

Nordic workshop in philosophy of science Uppsala, April 22-24, 2013
Venue: Eng4-2007

Schedule

presentation	Monday 22/4	
1	13-13.55	Paul Needham
2	14.00-14.55	Johan Gustafsson
	14.55-15.30 coffe	
3	15.30-16.25.	Marion Godman
4	16.30-17.25.	Erik Jansson Boström
	19.00 Buffé dinner	
	Tuesday 23/4	
5	9.00-9.55	Rani Lill Anjum
	coffe	
6	10.15-11.10.	Henning Strandin
7	11.15-12.10.	Sven-Ove Hansson
	lunch	
8	13.15- 14.10	Keizo Matsubara
9	14.15-15.10	Elias Saiden Nunez
	coffe	
10	15.30-16.25	Lars-Göran Johansson
11	16.30-17.25	Corrado Matta
12	17.30-18.25	Frank Zenker
	Wednesay 24/4	
13	9.00-9.55	John Cantwell
14	10.00-10. 55	Carlo Proietti
	coffe	
15	11.15-12.10	George Masterton

Participants

Fredrik Andersen	Fredrik.andersen@umb.no,	Ås
Rani Lill Anjum	Rani.anjum@umb.no	Ås
John Cantwell	jcantwell68@gmail.com ,	KTH
Marion Godman	Marion.godman@helsinki.fi	Helsingin Yliopisto
Johan Gustafsson	Johan.gustafsson@filosofi.uu.se	Uppsala Universitet
Sven-Ove Hansson	sven-ove.hansson@abe.kth.se	KTH
Brian Hepburn	bhepburn@ivs.au.dk	Århus Universitet
Erik Jansson Boström	Erik.bostrom@filosofi.uu.se	Uppsala Universitet
Lars-Göran Johansson	lars-goran.johansson @filosofi.uu.se	Uppsala Universitet
George Masterton	George.masterton@fil.lu.se	Lunds Universitet

Keizo Matsubara	keizo.matsubara@filosofi.uu.se	Uppsala Universitet
Corrado Matta	corrado.matta@edu.su.se	Stockholms Universitet
Paul Needham	Paul.needham@philosophy.su.se	Stockholms Universitet
Carlo Proietti	proietticarlo@hotmail.com	Lunds Universitet
Jorge Elias Saiden Nunez	Elias.nunez@umb.no	Ås
Henning Strandin	Henning.strandin@philosophy.su.se	Stockholms Universitet
Frank Zenker	Frank.zenker@fil.lu.se	Lunds Universitet

Abstracts

Rani Lill Anjum and Stephen Mumford

Causation in Science

Causation is essential for all empirical sciences but our philosophical understanding of it is outdated. In *Getting Causes from Powers* (OUP 2011) we offer an alternative to Hume's orthodoxy. While traditional conceptions of causation tend to abstract away from features such as context and complexity, we argue that a dispositional theory is better suited to deal with real causal situations.

John Cantwell

Actuality and the semantics of indeterminacy

I sketch a semantical framework that makes sense of the idea that we can make true claims about an indeterminate future -- a future in which it is truly indeterminate what actually will happen. I compare the framework to proposals by Lewis, Belnap and MacFarlane.

Marion Godman

Is Evil Eye Socially Learned?

The prospect of culture-bound syndromes as socially transmitted historical kinds
It has recently been argued that at least some kinds in the human sciences can be made sense of as socially transmitted historical kinds (Millikan 1999; Bach 2012: reference excluded for review). According to this view, the spatial-temporal distribution of the kinds, whose members share a rich inductive similarity, results from a historical chain of social transmission. I here assess how the proposal fares in the case of culture-bound syndromes. I first assess the prospects for cultural evolution theory in supporting and developing the notion of social transmission underlying culture-bound syndromes. I then discuss two problems that indicate a crucial limitation to the current historical kind approach to culturebound syndromes in particular and probably human kinds more generally. Most culture-bound syndromes are thought to depend in some way on culturally

transmitted system of beliefs about the distinctive physical and psychological features of each syndrome as well as its causes. For example in northeast Brazil, the syndrome of shock sickness is understood as a state of emotional vulnerability caused by one or more traumatizing events. This condition can in turn lead to a different syndrome, nerves (Rebhun 1994). The folk wisdom in this region thus hypothesizes that the syndromes are caused by the 'sickening power' of negative emotions. Moreover since many culture-bound syndromes almost exclusively affect one gender, it has been suggested that specific cultures also stipulates gender-specific display rules for emotional expression and appraisal (Guarnaccia and Rogler 1999).

In particular two concepts from culture evolution theory literature seem helpful in making sense of how these syndromes might be construed as socially transmitted historical kinds: First, culturally specific group approval of emotional display rules (and disapproval for deviations) can account for the historically stable and reliable transmission as well as the decreased cost of testing 'learned' behavior (Castro et al 2010). This is corroborated by research that suggests that culture-bound syndromes are to some extent beneficial for individuals – provided the benefits are reaped within a specific social or cultural niche (Prince 1985). Second, the concept of cultural niche construction, where humans actively engineer their environments in ways that allow for cultural inheritance, provides a better understanding of how social transmission gets off the ground in the first place (Sterelny 2003).

However, two important issues stand in the way of a straightforward socialtransmission-story for culture-bound syndromes. First, although the proposed conditions and vehicles for social transmission might explain the historical grounds of the beliefs about the syndromes, they arguably do a less good job as explanations of the syndromes themselves. That is, although the rules for the display and appraisal might be explained by group approval/disapproval, it is less clear that the experience and physical symptoms associated with the particular syndromes are learned by social transmission (Prince & Tchong-Laroche 1987). In fact merely appealing to the social transmission of belief systems indicates a rather undesirable anti-realism toward culture-bound syndromes.

Second, whether or not culture-bound syndromes themselves are social transmitted, the transmission may not be sufficiently vertical or culture-bound for the syndromes to constitute a historical lineage. There is, for instance, much evidence that an alleged culturebound syndrome such as, evil eye, is a result of convergent (cultural) evolution since several

cultures have adopted the belief that envy and anger of others has the power to victimize the sufferer (Dundes 1992). Another example is the dhat syndrome (semen-loss anxiety), which until recently was thought to be a “neurosis of the Orient”, but which different sources have now indicated occurs amongst men in Western society, at least until the 20th century (Sumathipala et al 2004). These cases also accord with the recommendations of the current DSM IV (soon to be replaced by DSM V). Although the DSM IV suggests that the importance of cultural identity and cultural variation should be taken into account more generally in the case of syndromes and disorders and has reserved the category of culturalbound

syndromes, it also expresses skepticism about the scope for genuine culture-bound syndromes once cultural variation is taken into account. These cases and considerations hence indicate that the historical kind framework would be of limited value as either the beliefs about the syndrome are often horizontally transmitted across cultures or the syndrome arises independently in different culture due to universal, physical or environmental, circumstances.

The upshot is then that to the extent that the historical kind account aims to explain the inductive similarity and spatial-temporal distribution of many of the “culture-bound” syndromes, one will need to not only take social levels, but also genetic and developmental levels of transmission into account.

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Johan Gustafsson

ON HISTORICIZING EPISTEMOLOGY

"Historical epistemology" does not constitute a homogeneous tradition of thought, but can rather be seen as a set of inquiries that share the common theme of trying to address epistemological questions from a historical perspective. Schematically, this field of investigation could be divided into: (1) a form of history of science that studies, among other things, epistemic concepts, epistemic objects and the dynamic development of scientific theories; (2) a philosophical approach that seeks to effect a thoroughgoing historicization of the theory of knowledge. Such an approach can be taken to entail a relativizing of claims to knowledge, but does not have to be. In this paper, we will focus on the philosophical strand.

Historicizing science, for most proponents of this approach, would include analyzing the criteria by which we judge the truth-content of scientific theories as historically variable. Within this tradition, one sees repeated attempts to displace the constitutive role of the subject of knowledge – which was supposed to account for its possibility – and replace it with some more fundamental structure. Within a historical analysis with such motives, concepts are formed that are meant to encompass this underlying entity: *thought style* (Ludwik Fleck), *paradigm* (Thomas S. Kuhn), *discursive practice* (Michel Foucault), etc. Whereas the turn to history that we encounter in Fleck, Kuhn and Foucault was part of a critique of abstract and speculative epistemological systems, it

would later on – and to some extent in the authors themselves – give rise to a new form of “speculative” epistemology. In analyzing this tendency, we will focus in on Foucault. In Foucault’s work, the subject of knowledge is replaced with “practices” or “apparatuses” that constitute subjects and objects. In this “nominalist” critique, the categories of subject and object are retained only as part of a framework for the analysis of practices. There is a tendency, though, in some interpreters of Foucault, to read the conceptual framework of discursive analysis as revealing a new form of universal mechanism for the constitution of subjects and objects in historical space. The works of Paul Veyne and Gilles Deleuze are used to exemplify this tendency, and it is shown how their readings give rise to a new form of universalist perspective, which is as much a metaphysical fiction as that which it was meant to replace. This perspective emerges out of the disengagement of Foucault’s conceptual framework from the historical analysis that it was designed to serve.

Sven Ove Hansson

Why do we perform experiments?

Experiments are a specific type of observations, characterized by manipulation of the object of observation in order to obtain a predictable and therefore repeatable outcome. What is the justification for performing experiments rather than other types of observations? In order to answer that question it is useful to distinguish between two types of experiments: Those aiming at knowledge of how the world can be manipulated to obtain certain effects (technological and agricultural experiments, medical trials etc) and those aiming at knowledge about the workings of the world. This contribution explains how these two types of experiments differ in terms of justification and interpretation.

Erik Jansson Boström

Max Weber’s two concepts of the meaning of science

In *Science as a profession and vocation* Max Weber discusses the meaning (Sinn) of science. I will argue that Weber in fact is talking about two different aspects of science, which is difficult to see. In order to make Weber’s points clearer I will introduce a distinction between “subjective” and “objective” meaning. The question of the objective meaning of science is a

question of *what* we do when we do science while the question of the subjective meaning of science is a question of *why* we do science. In *Economy and Society* Weber uses this concept of subjective meaning (subjektiv gemeinten Sinn) when he develops his sociology of motives for actions in general. My point is that Weber, as a part of his analysis in *Science as a profession and vocation* of motives for doing science, also develops an idea of what it means to do science in relation to the rationalization process of western society in general.

Lars-Göran Johansson 12

Physical Laws and Natural Necessity

We distinguish between laws and accidental generalisations; both types are expressed by universally generalized conditionals and both are believed to be true, but there is a difference still. There has been an intense debate about this, many different proposals have been made, but none has achieved general acceptance.

Most philosophers with a metaphysical inclination believe that laws are necessary in some sense to be further explained. Being an empiricist, I reject all kinds of metaphysics, although I strongly feel the urge to say that at least the fundamental laws of physics are necessary. In this talk I will propose that physical laws are necessary *de dicto*, that we should treat the expression 'necessary' not as a modal operator but as a semantic predicate and that this does not entail any metaphysical commitments and also explain our reasons to say about physical laws, at least some of them that they are necessary *de dicto*. I will discuss in some detail the reasons why we say about the fundamental laws of classical mechanics and electromagnetism that they are necessary.

George Masterton

What to do with a forecast?

In the literature one finds two non-equivalent responses to forecasts; deference and updating. Herein it is demonstrated that, under certain conditions, both responses are entirely determined by one's beliefs as regards the calibration of the forecaster. Further it is argued that the choice as to whether to defer to, or update on, a forecast is determined by the aim of the recipient of that forecast. If the aim of the recipient is to match their credence with the prevailing objective chances, they should defer to the forecast; if it is to maximise the veritistic value of their beliefs, they should update on the forecast.

Keizo Matsubara

On the semantics of modern theories of physics

What do theories of modern physics tell us about reality? How are we supposed to understand various claims and theoretical statements and to what extent does physics provide us with insights in questions regarding ontology. After the fall of logical positivism it is often stated, by scientific realists and anti-realists alike, that scientific theories are to be understood "literally". This is in my view a mistake and I will argue that we should not understand our theories literally, instead I will advocate the use of a form of empiricist semantics. This empiricist semantics is however, not as strict as the ones traditionally associated with logical positivism. Of special importance is to elucidate the relation between the mathematical formalism of a theory and its physical content. Another important point is that while many statements in physics should not be understood literally, they play an important heuristic and pragmatic function.

Corrado Matta

**Representation and Science Learning,
An Ontic Structural Realist Approach**

Summary:

The aim of my talk is to provide an ontic structural interpretation of science learning.

Ontic structural realism (OSR) is intended in my talk as the variety of structural realism that argues that the ontology of successful scientific theories is an ontology of relations.

In my talk I apply OSR on two levels. The first is the level of scientific theories intended as the content of science learning; the second level is that of theories of learning intended as scientific theories.

First of all, I provide two examples of theoretical framework in educational research: the constructivist and the social-semiotic. The first considers learning as the conceptual development of a mental representation, the second as the successful acquisition of the ability of using a symbolic system in a fruitful way, which is realized in the production of symbolic representations. I analyze the use of the term representation in light of the discussion concerning scientific representation that has characterized the debate in the analytic philosophy of science in the last decade, and in particular concerning the problem of what defines a correct representation of a target system.

The discussion of scientific representation in science learning enables us to identify the philosophical sources of the widespread skepticism in science education about the very idea of the *target of a scientific representation* in science learning. As I argue, Kuhnian and Wittgensteinian arguments have led educational researchers to deny that the development by the students of objectively correct scientific representations constitutes the aim of science learning. This has implied that many theories of science learning have embraced relativistic or skeptical positions such as the sociology of scientific knowledge. My claim is that these relativistic approaches to science, despite their great merits for science education, result in a picture of science learning as socialization into a practice or as indoctrination.

In contrast, I propose an alternative interpretation of theories of science learning, based on Ladyman and Ross's framework described in (Ladyman et al.:2007). As I argue, this alternative framework has the main reward of reintroducing scientific realism into the scope of science learning. In this part of my talk I focus on the thesis that objectively correct representations are defined as representations that locate a *real pattern*. The

keystone of this discussion is the problem of the projectibility of social scientific hypotheses.

Thus, OSR is not only applied as framework for interpreting the content of science learning (i. e. the representations that are mentally or symbolically developed by the learner), implying that these should be interpreted as pattern-locators. OSR is applied to the theories of learning *qua* scientific theories; that is, science learning is considered as a social phenomenon investigated by theories of learning. These latter, in order to account for (non-relativist) successful science learning, must be interpreted within the framework of OSR.

I conclude that OSR implies both the rewards of the pragmatic and social approaches to scientific theories while in the same time it allows us to argue for the possibility of objectively correct scientific representations. Furthermore, my discussion provides further ground for defending structural scientific realism in general, in that, as opposed to anti-realist theories of science, it is able to provide a unificatory account of science and science learning.

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Paul Needham

Moderate Realism: Resisting the Extremes of Preservation Realism

The debate between realists and antirealists is too often taken to extremes. Critics of Laudan's pessimistic induction and van Fraassen's constructive empiricism overkill with Putnam's miracle argument, the associated doctrine of preservation realism this has inspired and Psillos has pursued and the idea of inference to the best explanation. Moderate realism is quite sufficient. I have interpreted Duhem as a moderate realist and this seems an eminently reasonable position to me. Advocates of realism often say that theirs is the natural interpretation of the views scientists ordinarily express. I think this common-sense realism is deeply at odds with the efforts of preservation realists to save abandoned terms of science by endowing these bygone relics with a putative reference on the strength of an application of the causal so-called theory of reference. Negative existential claims are as important as positive existential claims, which would lose much of their force without the contrast. The robust realism of common-sense science rejects the existence of caloric and the ether, which makes the claim that atoms and molecules exist all the stronger.

But surely "atom" and "molecule" are terms that have "preserved their reference" (they are predicates whose extension or meaning is fixed), so that what we mean by them is what at any rate Dalton, if not Democritus, meant by them. Perhaps "molecule" calls for some qualification, its introduction postdating Dalton. But surely these terms have retained their meaning since they were introduced into science in the 19th century. Kripke's and Putnam's dabblings with extant terms of science such as "water" has led them to suggest that their extension has remained fixed. Does their line of argument apply to "atom" and "molecule" and justify any intuitions of preservation? I think that what is understood to be water today is not what Aristotle, or even chemists in the late 18th century, understood by the term. It seems to me that much the same holds for "atom" and "molecule", or at the very least, only a very carefully qualified account of the preservation of meaning is tenable, and this needs to be properly formulated.

Carlo Proietti

Fitch Paradox. Reformulation strategies for its solution.

Fitch-Church paradox (1963) consists of a straightforward formal derivation in modal logic to the effect that (a) $p \rightarrow \Diamond Kp$ entails (b) $p \rightarrow Kp$. It was only fifteen years after Fitch's article

was published that W.D. Hart interpreted this “unjustly neglected logical gem” as a genuine paradox for *verificationism*. The reasoning is simple: if “All truths are knowable” [(a)] entails that “All truths are known” [(b)] then a basic verificationist claim entails a blatant absurdity. Hence, verificationism is debatable on a simply logical ground.

A number of different logical solutions has since then been presented to counter this argument or show its limits. These solutions mostly fall into three categories. (1) *Restriction strategies*, that limit the universal scope of (a), viz. – in logical words – instances of (a) should not be substitution free. (2) *Weakening strategies*, which block the derivation of the paradox by invalidating some inference rules of classical logic. (3) *Reformulation strategies*, that deny that $p \rightarrow \Diamond Kp$ is indeed an adequate formal rendering of the verificationist thesis and claim that the apparent paradox only follows from propositional (bi)modal logic being too weak to correctly express such thesis.

Here I intend to investigate the rationale behind solutions of kind (3) and to open the formulation of the verification thesis to a wider range of possibilities allowed by first order modal logic. Secondly, I intend to analyze the more frequent objections to reformulation, paying particular attention to a “metaphysical” objection, according to which non-actual knowledge of actual truths – that solutions of kind (3) seem to entail – is a problematic notion.

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Eilas Saiden Nunez:

“The Physics of Agency: a non –reductive account of the Thermodynamic Structure of Simple Organisms”

Henning Strandin

Causal semantics (without a world similarity ordering)

This talk will be about formally interpreting causal claims. I will begin by problematizing Lewis's use of a relative, overall similarity ordering in his semantic theory of causation. I will go on to present an alternative (a work in progress) that takes a language and natural law--that is, a conceptual and nomic structure--as the model on which to define truth conditions for causal claims. I will then point to what I take to be important benefits of such a model, mainly the absence of a supposedly primitive similarity ordering, the way the logical role of background conditions can be analyzed, and how we can understand the nomic structure to be generated by theory. After presenting some of the important truth conditions on causal claims in terms of this model, I will say something very briefly about the role of context, and about what this choice of model suggests with respect to probabilities and indeterminism.

Frank Zenker

Reconstructing the Development of the Mole Concept

We introduce Gärdenfors's theory of conceptual spaces, and outline how it can be applied to theory dynamical processes in the empirical sciences. Furthermore, we give a reconstruction of the diachronic development of the physical quantity ('amount of substance') connected to the measuring unit 1 mole used in measurements of chemical reactions. *Vis-à-vis* this reconstruction, we discuss several issues—some of which are current, insofar as changing the standard definition of the mole is under discussion. Crucially, we point out that two ontologically rivaling interpretations of the mole are reflected in the two distinct metrics (namely that of the positive real numbers, R^+ , and that of the positive integers, Z^+).