.ucm.es](mailto:luis.fernandez@filos.ucm.es), 22–24 April.

**PRACTICAL RATIONALITY:** Intentionality, Normativity and Reflexivity, University of Navarra, 23–25 April.

**SDM:** 8th Siam International Conference on Data Mining, Hyatt Regency Hotel, Atlanta, Georgia, USA, 24–26 April.

### MAY

**SBIES:** Seminar on Bayesian Inference in Econometrics and Statistics, University of Chicago Graduate School of Business Gleacher Center, 2–3 May.

**SIG16:** 3rd Biennial Meeting of the EARLI-Special Interest Group 16—Metacognition, Ioannina, Greece, 8–10 May.



**UR:** Special Track on Uncertain Reasoning, 21st International Florida Artificial Intelligence Research Society Conference (FLAIRS-21), Coconut Grove, Florida, 15–17 May.

**AI PLANNING AND SCHEDULING:** A Special Track at the 21st International FLAIRS Conference (FLAIRS 2008), Coconut Grove, Florida, 5–17 May.

**RSKT:** Rough Sets and Knowledge Technology, Chengdu, 17–19 May.

**ISMIS:** The Seventeenth International Symposium on Methodologies for Intelligent Systems, York University, Toronto, Canada, 20–23 May.

**COMMA:** Second International Conference on Computational Models of Argument Toulouse, France, 28–30 May.

**EXPRESSIONS OF ANALOGY:** Faculty of Social and Human Sciences, New University of Lisbon, 29–31 May.

## JUNE

**WCCI:** IEEE World Congress on Computational Intelligence, Hong Kong, 1–6 June.

**CSHPS:** Canadian Society for History and Philosophy of Science, University of British Columbia, Vancouver, 3–5 June.

**CE:** Computability in Europe 2008: Logic and Theory of Algorithms, University of Athens, Athens, 15–20 June.

**DM:** SIAM Conference on Discrete Mathematics, University of Vermont, Burlington, Vermont, 16–19 June.

**LOGICA:** Hejnice, Czech Republic, 16–20 June.

**IEA-AIE:** 21st International Conference on Industrial, Engineering and Other Applications of Applied Intelligent Systems, Wroclaw, Poland, 18–20 June.

**HOPOS:** Seventh Congress of the International Society for the History of Philosophy of Science, Vancouver, Canada, 18–21 June.

**EPISTEME:** Law and Evidence, Dartmouth College, 20–21 June.

**IPMU:** Information Processing and Management of Uncertainty in Knowledge-Based Systems, Malaga, Spain, 22–27 June.

## JULY

**LOFT:** 8th Conference on Logic and the Foundations of Game and Decision Theory, 3–5 July.

**ICML:** International Conference on Machine Learning, Helsinki, 5–9 July.

**UAI:** Uncertainty in Artificial Intelligence, Helsinki, 9–12 July.

**COLT:** Conference on Learning Theory, Helsinki, 9–12 July.

**DEON:** Ninth International Conference on Deontic Logic in Computer Science, Luxembourg, 15–18 July.

**ISBA:** 9th World Meeting, International Society for Bayesian Analysis, Hamilton Island, Australia, 21–25 July.

**FIRST FORMAL EPISTEMOLOGY FESTIVAL:** Conditionals and Ranking Functions, Konstanz, 28–30 July.

## AUGUST

**CONFERENCE:** Language, Communication and Cognition, University of Brighton, 4–7 August, Brighton, UK.

**ESSLLI:** European Summer School in Logic, Language and Information, Freie und Hansestadt Hamburg, Germany, 5–15 August.

**IJCAR:** The 4th International Joint Conference on Automated Reasoning, 10–15 August.

**ICT:** The Sixth International Conference on Thinking, San Servolo, Venice, 21–23 August.

**COMPSTAT:** International Conference on Computational Statistics, Porto, Portugal, 24–29 August.

## SEPTEMBER

**10TH ASIAN LOGIC CONFERENCE:** Kobe University, Kobe, Japan, 1–6 September.

**SOFT METHODS FOR PROBABILITY AND STATISTICS:** 4th International Conference, Toulouse, France, 8–10 September.

## §6

## JOBS

**TENURE TRACK POSITION IN PHILOSOPHY:** Oglethorpe University, Atlanta, deadline 1 December 2007.

**DUKE STATISTICS:** Associate or Assistant Professor, deadline 1 December 2007.

**PLANNING POSTDOC:** PostDoc grant in Decentralized Planning under Uncertainty, Instituto Superior Tecnico, Lisbon, contact [Matthijs Spaan](#), deadline 5 December 2007.

**LEEDS PHILOSOPHY:** 4 lectureships, enquiries to [Steven French](#), deadline 7 December 2007.

**CANTERBURY STATISTICS:** Department of Mathematics and Statistics, University of Canterbury, New Zealand, deadline 7 December 2007.

**TiLPS:** three- to nine-months visiting fellowships, Tilburg Center for Logic and Philosophy of Science, enquiries to [Stephan Hartmann](#), deadline 15 December 2007.

**SOUTHAMPTON STATISTICS:** Research Fellow in Statistics, Southampton Statistical Sciences Research Institute, deadline 17 December 2007.

**CAMBRIDGE MACHINE LEARNING:** Two Postdocs, deadline 15 January 2008.

## COURSES AND STUDENTSHIPS

### Courses

**SECOND INDIAN WINTER SCHOOL ON LOGIC:** IIT Kanpur, 14–26 January 2008.

#### MA IN REASONING

An interdisciplinary programme at the University of Kent, Canterbury, UK. Core modules on logical, causal, probabilistic, scientific and mathematical reasoning and further modules from Philosophy, Psychology, Computing, Statistics and Law.

**MLSS:** 10th Machine Learning Summer School, Kiloa Coastal Campus, Australian National University, 3–14 March 2008.

**ESSLLI:** European Summer School in Logic, Language and Information, Hamburg, 4–15 August 2008.

### Studentships

**LOGIC AND PHILOSOPHY OF SCIENCE:** 4-year PhD position or a 80%-funded 4 year post-doctoral research position, The Center for Logic and Philosophy of Science at the Vrije Universiteit Brussel, [sonsmets@vub.ac.be](mailto:sonsmets@vub.ac.be), deadline 1 December 2007.

**COMPUTATIONAL NEUROSCIENCE:** 4-year PhD studentships, Gatsby Computational Neuroscience Unit, University College London, deadline 6 January 2008.

**ARCHÉ POSTGRADUATE STUDENTSHIPS:** The Arché Research Centre at the University of St Andrews is offering up to six three-year PhD studentships for uptake from September 2008, deadline 1 February 2008.

### Acknowledgements

*The Reasoner* is a development of the [progicnet](#) academic network supported by the [Leverhulme Trust](#).