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EDITORIAL

Many thanks to the editors of *The Reasoner* for giving me the opportunity to guest edit this issue. On the occasion of the recent launch of the [Finnish Centre of Excellence in](#)

the [Philosophy of the Social Sciences](#) (of which I am also a part) based here in Helsinki, I have interviewed three of my colleagues: [Raul Hakli](#), [Uskali Mäki](#) and [Petri Ylikoski](#).

Research at the Centre deals with

a variety of issues concerning for example models and simulations, collective intentionality, explanation and evidence, and marketization (details about these and the other themes that constitute the Centre's research agenda are presented in the interview below). A key thread that unifies the various research themes is interdisciplinarity in the social sciences and between them and the natural sciences. Interestingly, only a few issues back [Federica Russo](#) discussed interdisciplinarity together with [Robert Frodeman](#), Director of the [Center for the Study of Interdisciplinarity](#) (*The Reasoner*, 6(2)). In her editorial, Federica asked what interdisciplinarity is and what counts as interdisciplinary research; questions that are of practical relevance given the increasing weight that funding bodies place on interdisciplinary projects.



Caterina Marchionni

Here I take up a related but different aspect of interdisciplinarity: its status as an object of study for the philosophy of (social) science. In fact interdisciplinarity has characterized the sciences for quite some time but until recently philosophical interest in it has been relatively sporadic. So it seems just natural to ask why philosophers of science are *now* becoming more interested in it. Is it just to fill a gap that could or should have been filled earlier? Or is this interest a side product of the pressure placed on interdisciplinary research by funding bodies? Or instead, is there something genuinely novel in how interdisciplinary interactions shape the (social) sciences today? I am inclined to think that all these factors (and possibly others) contribute to motivate philosophers to study interdisciplinarity. Still, it is far from obvious which aspects of interdisciplinarity are philosophically relevant and how they are to be tackled. In other words, what does a philosophy of interdisciplinarity look like?

I can envisage two possibilities. The first is that interdisciplinarity per se does not pose any novel philosophical problem and philosophers of science can simply apply their traditional toolkit (for example concerning evidence, explanation, the unity of science) to analyze current episodes of interdisciplinary exchange. The second possibility is that today's interdisciplinary interactions involve significantly new challenges for the (social) sciences so that their philosophical analysis calls for novel resources, or at least for a suitable adjustment of existing ones. Below I pose this question to my interviewees and I believe their answers provide interesting material to reflect on the various ways in which philosophy of science can contribute to the study of interdisciplinarity.

Regardless of one's stance on the status of interdisciplinarity as a philosophical theme, there seems to be little doubt that the changes taking place in the social sciences are both the consequences and the sources of more frequent interactions across disciplinary boundaries. It is therefore a welcome development that many philosophers are ready to explore this territory. All the more so, because, as the interviewees note below, in times of change scientists are more likely to get engaged by philosophical discussions. And there might be a greater chance for us to contribute to social scientific

practice in a more concrete fashion.

CATERINA MARCHIONNI
Philosophy, University of Helsinki

FEATURES

Interview with Raul Hakli, Uskali Mäki and Petri Ylikoski

Caterina Marchionni: Welcome to [Raul Hakli](#), [Uskali Mäki](#) and [Petri Ylikoski](#) and thank you for having agreed to this interview. To begin, why don't you introduce yourself by briefly describing your academic background?

Raul Hakli: I did my master's degree in computer science and was involved in all kinds of fancy stuff like bioinformatics and artificial intelligence, but then I happened to take a few philosophy courses which totally messed up my mind, so I ended up with this really smart career move and did my PhD in philosophy instead. But I have been lucky so far in finding work at the university.

Petri Ylikoski: I'm a philosopher of science, educated mostly at the University of Helsinki, who has been working mostly on theory of explanation, philosophy of biology and philosophy of the social sciences. I have also had a long interest in sciences studies and consider myself a philosopher/sociologist hybrid—annoying people by playing a sociologist to philosophers and a philosopher for sociologists.



Raul Hakli

Uskali Mäki: I was trained in both philosophy and economics, mainly in Helsinki. I was sufficiently puzzled by the economics lectures and textbooks to decide to become a philosopher of economics before the field existed. I've visited several universities in North America and Europe, of which I spent eleven years (1995-2006) at Erasmus University in Rotterdam. My work proceeds within a broadly scientific realist conception of science, but I've also argued for the necessity to revise conventional versions of this conception in order to accommodate the diversity of science as well as some apparently antirealist insights. Among other things, I've developed an account of models and defended unrealisticness in models and their assumptions; a realist conception of the rhetoric of inquiry and social construction; and of explanatory unification. In the recent years I've focused on what I and others have called the philosophy of interdisciplinarity, including some work on economics imperialism as an exemplification.

CM: The Academy of Finland has awarded your (our) group the status of *Finnish Centre of Excellence in the Philosophy of the Social Sciences* to carry out a six-year research project on interdisciplinarity, cognitive tools, and the future of social science. What does this entail in practice?

UM: Having this status gives us some nice privileges, such as more prestige and more resources. This time the Academy of Finland

decided to reduce the number of these centres (which made it more difficult to win the status) in order to allocate more resources to each. Indeed, we will now be able to solidify and expand our activities on a longer term basis. As you know, the Centre of Excellence is an expansion of **TINT** (Trends and Tensions in Intellectual Integration) that we started in 2006. The group is growing, and is now between 20 and 30 people. Foreign visitors at different stages of their careers play an important role.



Uskali Mäki

The motivating observation behind the research is that the future of the (social) sciences is shaped by many sorts of interdisciplinary dynamics—from borrowing and collaborating to conquering and dismissing, and so on. We are looking into two classes of these dynamics: among the social sciences (such as when the models and methods of economics are increasingly applied in sociology and political science); and between the social sciences and other disciplines (such as experimental psychology, cognitive neuroscience, and evolutionary biology).

The research has two overall goals: to develop further ingredients for a philosophy of interdisciplinarity (we started working on this in 2006 on a more collective and systematic basis); and to develop philosophical analyses that would be relevant to practitioners in (social) scientific research and communication. The research will proceed through empirical case studies as well as consultations and collaborations with practitioners in our target disciplines.

CM: What do you see as the distinguishing features of our research group?

PY: We are all philosophers, but each of us has combined philosophy with quite different things, so we are in a way realizing Donald Campbell's fish-scale model of omniscience, although in quite small scale. Our philosophical attitude is probably one of our distinguishing features: we do not see any reason to draw a sharp line between philosophical and scientific questions. So we do not need to emphasize our philosophical credentials by focusing on traditional "pure" philosophical problems, however neither do we think that substantial sciences will solve all the philosophical problems. We are quite confident that you can find lively philosophical issues by focusing on contemporary issues in the social sciences. We are not anti-metaphysical, just against sterile ways to do metaphysics.

CM: Research at the Centre is divided into five interrelated themes: 1) the interdisciplinary transfer of models and other vehicles of surrogate reasoning; 2) economics and its interdisciplinary relations; 3) social ontology and collective intentionality; 4) explanation and evidence; 5) analytical sociology. Could each of you give us some information about the theme(s) in which you are principally involved?

UM: Note that the five themes overlap and will be pursued in interaction with one another. Many of us contribute to more than two themes. This is important for the exploitation of the potential synergies and the cohesion of the whole picture.

One of the themes has to do with models and other vehicles of surrogate reasoning and their transfer across disciplinary boundaries. This follows up on what we have done previously on models and simulations in various disciplines. Social sciences are

increasingly adopting new techniques of surrogate reasoning, but these are variously embedded in their respective disciplinary cultures and theoretical frameworks. So we look into these differences and possible convergences towards similar toolboxes and practices.

Another large theme has to do with economics and its interdisciplinary relations, both outward and inward. The former is exemplified by the so-called economics imperialism, expansion of economics to the domains of other disciplines—such as the increasing adoption of the market metaphor in disciplines other than economics. The equally controversial flow of influences on economics from experimental psychology and cognitive neurobiology (as in behavioural economics and neuroeconomics) are prime examples of the latter. We examine the structure, presuppositions, and consequences of these interactions.

RH: I'm part of Raimo Tuomela's team that studies social ontology and collective intentionality. We have a modest little aim of developing a general theory of human sociality that will account for everything from two people carrying a table upstairs to the complex social institutions of modern societies.

PY: Two of the key systematic themes will be explanation and evidence. Many of us have done a lot of work on explanation, and that is beginning to bear fruit: we are going to present our distinctive approach in a more systematic way and attempt to present its fruitfulness via series of case studies. The theme of evidence is more recent, and our distinctive approach to it is still developing. The interest in evidence is quite natural for us. We have been focusing on relations between scientific fields, and apart from explanation, the issues of evidence are crucial there. Similarly, the recent debates have shown that explanatory reasoning has an important role in the evaluation of evidence, which makes it natural for us to bring these two things more closely together. As said, we do a lot of our work via case studies. Here we have two strategic interests. First, we are interested in relations between biological and social sciences. By biological sciences we are referring to neurosciences, evolutionary biology, and genetics. The relations of all these fields to various social sciences are developing rapidly and they provide a lot of material for a philosophical observer. Not only that, scientists in the middle of these exchanges are very eager to hear what philosophers have to contribute. Of course we cannot focus on all of these developments systematically, we have to choose cases. Currently we are working on the domestication of neuroscience in addiction research, neuroeconomics, evolutionary theories of origins of morality and systems biology. Of course, we are scouting for interesting cases all the time. Our second strategic interest is in changes in the social sciences, especially in two big ones: sociology and economics. We are interested in how they change, or resist change. Especially we are interested in how they adopt and adapt methodological ideas like: agent-based simulation, network analysis, evolutionary game theory, experimental research, mechanism-based explanation, and how they relate themselves to the biological and cognitive sciences.



Petri Ylikoski

CM: Is there any topic in philosophy of science that you are *not* planning to do some

work on?

PY: Plenty. The appearance that we are working on almost everything is an optical illusion. There is a logic behind our choice of cases.

CM: So what's the logic?

PY: Here are some principles behind choices, in no particular order. 1) We focus on open-ended contemporary science, not historical cases that have been closed ages ago. This allows richer observation of the social aspects of scientific debate. 2) We choose cases that look promising from the point of view of philosophical intervention. This derives from our conviction that the ultimate test of philosophical ideas in science is their ability to improve scientific practices (judged by the scientists) rather than saving some philosophical intuitions (that could be just artifacts of philosophical training). 3) To be able to contribute, the case and issues have to have some kind of connection to our earlier work. This earlier work provides a bridgehead that allows fuller exploration of the case. The connection to earlier work is often comparative: we would compare the new case to one we are already familiar with and look for interesting differences between these cases of interfield exchange. 4) There is an ultimate aim of contributing to a bigger picture. Wilfrid Sellars once defined philosophy as follows: "The aim of philosophy, abstractly formulated, is to understand how things in the broadest possible sense of the term hang together in the broadest possible sense of the term." This will do as a general definition of philosophy, but if you replace "things" with "knowledge about humans and societies" you get pretty good idea of our view of the task of the philosophy of the social science.

CM: One of the key threads unifying the research themes is interdisciplinarity. Which specific philosophical problems does interdisciplinarity raise? And are the interdisciplinary interactions taking place now in the social sciences of a new kind?

UM: Interdisciplinarity involves many of the big issues in the philosophy of science rather directly. They deal with the similarities and differences between types of scientific discipline as well as issues of unity and disunity, integration and disintegration, pluralism and perspectivism, discovery and justification, evidence and epistemic virtues, explanation and causation, change and progress, simplicity and complexity, analogy and metaphor, commensurability and incommensurability, and much more. Among the key concepts we use for addressing these issues are those of model, explanation, evidence, and sociality.

The interdisciplinary interactions now shaping the social sciences are new, they take place right now and are dependent on the resources and constraints presently available. Naturally, not everything is equally new. Economics imperialism has been going on for some decades now, while the impact of neuroscience on the social sciences is a matter of the last decade or a little longer—not to forget though that there have been previous attempts of the same sort to ground social science on psychology and biology.

CM: Let me challenge you a little on this. Haven't interdisciplinary interactions always characterized the social sciences? Isn't what is now going on perhaps just a difference of scale rather than kind? Furthermore, since the key concepts you just mentioned are pretty much part of the standard toolkit of philosophy of science, couldn't this be interpreted as implying that interdisciplinarity does not pose any novel philosophical problems?

PY: I agree, however this does not imply that interfield exchanges lack philosophical interest. Interfield interaction provides for us what Robert K. Merton (1987: “Three Fragments From a Sociologist’s Notebooks: Establishing the Phenomenon, Specified Ignorance, and Strategic Research Materials” *Annual Review of Sociology* 13: 1–28) once called strategic research material—“strategic research sites, objects, or events that exhibit the phenomena to be explained or interpreted to such advantage and in such accessible form that they enable the fruitful investigation of previously stubborn problems and the discovery of new problems for further inquiry”. Two considerations are especially important. First, practicing scientists often shun conceptual and philosophical issues underlying their research and as a consequence of this often treat people who are interested in these issues—philosophers—as an annoyance. In interdisciplinary settings they are facing the limits of their tacit assumptions and they are forced to articulate and rethink them. These articulations and revisions provide interesting material for philosophers of science. In addition, the scientists are much more eager to hear what philosophers have to say, thus creating an opportunity for a constructive philosophical intervention. The second important point is that if we take “the Sellarsian agenda” mentioned above seriously, philosophers of science should pay special attention to seams that bind different pieces of scientific knowledge together. Given that the old philosophical fantasy of grand unified physical theory of everything is not humanly accessible, both the holes and the overlaps in the blanket of knowledge should be of philosophical interest.

UM: Indeed, interdisciplinary situations are particularly fitting for addressing philosophical issues, since these issues tend to be so transparent in those circumstances. But I believe there is more that justifies the very idea of the philosophy of interdisciplinarity. In general, I am in favour of an institutionalist philosophy of science, one that incorporates elaborate accounts of the institutional structure and dynamics of sciences. Disciplines are institutions, and interdisciplinary interactions take place within complex institutional frameworks. This is part of the reason why there tend to be chronic issues and recurrent patterns in interdisciplinary dynamics. Their distinctiveness requires some new elements of institutionalization also in the philosophy of science. Something like this would be part of my response to your second query. As to your first question, I’d say interdisciplinarity is as old as disciplinarity, but also that there are variations, even cycles, in interdisciplinary openness and the intensity of interactions, whether actual or just wanted. Due to both internal and external pressures, the intensity has grown recently, and not only in the social sciences. These are particularly exciting times for a philosopher of interdisciplinarity!

CM: Could you single out and explain in some detail one or two lines of research in the Centre’s agenda that you find particularly exciting?

UM: One of the themes that raised excitement also among our reviewers is marketization. We’ve had an interdisciplinary workshop running on this for a couple of years and this will continue. So we are talking about marketization as a mega trend in contemporary society, including the extension of the concept of the market across disciplinary boundaries as well as the commercialization of the conditions of scientific work. So there are at least three realms of marketization that are relevant to the philosophy of science and of interdisciplinarity. First, what to make of the traditional ideals

of scientific inquiry in the new commercialized circumstances that shape disciplines differently? How do Mertonian or other such principles cope with marketization in science? Second, how to compare and appraise, perhaps combine, the various disciplinary perspectives to real-world marketization? The challenge is not simple given that some disciplines, such as most of economics, considers the market as a most natural institution, while some others view ongoing marketization as a trend towards a perversion of community values or some such undesirable outcome. Third, the concept and models of the market increasingly travel from economics to other disciplines, so we now have models of political markets and of biological markets, and much more. Confusion and controversy easily arise. What exactly is being claimed in these disciplines, and how are those claims to be justified? You may see that taking marketization as a multifarious target for philosophy of science creates a tangible feeling of relevance!

RH: I currently find the question of group agency very exciting even though it's not a new one really. I mean that the social sciences have always struggled with trying to understand the complex relationship between individual and collective. But it seems to me that we are finally getting closer to understanding the different levels of intentionality in human societies. It is sometimes said that all social phenomena are a result of the actions of individuals who are trying to satisfy their preferences. In a sense it may be true, but it is a huge simplification because we all belong to various groups like families, companies, nations and organizations, and a lot of time we're not thinking what it is that I want but what my company wants or what my sports club plans to do or what my family needs. That is, a lot of time we are not thinking in terms of our wants or preferences but the wants and preferences of a group that we belong to. These group preferences affect our actions, and it is sometimes useful to think of society as consisting of group agents as well as individual agents. One could, of course, say that what happens in these cases is that we consider all these different groups that we belong in, weigh their objectives and adjust our preferences accordingly, and then we make an informed decision based on all the relevant considerations. I'm not sure if this is always the case, or at least I think we should consider alternative hypotheses as well.

I think in many cases what happens in such deliberation is that we end up focusing on one particular group and put the other considerations aside. I may decide to be a good family member or a good club member or a good worker, and then what I end up doing is what I think best satisfies the preferences of the chosen group. When I select the group, I act as an individual agent, but after that, I act as a part of group agent. The latter kind of action has been taken seriously in the collective intentionality literature and I find it pretty fascinating because it seems to be a huge departure from traditional individualistic thinking. But then if we start to talk in terms of group agents, group attitudes and group preferences, we need to engage in a philosophical project and try to understand what these things are, how they are formed, how they function and whether individual agents are still somehow primary.

CM: Raul, this sounds extremely interesting and I can see the contribution that philosophy can make here. And yet, I cannot help thinking of the suggestion made by Francesco Guala (2007: "The philosophy of social science: metaphysical and empirical." *Philosophy Compass* 2/6: 954–980) that it is about time that philosophers start developing an empirical approach to theories of collective intentionality. What is your

take on this? Can and should questions such as those about group agency and group attitudes be settled empirically?

RH: I think some of them can, yes. Some philosophical theories make different predictions about observable behaviour, and in such cases empirical research can be used to rule out some of the alternative hypotheses. Of course, it is not straightforward, and experimental methodology has its own problems. But I think philosophers should pay attention to empirical research that is already being done in this field and maybe sometimes even participate in it for example by offering constructive criticism of the design of the experiments and the interpretation of the results.

CM: Among the Centre's aspirations is to produce research that is relevant to social science and to intervene directly in its debates. This is easier said than done, however. A mismatch between the interests of scientists and those of philosophers of science is a real possibility. How do you plan to avoid this mismatch and successfully engage scientists?

UM: I'd say we are in a fortunate position in having chosen our research focus in such a way that the challenge of practical relevance might be easier to meet than in many other kinds of situation. I mean our focus on change in social science through interdisciplinary interactions. The practitioners in our target disciplines are not doing merely well-disciplined normal science, but trying out some new avenues. This often creates feelings of uncertainty and may prompt resistance and controversy. In such situations practitioners are far more willing to listen to those offering philosophical reflection on the issues they struggle with.

PY: I think that an ability to contribute to the scientific debates is one of the most important validity criteria for philosophy of science. Of course, that does not imply that all philosophy of science could make such contribution, and it might well be that certain approaches to philosophy of science could be highly unlikely to make such a contribution. The key challenge is that two audiences require different kind of communication. It would be foolish to think that a paper directed towards philosophical colleagues would automatically be the best instrument for communicating with practicing social scientists. The communication has to be adapted to local jargon and concerns of the field in question. Being able to do this requires a lot of effort—the dialects of various scientific tribes are quite diverse. However, if you do your homework and build your credibility, scientists can be very receptive—especially when you address them on the right issues and attempt to be constructive.

RH: Well, at least in our team we have thought that individualistic thinking prevails in some corners of the social sciences, and that social scientific explanation and modelling of social phenomena is often done individualistically. If we manage to develop tools that make the complex relationships between groups and individuals a bit more transparent, social scientists might find alternative ways of thinking about a lot of issues. For instance, many economists are working with game-theoretical tools, and game theory is individualistic in the sense that the agents are all alike: They have their degrees of belief and their preferences, and they are usually thought to be individuals although they could as well represent firms or nations, for instance. It is not possible to have both individuals and groups in the same model so that individuals would have their preferences but could also act on the basis of the preferences of their group. No

doubt once these group-based ideas get fully developed, all social scientists working with game theory will want to switch to group game theory instead.

CM: How has the Finnish philosophical tradition influenced your own philosophical approach?

UM: The generation that followed Jaakko Hintikka includes Ilkka Niiniluoto and Raimo Tuomela. I am in the generation thereafter, and indeed have been influenced by those two, especially regarding my interest in scientific realism—yet my current understanding of scientific realism is an outcome of a long journey away from what they taught me. But surely I have kept my interest in truth (as has Niiniluoto) and social ontology (as has Tuomela).

RH: Personally, I have inherited an interest in logic, reasoning and philosophy of science which have traditionally been popular here. But even more important than the actual topics is the example that Finnish philosophers like Georg Henrik von Wright and Jaakko Hintikka have shown by going abroad, participating in discussions and getting their ideas known. I mean that you will have to do good research, of course, but that is not enough, you will have to go out and let other people know about your ideas and try to learn from what they are doing. Well, I haven't done it enough myself I guess, but I think it is something to aim at.

PY: I cannot see much continuity between previous generations and myself in terms of specific ideas or philosophical methodology. However, I think I have benefitted greatly from a general atmosphere in which philosophy of science is regarded as having a central role in philosophy. Of course, I have myself adopted this idea: most of modern philosophy should be done with scientific materials and this gives philosophy of science a central role in the discipline.

CM: Do you foresee the possibility of developing a common characteristic approach to the philosophy of social sciences? Maybe an approach such that years from now will have its own brand name...

RH: Yes, this is an excellent idea. Thank you, Cate, for bringing it up! In my experience so far, the hardest part in the activities within this group has been agreeing on names: As soon as we find a good enough name for the approach so that everyone can accept it, I'm sure that coming up with the actual method will be a piece of cake!

PY: Well, we are not planning to develop such an approach, in other words, we are not preparing any kind of 'Helsinki Manifesto' for a school of thought. However, it is a plausible sociological hypothesis that if a group of people work on shared themes and interact intensively for a long time, some kind of characteristic features will emerge that will make the group members identifiable. Of course, those characteristics can only be identified retrospectively.

UM: If having a school were to require a great deal of uniformity of substantive philosophical convictions, I might not want a school at all. Some diversity is important for dynamism and rigour, and it is a source of more fun too. But we already share a lot in the themes of work and the styles of working—and, well, enough also in substantive views—so that on a permissive enough reading of “common approach” we might already have it!

A purely epistemological version of Fitch's Paradox

The *knowability thesis* is the idea that every truth is knowable at least in principle: $\phi \rightarrow \diamond K(\phi)$ (I will always implicitly quantify ϕ and ψ over the set of all formulas, except within deductions, where they stand for fixed formulas). Fitch's Paradox is the fact that, along with other basic epistemic assumptions, the knowability thesis implies the *omniscience principle*, $\phi \rightarrow K(\phi)$. Though the knowability thesis seems reasonable, the omniscience principle is absurd. This seems a devastating blow against anti-realism. As for the "other assumptions," there is infinite variation, to the point that almost all the major papers employ slightly different assumptions. Still, the Church-Fitch argument does not fundamentally change, and can always be glossed as follows (*):

1. Formally verify (using the "other assumptions") the absurdity of Moore's Paradox (usually because $K(\phi \wedge \neg K\phi)$ implies $K(\phi)$ and $\neg K(\phi)$).
2. Conclude $\neg \diamond K(\phi \wedge \neg K\phi)$.
3. If $\phi \wedge \neg K\phi$ is true, then by the knowability thesis, $\diamond K(\phi \wedge \neg K\phi)$.
4. Therefore, $\phi \wedge \neg K\phi$ can not be true. So $\phi \rightarrow K(\phi)$.

By the *weak omniscience principle* I mean the schema $\phi \rightarrow K(K(\phi))$, and by the *purely epistemic knowability thesis* I mean the schema $\phi \rightarrow \neg K(\neg K(\phi))$. The latter implies the former by the standard Fitch's Paradox, given the usual other assumptions. My aim is to show that the implication holds given a more barren set of other assumptions, by an argument which is qualitatively different than (*). The plausibility of purely epistemic knowability will be discussed below.

We make the following assumptions:

- \wedge : $K(\phi \wedge \psi) \rightarrow K(\phi) \wedge K(\psi)$.
- Purely Epistemic Knowability (PEK): $\phi \rightarrow \neg K(\neg K(\phi))$.
- Rule of Necessitation: From a deduction of ϕ , we may deduce $K(\phi)$.

From these we deduce weak omniscience as follows.

1. Assume $K(\phi \wedge \neg K(K(\phi)))$.
2. By \wedge , $K(\phi)$ and $K(\neg K(K(\phi)))$.
3. By PEK applied to $K(\phi)$, we have $\neg K(\neg K(K(\phi)))$.
4. Contradiction. Discharge 1 and conclude $\neg K(\phi \wedge \neg K(K(\phi)))$.
5. By Rule of Necessitation, conclude $K(\neg K(\phi \wedge \neg K(K(\phi))))$.
6. Assuming $\phi \wedge \neg K(K(\phi))$, we would have $\neg K(\neg K(\phi \wedge \neg K(K(\phi))))$ by KEP. That would contradict 5, so $\neg(\phi \wedge \neg K(K(\phi)))$, or equivalently, $\phi \rightarrow K(K(\phi))$.

This argument is qualitatively different for four reasons. First, it factors through a *weak* Moore’s paradox: “It’s raining, and I don’t know that I know it’s raining.” Second, the Moore contradiction is not obtained by stripping away modal operators (which seems impossible without additional assumptions) but rather by piling new modal operators on! Third, it never directly uses any consistency assumption on K , neither $K(\phi) \rightarrow \phi$ nor even the weaker $\neg(K(\phi) \wedge K(\neg\phi))$. Finally, it makes no use of modalities of possibility or necessity.

If we assume that all necessities are known (contrapositively, all unknowns are unnecessary) then it follows that PEK is stronger than the usual knowability thesis:

$$\neg K(\neg K(\phi)) \rightarrow \neg \Box(\neg K(\phi)) \text{ and } \neg \Box(\neg K(\phi)) \rightarrow \Diamond(K(\phi)).$$

In a sense, PEK is the polar opposite of the *negative introspection axiom* (sometimes called **5**), which says $\neg K(\phi) \rightarrow K(\neg K(\phi))$. Is an assumption like PEK really plausible? Not in the contexts where Fitch’s Paradox is normally discussed (human knowledge), so this note is at best an interesting curiosity in the bigger picture of Fitch’s Paradox. But Purely Epistemic Knowability is somewhat plausible in the area of machine knowledge. A machine can be programmed to mechanically “know” formulas in a crude epistemic language (too weak, say, for Kurt Gödel’s incompleteness theorems), and its knowledge can be closed under modus ponens and can include various epistemic axioms and rules, and furthermore it can extend to include contingent facts such as “the fifth user-input is a 0”. We speak, as always, of idealized knowledge: the machine cannot reason that “I do not know that the fifth user-input is 0 because I haven’t received five inputs yet”: it has no way of talking about how many inputs it has received (the machine may be what socket programmers call *blocking*: if its programming instructs it to query the n th input before that input is received, the machine freezes until receiving the input). Neither can the machine deduce later inputs based solely on earlier ones, assuming the user has free will. The only way for the machine to conclude “I do not know that the fifth user-input is 0” is for the machine to observe, say, that the fifth user-input is 1. But this is all just a very drawn out way of articulating the Purely Epistemic Knowability thesis.

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Two Problems for the Contextual Theory of Scientific Understanding

In this paper I argue that the contextual theory of scientific understanding as developed by Henk De Regt & Dennis Dieks (2005: “A Contextual Approach to Scientific Understanding”, *Synthese* 144, 137–170) and further developed by Henk De Regt (2009: “The Epistemic Value of Understanding”, *Philosophy of Science* 76: 585–597) is not contextual in one crucial respect: the meaning of understanding. I also argue that its scope needs to be restricted: it cannot be a theory of scientific understanding in general.

De Regt & Dieks first develop an argument for the assumption that achieving understanding is one of the epistemic aims of science (p. 139–143) and then investigate what scientific understanding is. Two central tenets of the theory as developed in the



2005 paper are CUP (Criterion for Understanding Phenomena) and CIT (Criterion for the Intelligibility of Theories):

CUP: A phenomenon P can be understood if a theory T of P exists that is intelligible (and meets the usual logical, methodological and empirical requirement). (p. 150)

CIT: A scientific theory is intelligible for scientists (in context C) if they can recognize qualitatively characteristic consequences of T without performing exact calculations. (p. 151)

De Regt & Dieks illustrate CUP and CIT by means of the explanation of Boyle's law by the kinetic theory of gases. They use the qualitative analysis which can be found in the introductory sections of Ludwig Boltzmann's *Lectures on Gas Theory* to make their case. They conclude as follows:

Together these conclusions lead to a qualitative expression of Boyle's ideal gas law. It is important to note that the above reasoning does not involve any calculations. It is based on general characteristics of the theoretical description of the gas. Its purpose is to give us *understanding* of the phe-

nomena, *before* we embark in detailed calculations. (pp. 152–153; italics in original)

They do not deny that exact calculations are important. But these are not the only thing that matter in science:

What we emphasise is the importance of understanding as an *additional* epistemic aim of science. (p. 153)

Let us now investigate to what extent the theory is contextual. De Regt & Dieks claim that the means which scientists use to achieve understanding vary:

There is no universal tool for understanding, but a variety of ‘toolkits’, containing particular tools for particular situations. (p. 158)

They also claim that achieving understanding is context-dependent in the sense that it depends on the capacities of the scientist, not only on the quality of the theory (p. 151). On the other hand, they also claim that. . .

Our approach retains a general, non-trivial specification of what it means to possess scientific understanding of a phenomenon. (p. 165)

Summarising, what De Regt & Dieks claim is (a) that understanding as an aim of science has a *context-independent* meaning, viz., qualitative derivations, (b) that *success* in achieving understanding is context-dependent, and (c) that the *tools* scientists use to arrive at understanding vary. Clauses (b) and (c) make the theory partially contextual, clause (a) makes it partially non-contextual. They are contextualists about the *tools* for understanding and about the *success* of understanding, but not about its *meaning*. The aim of their paper is to present an analysis of the nature of scientific understanding and of how explanations can lead to understanding (p. 137 and p. 165). They present a *non-contextual* theory of the nature of scientific understanding and a *contextual* theory about how and when it is achieved.

In the more recent 2009 paper, we find a similar view:

Whether theory *T* is intelligible depends not only on the virtues of *T* itself but also on such contextual factors as the capacities, background knowledge and background beliefs of the scientists in *C*. Accordingly, CIT can accommodate the variety of ways in which understanding is achieved in scientific practice. Qualitative insight into the consequences of a theory can be gained in many ways [...] (p. 595)

Again, we have contextual variation in tools and success, but there is only one goal: qualitative insight into the consequences of a theory. This makes the theory of de Regt & Dieks less contextual than e.g., van Fraassen’s account (1980: *The Scientific Image*. Oxford: Clarendon Press). Van Fraassen claims that there are different goals (p. 156).

The second point I want to make here is that the scope of the theory has to be restricted. The non-trivial specification referred to in the quote above can be represented as follows:

(QD) In all possible contexts understanding as an epistemic aim of science consists in the capacity to make qualitative derivations with a theory.

This formulation is a “contraction” of CUP and CIT in which the “middle term” (intelligibility) is removed. Contrast this with a more moderate claim:

(QD*) In all possible contexts where scientist try to understand a phenomenon by means of a theory, understanding as an epistemic aim of science consists in the capacity to make qualitative derivations with the theory.

In this more moderate claim, the main idea of the theory (qualitative derivations) is maintained but confined to cases where scientific theories are used as tool. Without this restriction on the scope the theory of De Regt & Dieks is quite trivially false. Suppose that I explain why one pendulum has a longer period than another one by deriving it from a difference in length and the pendulum law. No exact calculations are required here, the derivation gives qualitative insight into the consequences of the pendulum law. Nevertheless, this does not count as understanding according to (QD) because no theory is used in the explanans. I propose to confine the scope of the theory by means of a partial specification of the content of the explanans, as is done in (QD*). The alternative is to assume that understanding always requires the application of a scientific theory. That would rule out many cases that we (a) we intuitively qualify as understanding and (b) involve qualitative derivations. It is not clear which direction de Regt & Dieks want to go. CUP is not a biconditional; this suggests that they want to leave room for other possibilities, i.e., agree with (QD*). However, their paper also contains stronger claims, such as the claim that they retain a *general* specification on scientific understanding (cfr. the quote above). If they really think they have found something general, they adhere to (QD) and CUP must be reformulated as a biconditional.

Summarising, the so-called contextual theory of understanding faces two problems: it is not contextual with respect to the meaning of understanding and its scope has to be restricted to cases where theories are used.

ERIK WEBER

Centre for Logic and Philosophy of Science,
Ghent University

NEWS

Natural Information, 13 February

Many events or states seem to carry information about the occurrence of some other events or states (e.g., fingerprints and ringing doorbells). Natural information of this kind is often analysed in terms of the work by Paul Grice and Fred Dretske. But various difficulties with these standard approaches remain unresolved. This workshop at University of Aberdeen explored new work on natural information.

Karen Neander (Duke) proposed a singular causal account of information, according to which one token event carries information about another if it causes it or is caused

by it. Such an account requires token causation for information. Neander argued that different theories of information may pursue different projects and may therefore differ in the criteria of adequacy.

Andrea Scarantino (Georgia State) traced the changes in Dretske's views about information and outlined a probabilistic theory of information. He developed the idea that an event carries information about another if the occurrence of the first changes the probability of the latter. Pace Dretske, carrying information does not require that the probability be raised to unity.

Aaron Meskin (Leeds) defended a counterfactual theory of information. On this account, one state carries information about another if a certain counterfactual relation obtains between them. Whether or not this relation obtains is independent of facts about any receivers. Meskin argued that the counterfactual theory can account for probabilistic events.

Nicholas Shea (Oxford) discussed issues about the relation between representational content and correlational information in Skyrms-type models. He argued that Skyrms equates representational content with Kullback-Leibler information. Shea introduced a distinction between such information and functional content and developed a quantitative approach for the latter.

Ruth Millikan (Connecticut) argued that correlational views of information have so far not satisfactorily addressed the reference class problem. She proposed a non-arbitrary way of specifying the reference class, according to which information becomes relative to the signal receiver. Millikan then extended this approach from correlations to single-case patterns.

Hilmi Demir (Bilkent) investigated the fate of Grice's distinction between non-natural meaning and natural meaning. He argued that in both Dretske's and Scarantino and Piccinini's works, Grice's distinction is assumed to form a dichotomy. Demir suggested a revised version of Grice's distinction, in which natural and non-natural meaning categories form a continuity.

Ulrich Stegmann (Aberdeen) explored some assumptions and gaps in probabilistic theories of information. He argued that such theories should say more about the sense in which information 'enables' receivers to learn something from a signal, and he suggested how the problem of single-case probabilities may be circumvented for explanatory purposes.

ULRICH STEGMANN

University of Aberdeen

Perspectives on Structuralism, 16–18 February

With 20 talks, ten of which from the call for papers, the largest European meeting on the structuralist program so far took place at the [Center for Advanced Studies](#), Ludwig Maximilian University, Munich, Germany, funded by a generous grant to Holger Andreas.

Holger Andreas (Munich, Germany) rebutted the claim that the structuralist framework is incompatible with a broadly Carnapian conception of scientific theories, pre-

senting a Carnap-Sneed system combining the merits of Carnap's dual level conception with the structuralist framework's expressive power.

Wolfgang Balzer and Klaus Manhart (Munich, Germany) studied the notion of a process from a structuralist perspective, elaborating definitions of a structure of states, kinds of processes, and a process itself, then applied this to investigate and clarify relations between scientific and social processes.

Christian Damböck (Vienna, Austria) proposed a reduction device, meta-theoretically relating two object level-theories by means of an (empirical) truth claim, which establishes full interpretations at the level of inter-theory-relations so that reductions between (partially) incommensurable theories become possible.

José Diez (Barcelona, Spain) and Pablo Lorenzano (Quilmes, Argentina) applied the structuralist framework to reconstruct a natural selection guiding principle no more or less of a law or definition than Newton's $F = ma$; whence, should the theory of natural selection "die," then it is in good company.

José L. Falguera and Xavier de Donato (Santiago de Compostela, Spain) addressed Kuhn's notion of local incommensurability, and proposed a distinction between characteristic and non-characteristic terms of two incommensurable theories which suffices to determine what these theories have in common, leading to a revised definition of incommensurability.

Mathias Frisch (Maryland, USA) made causation intelligible through the structuralist meta-theory, where the key idea is to extend partial models from measurements to full models under causality constraints (using Pearl's account of causation) such that considerations of likelihood allow for a further delimitation of admissible models.

Ulrich Gähde (Hamburg, Germany) addressed the standard structuralist account of extending an empirical base set into a theoretical description, arguing that not only may the values of theoretical or non-theoretical functions be determined in a theory-dependent way but also the base sets on which these functions are defined.

Peter Gärdenfors and Frank Zenker (Lund, Sweden) presented how to recover key distinctions of the structuralist program, particularly the kinds of models, in conceptual spaces, arguing for a reformulation of some elements of a 'theory core,' to achieve a richer distinction than normal vs. revolutionary science.

Lena Hofer (Munich, Germany) explicated an intuition she calls "the promise of theories:" the claim that a theory will, in the future, (continue to) describe all phenomena of its empirical base, making use of the recently forwarded Carnap-Andreas semantics.

Martin Hoffmann (Hamburg, Germany) reconstructed the basic theory element of intelligence factor theory in biological psychology; while intelligence potentials are considered to have a genetic basis, they are assumed to be realized by adequate environments (e.g., schools), resulting in a measure for the heritability of intelligence.

Mariano Lastiri (Buenos Aires, Argentina) provided a sketch of the quantum measurement problem of non-relativistic quantum mechanics (QM) for closed systems such that the quantum state, momentum, energy, mass, angular momentum, and spin are QM-theoretical, while position operators, velocity, and time are QM-non-theoretical.

Hannes Leitgeb (Munich, Germany) engaged in the Kantian *a priori* as "constitutive of the concept of the object of [scientific] knowledge," to demonstrate that, by means of Hilbertian epsilon terms—as suggested by Carnap—the Ramsey sentence of an

empirical theory can determine a mathematical structure mediating between empirical phenomena and theoretical laws, thus making the relativized *a priori* more precise.

Pablo Lorenzano (Quilmes, Argentina) reacted to a critique by Frederick Suppe, who excludes the Sneed-Stegmüller approach from the semantic conception, finding Suppe to have misunderstood and, thereby, hindered a better dialogue among proponents of the semantic view.

Sebastian Lutz (Utrecht, The Netherlands) presented results suggesting that transitions from sentences to structures to pure structures (and back!) are possible, while problems (e.g., the connection to the world) and solutions (e.g., change of structure through definitions) transfer across the semantic and the syntactic view.

Tillmann Massey (Munich, Germany) provided examples of author co-citation analysis, pointing out that, *vis-à-vis* policy makers' current expectations as to its viability in measuring the impact of funding schemes, structuralism can and should engage with this topic to a greater extent.

Thomas Meier (Munich, Germany) considered structural realism as an epistemology for the structuralist program such that its meta-theoretical elements can improve structural realism in making more precise the connections between empirical theories, particularly through set theoretic specifications of structural continuity.

C. Ulises Moulines (Munich, Germany) presented a general structuralist framework to represent types of theoretical change by means of inter-theoretical relations, improving upon Kuhn's diachronic distinction (into normal vs. revolutionary science) through a four-valued scheme (crystallization, evolution, embedding, replacement).

Graciana Petersen (Hamburg, Germany) treated models of fluid dynamics in application to models for wind energy assessment based on the Navier-Stokes equations as evidence of a seeming disorganization of theoretical approaches and uncertainties in the practitioner's ability to reliably assess wind energy.

A.V. Ravishankar Sarma (Kanpur, India) developed a structuralist framework for belief revision with a causal epistemic entrenchment ordering (overlapping some formal properties of AGM's epistemic entrenchment), and presented the transition from Cartesian to Newtonian mechanics as a case guided by principles of causal relevance.

Gerhard Schurz (Düsseldorf, Germany) proposed a criterion of empiricity based on cognitive-psychological learnability—thus allowing for a *gradual* notion of observability—, while T-theoreticity and pre-T-theoreticity are defined recursively through quasi-reduction sentences, and relative to a theory, allowing for measurement chains.

Organizers were Holger Andreas and Frank Zenker. See [here](#) for abstract; selected papers to appear 2013 with *Erkenntnis*.

FRANK ZENKER

Lund University, Sweden

HOLGER ANDREAS

LMU, Munich, Germany

Laws and Chances, 5 March

The workshop focused on laws and objective probabilities in the special sciences and in physics (it was held in Cologne and organized by the DFG Research Group *Causation and Explanation*).

Barry Loewer argued for a statistical-mechanical explanation of macroscopic statistical laws. A paradigm example of such a statistical and time-asymmetric macro-law is the second law of thermodynamics. The explanation why the macro-world (which we assume to be governed by deterministic and time-symmetric fundamental laws) conforms to the second law is provided by positing a low entropy state at the initial state of the universe (the so-called past-hypothesis) and a uniform probability distribution over the micro-states possibly realizing this special initial state. Loewer further argued that the statistical-mechanical approach is superior to Tim Maudlin's metaphysics of (statistical) laws and the direction of time.

Reminding us of the old and solved problem David Lewis had with predicates/properties in his original best systems account of laws of nature, Markus Schrenk made us aware of new problems with predicates/properties that the better best system account for special science laws is facing. Schrenk argued that the difficulties—such as inter-science relations and their demarcation, and possible contradictions between the sciences—can be overcome but probably only at the cost of giving the better best system account a pragmatist twist.

The aim of Claus Beisbart's talk was to assess David Lewis's best system account of objective chances and to compare it to Carl Hoefer's recent proposal. He argued that Lewis's account does a good job in capturing pre-theoretical intuitions about chances, but that the account needs further elaboration because of the zero-fit problem. Beisbart further suggested that Lewis's account does not strictly exclude lawless chances.

Alexander Reutlinger and Andreas Hüttemann addressed a proposal by John Earman and John Roberts according to which the laws of the special sciences are nothing but statistical laws that are not in need of *ceteris paribus* qualifications (the "statistical account" of laws in the special sciences). Hüttemann and Reutlinger argued that the statistical account fails because: (a) not all special science laws are associated with a statistical pattern required for being statistical law (that is, a specific probability distribution), (b) some Humean accounts of objective probability face a problem if the statistical laws involve idealizations, and (c) the truth of many statistical generalizations in the special sciences *does* seem to depend on the fact that particular *ceteris paribus* conditions obtain.

John Roberts's advocated Nomic Frequentism (NF). According to NF, statistical laws are laws about frequencies. That is, insofar as probabilities figure in law statements they refer to frequencies. Roberts's account remains silent on probabilities that do not play a role in any law of nature. He argued that one implication of NF is that probabilities (as referred to in statistical laws) are not single-case chances—rather they are type-level probabilities. Roberts's primary goal was to defend NF against various objections such as: Does NF imply arithmetical restrictions on, for instance, how many coin-tosses there can be? Is a proponent of NF committed to "spooky" action at a distance? If NF is true, is the independence of the outcomes of probabilistic experiments

violated? Does NF allow for frequency tolerance of probability assignments?

The workshop was organized by Alexander Reutlinger.

ALEXANDER REUTLINGER

Department of Philosophy, University of Cologne

Disposition, Causes, Modality, 7–9 March

The workshop (held in Cologne and organized by the DFG Research Group *Causation and Explanation*) focused on Humean and dispositionalist accounts of dispositions, causation and modality. Dispositionalists are philosophers who defend the view that many or all properties have an irreducible dispositional nature. Some dispositionalists have recently claimed that causal powers, capacities, tendencies, etc. bring their own kind of modality to the world. Forces or vectors sometimes serve as a preliminary characterization of a disposition's sui generis modality. However, such a theory of dispositional modality has yet to be spelled out in detail.

Daniel von Wachter argued against the claim that causes necessitate their effects, while Neil Williams provided an argument supporting the claim. The main points of disagreement between von Wachter and Williams regard (a) what counts as a possible disturbing factor of a determinist relation between cause and effect, and (b) the completeness of the fundamental laws of physics, and (c) and what status we should assign to “totality fact”-provisos that say that the events specified are all there is (so that nothing else could possibly intervene).

Richard Corry suggested an improvement of Mumford and Anjum's recent account of powers. Corry proposed not to model powers as vectors (as Mumford and Anjum do) but as vector-fields. The vector-field model provides an account of the “infinitely-multi-track nature” of many powers (by modeling it as a function), and it provides a bridge to the ontology of fields found in physics. Relatedly, Olivier Massin discussed the metaphysical relation between component forces and resultant forces in Newtonian mechanics—both of which are typically represented by vectors. The upshot of Massin's argument was that, roughly, resultant forces should be understood as the mereological sum of the component forces.

John Roberts advocated an original approach to nomic and counterfactual modality by applying modal normativism (as defended, in some form, by Sellars, Brandom, and Thomasson). Modal Normativism is the view that modal discourse is not descriptive discourse; instead it serves the prescriptive purpose of expressing norms of one sort or another. Roberts's proposal is to assume that nomic claims express epistemic norms regarding reliable methods of measurement.

Helen Beebe and Ralph Busse presented arguments against dispositional essentialism. Beebe argued that essentialist claims such as “necessarily, if something has property *P*, then law *L* is true” should not be understood as a posteriori claims. Rather they should be interpreted as a priori necessities. Beebe argued that, if this is so, this amounts to a reductio of the essentialist claim. Complementing Beebe's talk, Busse pointed out several problems for the metaphysics of modality endorsed by dispositional essentialists.

Jonathan Jacobs and Barbara Vetter defended dispositionalism. Jacobs explored a neo-Aristotelian theory of the “grounds” for modal truths, according to which modality is grounded in substances and their powers. Vetter outlined an account of grounding modality in dispositions or potentialities.

The talks were commented by Kristina Engelhard, Arno Goebel, Siegfried Jaag, Elina Pechlivanidi, Alexander Reutlinger, Stefan Schmid, Matthew Tugby, Daniel Wehinger, and Alastair Wilson.

The workshop was organized by Markus Schrenk and Alexander Reutlinger.

ALEXANDER REUTLINGER

Department of Philosophy,
University of Cologne

Graduate Conference in Philosophy of Science, 8–9 March

The Erasmus University Rotterdam hosted a graduate conference in philosophy of science on March 8–9. This event was the second of its kind in the Dutch-Flemish region—the first was held in Ghent in November 2010. Given the success of these two conferences, more events in this series are to be expected.

This year’s event featured four research-paper sessions, two research-project sessions and three keynote lectures. Each session with student speakers had an official discussant.

The first research-paper session was on formal philosophy of science and started with Lucas Halpin’s (UC Davis) proposal for a new definition of analyticity. Dominik Klein (Tilburg) then offered a procedure for aggregating experts’ judgements through weighted averaging. He identified conditions under which this aggregating procedure outperforms alternatives. Finally, Patryk Dziurosz-Serafinowicz (Groningen) offered an interpretation of Lewisian chance in terms of expert functions.

The second session focused on the history of science. First, Tom Bunce (Durham) discussed Max Born’s philosophy of causation which is articulated around the principles of antecedence and contiguity. Second, Matias Slavov (Jyväskylä) compared Newton’s outlook on gravitation to Hume’s conception of causality. Third, Marij Van Strien (Ghent) put the contemporary discussion about the Norton dome in historical perspective by showing that although some French authors in the 19th century discussed the same problem, they did not interpret it as a threat to determinism.

In the third session on general philosophy of science, Mikael Melan (Turku) argued that whether an explanation is relevant is fundamentally a contextual issue, and that one should distinguish between qualitative and quantitative aspects of relevance. Olivier Sartenaer (Louvain) then offered a taxonomy enabling one to distinguish between two consistent emergentist positions, each coming with a particular interpretation of the maxim ‘neither dichotomy, nor identity’.

The last research-paper session on the philosophy of economics included four presentations. First, Pim Klaassen (Amsterdam) articulated the meaning of ‘trust’ in neoeconomics and argued that this meaning is substantially different from the colloquial meaning of trust. Second, Luis Mireles-Flores (Rotterdam) used the case of the North

American Free Trade Agreement to illustrate how the types of evidence which should inform policy making can be at odds with the types of evidence needed to support a causal generalization. Third, Stefan Mendritzki (Eindhoven) analysed the notion of a ‘stylized fact’, which is widely used in economics but has not yet been the object of philosophical explication. Finally, Guus Dix (U of Amsterdam) traced the emergence of the concept of ‘incentive’ in economics.

The research-project sessions featured four students who have recently started their PhD or will be starting soon. Ioan Dragos (Ryerson) presented his research on the possibility of reconciling the Strong Programme with realism. Joost Hengstmengel (Rotterdam) gave an overview of his PhD project on the role of divine providence in early-modern economic thought. Hisashi Oki (Rotterdam) described his Master thesis on the issue of adaptive preferences for capability-based development policies, and he described how he wants to expand on this work in the future. Nikolaos Skiadopoulos (Athens) presented his project on the assumption of instrumental agency in the history of choice theory.

Three keynote lectures were also on the programme. Arianna Betti (Amsterdam) opened the conference with a talk on the classical model of science as a cognitive schema to interpret the history of philosophy. James McAllister (Leiden) closed the first day by presenting his third way to the history of science—between presentism and contextualism. The final talk was by Ingrid Robeyns (Rotterdam) who argued that we need a procedure to identify the rich—analogue to the procedure to identify the poor—and gave the outline of such a procedure.

This graduate conference has been a great occasion for students to build up their network and receive comments on their work. It also illustrated the diversity of approaches and topics in contemporary philosophy of science.

FRANÇOIS CLAVEAU

Erasmus Institute for Philosophy and Economics,
Erasmus University Rotterdam

Graduate Conference of the Vienna Forum for Analytic Philosophy, 9–11 March

From March 9–11, 2012, the Vienna Forum for Analytic Philosophy held its first Graduate Conference on contemporary theories of truth. It was an intense programme consisting of three distinguished keynote speakers and eight submitted student papers. The first keynote address on Belief Truth Norms was given by Paul Horwich (NYU). He presented the thesis that we ought to want to have true beliefs and addressed the question why we should endorse it. He thereby argued against pragmatic approaches that we do not just want justified beliefs, but that knowledge gets its value from beliefs being true.

On Saturday, Ceth Lightfield and Danilo Dantas (both UC Davis) gave a detailed analysis of several aspects of Paul Horwich’s minimalism. In particular, they discussed the “substitution problem”, the “generalization problem” and possible responses to the Liar Paradox.

Monika Gruber (Salzburg) was concerned with the question raised by Alfred Tarski, whether we can construct a theory of truth for languages of infinity order and the related question whether we can make sense of an unbounded hierarchy of languages.

After that, Tyrus Fisher (UC Davis) gave hints on how one could avoid problems concerning the totality of Equivalence Schema Instances. Ivo Pezlar (Brno) presented a modal explication of truth in which it is possible to model truth as an operator in a relational semantics.

Thomas Schindler (Munich) gave an account of “grounding” which makes it possible to analyze various paradoxes in a more fine-grained way than other presently available theories. Thus, in contrast to, for example Kripke’s negative definition of defective, viz., ungrounded sentences (a sentence is ungrounded iff it is not in the minimal fixed point), his theory gives rise to a positive definition using the notions of sensitivity and dependence.

The second keynote speaker, Jeffrey Ketland (Oxford), gave a penetrating discussion of philosophical issues surrounding Deflationism and Semanticism (his term). He presented the core theses of both viewpoints and addressed the major issues requiring future research in this area.

On Sunday, Evan Clarke (Boston College) gave a sophisticated analysis and critique of Williamson’s views on vagueness, addressing the core theses and problems. Lukas Likavcan (Brno) raised questions concerning the truth-determinateness of pragmatic presuppositions as used by Stalnaker and defended the importance of contextual factors for the determination of truth values in ordinary language.

Finally, the third keynote address was delivered by Leon Horsten (Bristol) on truth and conditionals. In his talk, he compared various conditionals employed in non-classical solutions to the semantic paradoxes. He criticized Field’s conditional as having no uniform motivation and being not Kripkean in spirit (as Field’s hierarchy does not reach a fixed point). Then he reconsidered Yablo’s conditional as a possible alternative to Field’s, presented its main properties and raised the open question how the construction can be consistently iterated.

To sum up, we had a philosophically inspiring and instructive conference with outstanding presentations and lively discussions.

LEO STADLMÜLLER

SEBASTIAN KLETZL

Vienna Forum for Analytic Philosophy,
University of Vienna

Calls for Papers

DISAGREEMENTS: special issue of *Erkenntnis*, deadline 1 April.

PROBABILITY, LOGIC AND LEARNING: special issue of *Theory and Practice of Logic Programming*, deadline 2 April.

LOGICAL ISSUES IN THE HISTORY AND PHILOSOPHY OF COMPUTING: special issue of *History and Philosophy of Logic*, deadline 15 April.

TRENDS IN THE HISTORY AND PHILOSOPHY OF COMPUTING: special issue of *Philosophy & Technology*, deadline 15 April.

FORMAL AND INTENTIONAL SEMANTICS: special issue of *The Monist*, deadline 30 April.

THE MIND-BODY PROBLEM IN COGNITIVE NEUROSCIENCE: special issue of *Philosophia Scientiae*, deadline 1 May.

INFORGS AND THE INFOSPHERE: THEMES FROM LUCIANO FLORIDI'S PHILOSOPHY OF ARTIFICIAL INTELLIGENCE: special issue of *The Journal of Experimental & Theoretical Artificial Intelligence*, deadline 1 July.

MIND AND PARADOX: special issue of *Journal of Experimental & Theoretical Artificial Intelligence*, deadline 1 July.

THE AIM OF BELIEF: special issue of *Teorema*, deadline 15 September.

SCIENCE VS. SOCIETY? SOCIAL EPISTEMOLOGY MEETS THE PHILOSOPHY OF THE HUMANITIES: special issue of *Foundations of Science*, deadline 31 October.

WHAT'S HOT IN . . .

. . . Uncertain Reasoning

A recent exchange between D.G. Mayo and S. Senn on *Rationality, Markets and Morals* (Vol. 3, 2012, Special Topic: Statistical Science and Philosophy of Science, edited by D.G. Mayo, A. Spanos and K.W. Staley) raises a number of questions about the very meaning of the noun “Bayesian” and the adjective “bayesian”.

The fact that Bayesians could come in (way too) many sorts was combinatorially illustrated by I.J. Good (1983: “46656 kinds of Bayesians” in *Good Thinking*, University of Minnesota Press, 20–22.). Senn’s paper “You May Believe You Are a Bayesian But You Are Probably Wrong”, goes much beyond Good’s provocation effectively suggesting that bayesianism is akin to a civil religion to which some statisticians feel culturally obligated but which needn’t actually constrain their practice. Mayo takes issue with this in a way which Senn doesn’t find convincing.

One might feel that “deep down”, to borrow a term from the Mayo-Senn exchange, this whole issue goes little beyond what otherwise appears to be a terminological quarrel. Those who share this worry may find the survey by S.E. Fienberg (2006: “When Did Bayesian Inference Become Bayesian”, *Bayesian Analysis* 1(1): 1–40) quite interesting indeed. To avoid raising too high expectations, Fienberg reminds us that Bayes’s Theorem might not be Bayes’s work and that the first recorded use of the adjective “bayesian” is due to statistician R.A. Fisher, who clearly meant it in a pejorative sense. If that’s how it began, no wonder that the subject turned out to be a terminological mine field.

Yet Fienberg guides us through a fascinating history which counts essentially three major phases. The first begins, naturally, with Bayes. His theorem was published in a [paper](#) communicated by R. Price at the Royal Society in 1763 (after Bayes’s death). However it wasn’t until the publication of Laplace’s *Théorie Analytique des Probabilités* in 1812 that the two key ideas of the Price-Bayes paper, namely the statement of the

“inverse probability” problem and the subjective interpretation of probability, became directly relevant to the scientific and cultural development of the time.

The second phase is somewhat negative and culminates in the replacement of “inverse methods” with the frequentist concepts of hypothesis testing and confidence intervals. The work of Fisher, Pearson and Neyman, albeit heterogeneous, gave rise to what some authors refer to as “classical statistics”. Meanwhile, subjectivists like Borel, Keynes, Ramsey and de Finetti—again hardly a homogeneous bunch!—set the stage for a neo-bayesian revival. It is in response to this emerging view that Fisher makes the first (derogative) use of the adjective bayesian in 1950. As Fienberg notes:

In personal correspondence, Jack Good notes that “Bayesian” is now usually used to refer to a whole philosophy or methodology in which subjective or logical probabilities are used, and Fisher had a far more restricted notion in mind! But “Bayesian” is the word Fisher chose to use, and such negative usage of the term suggest that it might have been used similarly by others in previous oral exchanges. (2006: 16)

This much for the first use of the adjective. But what about its usage? Fienberg suggests that the essential event which shaped the current meaning of the adjective was the publication of L.J. Savage (1954: *The Foundations of Statistics*, Wiley). Just to confirm that the development of bayesianism has been far from linear, Savage never uses the adjective “bayesian” in his book.

Fienberg is careful enough not to make any attempts at defining who is a Bayesian, or what does “bayesian inference” means precisely. One good reason for doing so might certainly be that “bayesian” means (slightly) different things in statistics, philosophy, artificial intelligence, economic theory, and presumably in all the special fields in which it is applied. Yet Fienberg’s chronicle provides evidence that the subjective interpretation of probability, the choice theoretic setting and the recognition of the importance of the likelihood principle in inductive reasoning have provided much coherence to the otherwise disarrayed emergence of bayesianism. All these aspects certainly play a major role in the theory developed by Savage, who, among other things, translated Borel into English and contributed essentially to circulating de Finetti’s idea across the English speaking statistical community.

HYKEL HOSNI

Scuola Normale Superiore, Pisa
LSE Choice Group, London

LETTERS

Dear Reasoners,

In ‘An argument for not equating confirmation and explanatory power’ (*The Reasoner* 6(3):39–40), the statement (S) was inadvertently misspelled. It should read:

Symmetry (S) [Corrected]: For any e_1, e_2, h and any $P, E(e_1, h) > / = / < E(e_2, h)$ iff $E(\neg e_1, h) < / = / > E(\neg e_2, h)$.

This is the assumption that supports the relevant step in the Proof, namely, that from $E(e, h) = E(e \wedge x, h)$ to $E(\neg e, h) = E(\neg(e \wedge x), h)$.

VINCENZO CRUPI

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EVENTS

APRIL

YSM: Young Statisticians' Meeting, Cambridge, 2–3 April.

DARC: Dynamics Of Argumentation, Rules, and Conditionals workshop, Luxembourg, 2–3 April.

BCTCS: British Colloquium for Theoretical Computer Science, Manchester, UK, 2–5 April.

SBP: International Conference on Social Computing, Behavioral-Cultural Modeling, & Prediction, University of Maryland, 3–5 April.

MIND, METHOD AND MORALITY: Pittsburgh, 6–7 April.

CNCS: International Conference on Computer Networks and Communication Systems, Malaysia, 7–8 April.

EMCSR: European Meetings on Cybernetics and Systems Research, Vienna, 10–13 April.

TIME FOR CAUSALITY: Workshop on Causal Inference and Dynamic Decisions in Longitudinal Studies, Bristol, 10–13 April.

evoSTOC: Evolutionary Algorithms in Stochastic and Dynamic Environments, Malaga, Spain, 11–13 April.

PHDs IN LOGIC IV: Ghent, 12–13 April.

OBJECTS, KINDS AND MECHANISMS IN BIOLOGY: One Day Workshop, University of Leeds, 13 April.

NORTHWESTERN/NOTRE DAME GRADUATE EPISTEMOLOGY CONFERENCE: Northwestern University, Evanston, IL, 13–14 April.

PHILOSTEM: 3rd Midwest Workshop in Philosophy of Science, Technology, Engineering, and Mathematics, Indiana, USA, 13–14 April.

BMC2012: Workshop on Turing's Legacy in Mathematics and Computer Science, University of Kent, 16–19 April.

PROOF THEORY AND MODAL LOGIC: Barcelona, 16–19 April.

CONFRONTING INTRACTABILITY IN STATISTICAL INFERENCE: University of Bristol, 16–19 April.

COLLECTIVE INTELLIGENCE: MIT, Cambridge, MA, 18–20 April.

BEING FREE, DOING FREE: Freedom Between Theoretical and Practical Philosophy, University of Freiburg, Germany, 19–21 April.

GIRL: 1st Conference on Games, Interactive Rationality and Learning, Lund, 19–21 April.

PSYCHOLOGY, EMOTION, AND THE HUMAN SCIENCES: University of Windsor, Windsor, Ontario Canada, 20–21 April.

MAICS: 23rd Midwest Artificial Intelligence and Cognitive Science Conference, Ohio, 21–22 April.

AISTATS: 15th International Conference on Artificial Intelligence and Statistics, La Palma, Canary Islands, 21–23 April.

AGENTS AND CAUSES: Interdisciplinary Aspects in Mind, Language and Culture, Bielefeld, 21–23 April.

ENTIDENTIC: Entity and Identity in Bioethics, Paris, France, 23–24 April.

THE PROGRESS OF SCIENCE: Tilburg Center for Logic and Philosophy of Science, 25–27 April.

SDM: 12th SIAM International Conference on Data Mining, Anaheim, California, USA, 26–28 April.

MAY

BoBiCOLL: 1st Bochum-Bielefeld Colloquium: Philosophical Perspectives on Epistemology, Mind, and Science, Ruhr-Universität Bochum, Germany, 4–5 May.

SOPHA: Société de philosophie analytique, Paris, 4–6 May.

ICDDM: International Conference on Database and Data Mining, Chengdu, China, 5–6 May.

ICFCA: 10th International Conference on Formal Concept Analysis, Leuven, Belgium, 6–10 May.

BELIEF FUNCTIONS: Compiègne, France, 9–11 May.

NATURALISM AND NORMATIVITY IN THE SOCIAL SCIENCES: University of Hradec Králové, Czech Republic, 10–12 May.

PHILOSOPHY AND COMPUTATION: Lund University, Sweden, 12–13 May.

ABMPHIL: Agent-Based Modeling in Philosophy, Spa, Belgium, 15–19 May.

CASI: 32nd Conference on Applied Statistics Ireland, 16–18 May.

GAMES, GAME THEORY AND GAME SEMANTICS: 8th International Symposium of Cognition, Logic and Communication, Riga, Latvia, 18–20 May.

LMP: 12th Annual Philosophy of Logic, Mathematics, and Physics Conference, University of Western Ontario, 20–21 May.

SLACRR: St. Louis Annual Conference on Reasons and Rationality, 20–22 May.

IPDPS: 26th IEEE International Parallel and Distributed Processing Symposium, Shanghai, China, 21–25 May.

JdS: 44th Journées de Statistique, Brussels, 21–25 May.

PHML: Philosophy, Mathematics, Linguistics: Aspects of Interaction, St. Petersburg, Russia, 22–25 May.

UR: Uncertain Reasoning, Special Track at FLAIRS-25, Marco Island, Florida, USA, 23–25 May.

SSHAP: Mind, Language and Cognition, McMaster University, Canada, 24–26 May.

PHILMILCOG: 10th Annual Graduate Conference on the Philosophy of Mind, Language and Cognitive Science, University of Western Ontario, 24–26 May.

THE AIMS OF INQUIRY AND COGNITION: Edinburgh Epistemology Research Group, University of Edinburgh, 25–26 May.

EXPERTS AND CONSENSUS IN ECONOMICS AND THE SOCIAL SCIENCES: University of Bayreuth, Germany, 25–26 May.

CSAE: IEEE International Conference on Computer Science and Automation Engineering, Zhangjiajie, China, 25–27 May.

ICKD: 2012 International Conference on Knowledge Discovery, Indonesia, 26–27 May.

AI2012: Canadian Conference on Artificial Intelligence, 28–30 May.

RTA: 23rd International Conference on Rewriting Techniques and Applications, Japan, 28 May–2 June.

FEW: 9th Annual Formal Epistemology Workshop, Munich, 29 May–1 June.

ICCC12: Third International Conference on Computational Creativity, Dublin, 30 May–1 June.

STOCHMOD: 4th meeting of the EURO Working Group on Stochastic Modeling, Ecole Centrale Paris, 30 May–1 June.

HUMAN COMPLEXITY: The University of North Carolina, Charlotte, 30 May–1 June.

CAMBRIDGE PRAGMATISM: a Research Workshop, Cambridge, UK, 31 May–1 June.

RUDOLF CARNAP LECTURES: Ruhr-Universität Bochum, 31 May–2 June.

JUNE

INCOMMENSURABILITY 50: Taipei, Taiwan, 1–3 June.

ICFIE: International Conference on Fuzzy Information and Engineering, Hong Kong, 2 June.

TRENDS IN LOGIC XI: Advances in Philosophical Logic, Ruhr University Bochum, 3–5 June.

LAMAS: 5th Workshop on Logical Aspects of Multi-Agent Systems, Valencia, 4–5 June.

WCSB: 9th International Workshop on Computational Systems Biology, Ulm, Germany, 4–6 June.

FEW: Formal Epistemology Week, Konstanz, 4–6 June.

AAMAS: 11th International Conference on Autonomous Agents and Multiagent Systems, Valencia, Spain, 4–8 June.

CILC: 9th Italian Convention on Computational Logic, Sapienza University of Rome, 6–7 June.

EXTENDED COGNITION AND EPISTEMOLOGY: Amsterdam, 6–7 June.

MFPS: 28th Conference on the Mathematical Foundations of Programming Semantics, University of Bath, 6–9 June.

MINDS, BODIES, AND PROBLEMS: Bilkent University, Ankara, 7–8 June.

EDINBURGH EPISTEMOLOGY GRADUATE CONFERENCE: University of Edinburgh, 8–9 June.

FOUNDATIONS OF LOGICAL CONSEQUENCE: University St Andrews, 8–10 June.

NMR: 14th International Workshop on Non-Monotonic Reasoning, Rome, Italy, 8–10 June.

RATS: Recent Advances in Time Series Analysis Workshop, Cyprus, 9–12 June.

NORDSTAT: 24th Nordic Conference in Mathematical Statistics, Northern Sweden, 10–14 June.

WORKSHOP ON THE INCOMPUTABLE: Kavli Royal Society International Centre, Chicheley Hall, UK, 12–15 June.

MS5: Conference on Models and Simulations, Helsinki, 14–16 June.

CSAM: Classification Society Annual Meeting, Carnegie Mellon University, Pittsburgh, PA, 14–16 June.

BASIC KNOWLEDGE: Conference on the A Priori, Aberdeen, 16–17 June.

SAT: International Conference on Theory and Applications of Satisfiability Testing, Trento, Italy, 17–20 June.

LOFT: 10th Conference on Logic and the Foundations of Game and Decision Theory, Sevilla, Spain, 18–20 June.

DM: Discrete Mathematics, Dalhousie University, Halifax, Nova Scotia, Canada, 18–21 June.

LOGICA: Hejnice, northern Bohemia, 18–22 June.

CiE: Computability in Europe, University of Cambridge, Cambridge, 18–23 June.

RETHINKING SCIENCE AFTER THE PRACTICE TURN: Nancy, France, 19–20 June.

SISSM: Scientific Meeting of the Italian Statistical Society, Rome, Italy, 20–22 June.

PHILOSOPHICAL INSIGHTS: Senate House, University of London, 21–23 June.

MBR12: Model-Based Reasoning in Science and Technology, Sestri Levante, Italy, 21–23 June.

SPP: Annual Meeting of the Society for Philosophy and Psychology, University of Colorado at Boulder, 21–24 June.

HOPOS: Halifax, Nova Scotia, Canada, 21–24 June.

CCA: 9th International Conference on Computability and Complexity in Analysis, Cambridge, UK, 24–27 June.

COLT: 25th Annual Conference on Learning Theory, Edinburgh, 25–27 June.

MPC: 11th International Conference on Mathematics of Program Construction, Madrid, Spain, 25–27 June.

ARTIFICIAL INTELLIGENCE AND SOFT COMPUTING: Naples, Italy, 25–27 June.

VANiM: Values and Norms in Modeling, Eindhoven, The Netherlands, 25–27 June.

LICS: 27th ACM/IEEE Symposium on Logic In Computer Science, Dubrovnik, Croatia, 25–28 June.

SQUARE OF OPPOSITION: American University of Beirut, 26–29 June.

ICML: 29th International Conference on Machine Learning, University of Edinburgh, 26 June–1 July.

IJCAR: 6th International Joint Conference on Automated Reasoning, Manchester, UK, 26 June–1 July.

SEMANTICS AND PRAGMATICS OF CETERIS PARIBUS CONDITIONS: University of Düsseldorf, 28–29 June.

DGL12: Sixth Workshop in Decisions, Games & Logic, LMU Munich, 28–30 June.

EEN: European Epistemology Network Meeting, Universities of Bologna and Modena, Italy, 28–30 June.

EVOLUTION AND FUNCTION OF CONSCIOUSNESS: Summer School in Cognitive Science 2012, Montreal, Canada, 30 June–9 July.

JULY

UNCERTAINTY IN COMPUTER MODELS: Sheffield, UK, 2–4 July.

AISB/IACAP: Birmingham, UK, 2–6 July.

HAI: Hypercomputation and AI Symposium, Birmingham, UK, 2–6 July.

LASR: 31st Leeds Annual Statistical Research Workshop, University of Leeds, 3–5 July.

BOUNDED RATIONALITY: Summer Institute on Bounded Rationality, Berlin, Germany, 3–10 July.

FOUNDATIONS FOR AN INTERDISCIPLINARY DECISION THEORY: Max Planck Institute for Human Development, Berlin, Germany, 3–10 July.

ICT: 7th International Conference on Thinking, London, 4–6 July.

IIBM: 5th International Workshop on Intelligent Informatics in Biology and Medicine, Palermo, Italy, 4–6 July.

HISTORY AND PHILOSOPHY OF PROGRAMMING: Ghent University, 5–6 July.

BSPS: Annual Conference of the British Society for the Philosophy of Science, University of Stirling, 5–6 July.

CAV: 24th International Conference on Computer Aided Verification, Berkeley, 7–13 July.

ISSCSS: International Summer School in Cognitive Sciences and Semantics, Latvia, 8–18 July.

ASC: 21st Australian Statistical Conference, Adelaide, 9–12 July.

IPMU: 14th International Conference on Information Processing and Management of Uncertainty in Knowledge-Based Systems, Catania, Italy, 9–13 July.

ICALP: 39th International Colloquium on Automata, Languages and Programming, University of Warwick, 9–13 July.

FOUNDATIONS OF MATHEMATICS: University of Cambridge, 10–12 July.

TViTC: Theoretical Virtues in Theory-Choice, University of Konstanz, 12–14 July.

DEON: 11th International Conference on Deontic Logic in Computer Science, University of Bergen, Norway, 16–18 July.

WORLD COMP: The 2012 World Congress in Computer Science, Computer Engineering, and Applied Computing, Nevada, USA, 16–19 July.

DMIN: 8th International Conference on Data Mining, Nevada, USA, 16–19 July.

SIPTASS: Society for Imprecise Probability: Theories and Applications Summer School, Pescara, Italy, 16–20 July.

INTERFACES OF THE MIND: workshop at Ruhr-Universität Bochum, Germany, 19–21 July.

ISA: IADIS International Conference Intelligent Systems and Agents, Lisbon, Portugal, 21–23 July.

PARADOX AND LOGICAL REVISION: LMU, Munich, 23–25 July.

WoMO: 6th International Workshop on Modular Ontologies, Graz, Austria, 24 July.

FOIS: 7th International Conference on Formal Ontologies in Information Systems, Graz, Austria, 24–27 July.

AUGUST

NAFIPS: 31th North American Fuzzy Information Processing Society Annual Conference, Berkeley, 6–8 August.

ESSLLI: 24th European Summer School in Logic, Language and Information, Poland, 6–17 August.

KDD: 18th ACM SIGKDD Conference on Knowledge Discovery and Data Mining, Beijing, China, 12–16 August.

ITP: 3rd Conference on Interactive Theorem Proving, Princeton, NJ, 13–16 August.

LOGIC AND COGNITION: Logic and Cognition Workshop, Opole, Poland, 13–17 August.

UAI: Conference on Uncertainty in Artificial Intelligence, Catalina Island, USA, 15–17 August.

SLS: 8th Scandinavian Logic Symposium, Roskilde University, Denmark, 20–21 August.

ALFAN: Latin American Analytic Philosophy Conference, Buenos Aires, Argentina, 21–24 August.

CTF: Concept Types and Frames in Language, Cognition, and Science, Düsseldorf, 22–24 August.

AIML: Advances in Modal Logic, Copenhagen, 22–25 August.

FMIP: Munich / Groningen Summer School: Formal Methods in Philosophy, Groningen, 23–28 August.

EASLLC: International Conference and the Second East-Asian School on Logic, Language and Computation, Chongqing, China, 25–31 August.

FLINS: 10th International FLINS Conference on Uncertainty Modeling in Knowledge Engineering and Decision Making, 26–29 August.

CLIMA: 13th International Workshop on Computational Logic in Multi-Agent Systems, Montpellier, France, 27–28 August.

ECAI: 20th European Conference on Artificial Intelligence, Montpellier, France, 27–31 August.

COMPSTAT: 20th International Conference on Computational Statistics, Cyprus, 27–31 August.

COLLECTIVE INTENTIONALITY: University of Manchester, 28–31 August.

CNL: Workshop on Controlled Natural Language, Zurich, 29–31 August.

FoR&D: Conference on Frontiers of Rationality and Decision, University of Groningen, 29–31 August.

September

CSL: 21st EACSL Annual Conference on Computer Science Logic, Fontainebleau, France, 3–6 September.

ABS: Applied Bayesian Statistics School, Italy, 3–7 September.

ICLP: 28th International Conference on Logic Programming, Budapest, 4–8 September.

iKNOW12: 12th International Conference on Knowledge Management and Knowledge Technologies, Graz, Austria, 5–7 September.

LOGIC AND RELATIVITY: 1st International Conference on Logic and Relativity, Budapest, 8–12 September.

WEO-DIA: 1st Workshop on Well-founded Everyday Ontologies–Design, Implementations & Applications, Wroclaw, Poland, 9 September.

COMMA 2012: 4th International Conference on Computational Models of Argument, Vienna, Austria, 10–12 September.

LATD: Logic, Algebra and Truth Degrees, Japan, 10–14 September.

DATALOG 2.0: 2nd Workshop on the Resurgence of Datalog in Academia and Industry, Vienna, Austria, 11–14 September.

ENFA: 5th Meeting of the Portuguese Society for Analytic Philosophy, University of Minho, Braga, 13–15 September.

SIFA: 10th National Conference of the Italian Society for Analytic Philosophy, Alghero, 13–15 September.

SUM: 6th International Conference on Scalable Uncertainty Management, Marburg, Germany, 17–19 September.

ILP: 22nd International Conference on Inductive Logic Programming, Dubrovnik, 17–19 September.

GAP8: 8th Conference of the Society for Analytic Philosophy, Germany, 17–20 September.

SEM-DIAL: 16th Workshop on the Semantics and Pragmatics of Dialogue, Université Paris-Diderot, 19–21 September.

CALINTSS: Causation and Laws in the Special Science—Metaphysical Foundations, Konstanz, 21–22 September.

ENPOSS: 1st European Network for the Philosophy of the Social Sciences Conference, University of Copenhagen, 21–23 September.

ECML-PKDD: European Conference on Machine Learning and Principles and Practice of Knowledge Discovery in Databases, Bristol, UK, 24–28 September.

JELIA: 12th European Conference on Logics in Artificial Intelligence, Toulouse, 26–28 September.

CONSCIOUSNESS AND VOLITION: 1st International Krakow Conference in Cognitive Science, Krakow, Poland, 27–29 September.

COURSES AND PROGRAMMES

Courses

LI: Logic and Interactions, Winter School and Workshops, CIRM, Luminy, Marseille, France, 30 January–2 March.

ESSLLI: 24th European Summer School in Logic, Language and Information, Opole, Poland, 6–17 August.

FMIP: Munich / Groningen Summer School: Formal Methods in Philosophy, Groningen, 23–28 August.

Programmes

APHIL: MA/PhD in Analytic Philosophy, 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.

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.

MASTER PROGRAMME: Philosophy of Science, Technology and Society, Enschede, the Netherlands.

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 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 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 COGNITIVE SCIENCE AND HUMANITIES: LANGUAGE, COMMUNICATION AND ORGANIZATION: Institute for Logic, Cognition, Language, and Information, University of the Basque Country, Donostia, San Sebastian.

MRES IN METHODS AND PRACTICES OF PHILOSOPHICAL RESEARCH: Northern Institute of Philosophy, University of Aberdeen.

MSC IN APPLIED STATISTICS: Department of Economics, Mathematics and Statistics, Birkbeck, University of London.

MSC IN APPLIED STATISTICS AND DATAMINING: School of Mathematics and Statistics, University of St Andrews.

MSC IN ARTIFICIAL INTELLIGENCE: Faculty of Engineering, University of Leeds.

MA IN REASONING

An interdisciplinary programme at the
University of Kent, Canterbury, UK.

Core modules provided by Philosophy and further modules from Psychology,
Computing, Statistics, Social Policy, Law, Biosciences and History.

MSc IN COGNITIVE & DECISION SCIENCES: Psychology, University College London.

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 MATHEMATICAL LOGIC AND THE THEORY OF COMPUTATION: Mathematics, University of Manchester.

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 Sebastian).

OPEN MIND: International School of Advanced Studies in Cognitive Sciences, University of Bucharest.

PHD SCHOOL: in Statistics, Padua University.

JOBS AND STUDENTSHIPS

Jobs

POST-DOC POSITION: in Probabilistic Reasoning, Vienna University of Technology, Austria, until filled.

POST-DOC POSITIONS: in all areas of speech and language processing at the Human Language Technology Center of Excellence at Johns Hopkins University, until filled.

POST-DOC POSITION: on the project “Explanatory Reasoning: Normative and Empirical Considerations,” Tilburg Center for Logic and Philosophy of Science, until filled.

POST-DOC POSITION: in the Philosophy and History of Science and Medicine, University of Saskatchewan, deadline 1 April.

LECTURER: in History and Philosophy of Science, Department of Science and Technology Studies, University College London, deadline 23 April.

LECTURER: in Statistics, Department of Economics, Mathematics and Statistics, Birkbeck, University of London, deadline 23 April.

POST-DOC POSITION: in Philosophy of Language, Ruhr University Bochum, Germany, deadline 1 June.

Studentships

THREE DOCTORAL TRAINING GRANTS: School of Computing, Faculty of Engineering, University of Leeds, until filled.

PHD POSITION: in Bayesian Decision Theory, School of Computer Science and Statistics, Trinity College Dublin, until filled.

TWO PHD POSITIONS: in the project “Designing and Understanding Forensic Bayesian Networks with Arguments and Scenarios”, Utrecht University / University of Groningen, to be filled asap.

PHD POSITIONS: in the Statistics & Probability group, Durham University, until filled.

PHD POSITIONS: in Statistical Methodology and its Application, University College London, until filled.

PHD POSITION: in Logic and Theoretical Philosophy at the Institute for Logic, Language and Computation at the University of Amsterdam, until filled.

PHD POSITION: in Statistics, Department of Mathematics, University of Oslo, deadline 1 April.