

The Book of Abstracts

Graduate Conference in Theoretical Philosophy

University of Groningen

18-20 April 2013

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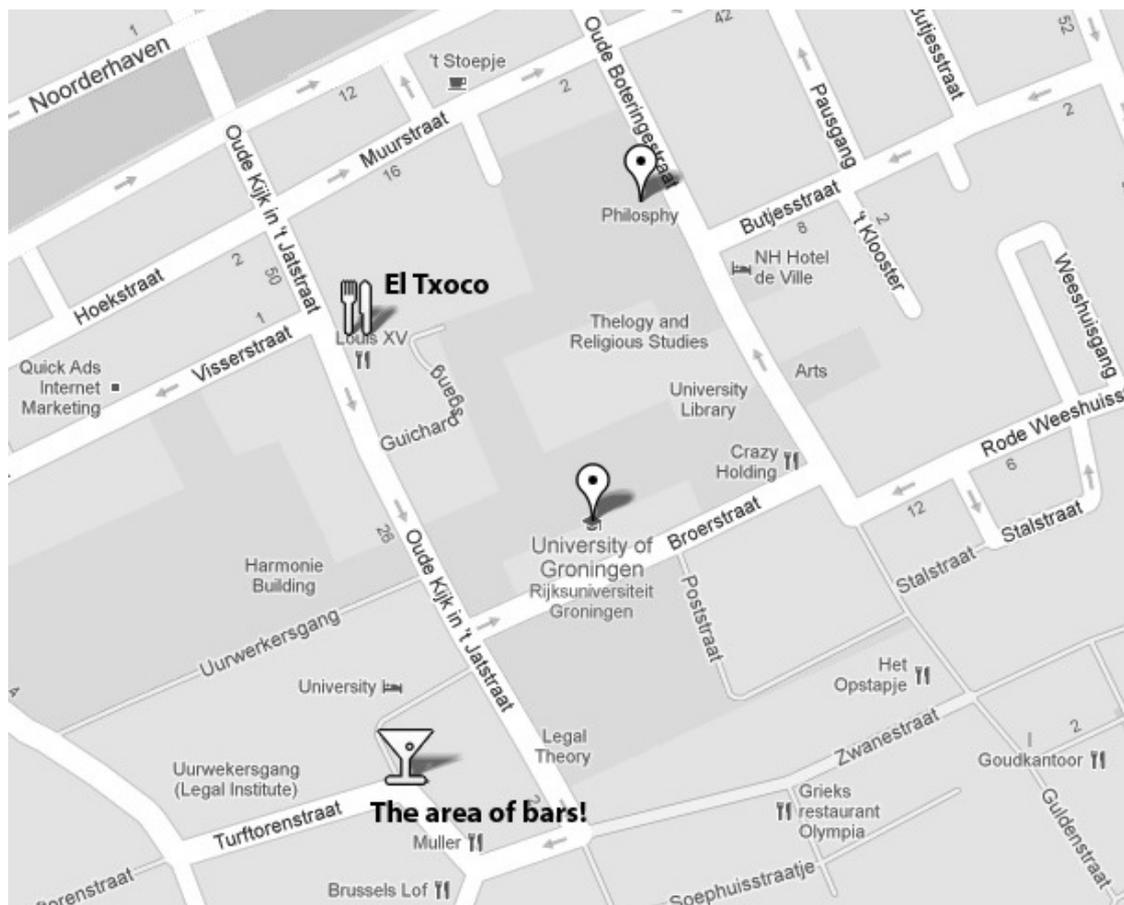
Academy Building
(Main building of the University of Groningen)
Broerstraat 5

SATURDAY

Faculty of Philosophy
Oude Boteringestraat 52

Conference dinner: FRIDAY, 6PM

Restaurant El Txoko
Oude Kijk in 't Jatstraat 53



Keynote Lectures

Patrick Blackburn

ON THE CHARACTER OF TEMPORAL INDEXICALS

In this talk (which is based on joint work with Klaus Frovin Jørgensen) I re-examine early work on the semantics of temporal indexicals through the lens of modern hybrid logic. Modern hybrid logic is a natural tool for handling many varieties of temporal reference, and as I shall show, this suitability extends even to temporal indexicals such as Now, Yesterday, Today and Tomorrow. The key idea is to return to the classic work of Hans Kamp and David Kaplan. However, instead of exploiting their two-dimensional semantics using additional modalities, we make use of character-encoding propositional symbols instead. One of these symbols (namely Now) enables us to unlock the logic of temporal indexicals in a simple and general way.

I won't be assuming much knowledge of logic or linguistics in this talk. Indeed, a very modest acquaintance with logical notations and notions will be more than enough to follow this talk. My aim will be to present a simple overview of the issues and to explain (as clearly as possible) just why it is that hybrid logic gives us simple solutions to some rather tricky issues.

John Dupré

THE DISORDER OF THINGS REVISITED

Twenty years ago I published *The Disorder of Things*, which attacked a still popular view of science as a potentially unified system of laws, ultimately derivable from the fundamental laws of physics. Instead, I argued that we should see science as a loosely connected set of practices, each with its own distinctive set of concepts, models, and methods. This kind of picture has gained widespread, though certainly not universal, acceptance among philosophers of science over the intervening years. I also argued that the disunified character of science reflected a fundamental lack of order in the world it aimed to describe.

In this talk I will review some of the themes of that book, but also show how, perhaps surprisingly, spectacular advances in parts of science that are often conceived as highly reductionistic, such as genomics, molecular biology, and microbiology, have actually lent further support to central tenets of disunified science, including conceptual pluralism and emergence. Finally, I shall suggest that these developments, and pluralism more generally, are best understood in terms of an underlying ontology that sees processes rather than things as fundamental.

Hannes Leitgeb

THE HUMEAN THESIS ON BELIEF (AND ITS EQUIVALENTS)

What should a joint theory of rational belief and rational degrees of belief look like? While the former concept will contribute principles of doxastic logic, the latter will contribute principles of probability theory, but how can we make sense of their interaction? And how can we avoid the Lottery paradox and related paradoxes? I will present three different approaches of how to answer these questions: the first one is an explication of what I call the Humean thesis of belief; the second one is a combination of doxastic logic with the right-to-left direction of what is called the Lockean thesis on belief in the literature; the third one puts together AGM belief revision and the left-to-right direction of the Lockean thesis (formulated for conditional belief). As it happens, all of them will ultimately justify one and the same joint theory of belief and degrees of belief according to which belief corresponds to stably high degree of belief. (Actually, there is a fourth “accuracy” approach from which once again the same theory of belief follows, but we will not have time to deal with it.)

Ruth Millikan

NEW THOUGHTS ON INDEXICALS AND DEMONSTRATIVES

A new way of describing the truth conditional semantics of indexicals and demonstratives is presented according to which they are anaphors, their antecedents being self-signs. This requires an untraditional way of looking at conventional signs and at the way language is embedded in the world.

Contributed Talks

EPISTEMOLOGY

Alba Amilburu

NATURAL KIND THINKING IN PHILOSOPHY OF SCIENCE: A META-THEORETICAL ANALYSIS

The notion of “natural kind” plays an important role both in philosophy of science for understanding grouping practices, and in scientific practice as a methodological tool, because it facilitates a comparison of different grouping strategies. In order to investigate the contribution of this philosophical concept, we need first to clarify what makes a kind a *natural* kind. In this paper I argue that the notion of “natural kind” is ambiguous: a fundamental disagreement concerns how philosophers understand naturalness. Thus, a meta-theoretical analysis—i.e., an interpretation of the different theoretical accounts of natural kinds that conform the current debate—is a necessary step to clarify the uses and meaning of the “natural kind” concept.

Recently, Reydon (2010) presented an interesting meta-theoretical analysis of the current debate on natural kinds. He identifies two lines of work or traditions, called by Reydon, a “metaphysical approach” and an “epistemological approach”. In the first approach, natural kinds are conceived as real kinds that exist in nature independently of observation and human reasoning. In this classical tradition, the theoreticians (for example, Ellis 2001) face issues such as how we should conceive and develop the idea of natural kindhood, or which sorts of real essence correspond to the different types of natural kind that exist. In the “epistemological approach” natural kinds are considered as groups of particulars that are made by us with the purpose of being useful in a certain context. The main concern for authors close to this line of work (for example, Boyd 1991) is to understand what makes certain groupings of things suitable for featuring successfully in explanation and prediction.

In this paper, first, I examine critically Reydon’s proposal pointing out certain aspects of his analysis that should be improved, and second, I present a different proposal, which includes and develops some aspects of Reydon’s analysis but introduces new elements in order to overcome those limitations. In particular, I argue that the distinction between a metaphysical and an epistemological line of work is better understood as a distinction between an “essentialist approach” where *natural kindhood* is metaphysically clearly defined in terms of essentialism, and a “non-essentialist approach” in which membership criterion is (at first) metaphysically indefinite because it is relative to an epistemic contribution. This second line of work is an approach that it is not restricted to some unitary metaphysical formulation of what natural kindhood is. Authors close to this second line of work understand scientific grouping practices and group concepts as a decision-making calculus over where and how to draw and describe kind boundaries, as pointed out by MacLeod (2010).

I argue that this new proposal is best suited for explaining in what sense philosophers disagree on how to understand the controversial idea of “naturalness”, and for understanding the peculiarities and differences among different theoretical accounts of natural kinds.

Coos Engelsma

FOUNDATIONALISM AND THE ARBITRARINESS OBJECTION

The epistemic regress problem is a problem arising in discussions about the justification of beliefs. Some of our beliefs are justified by reference to other beliefs. However, what about the beliefs that they are justified by? Are those justified by still other beliefs? If so, how about the beliefs that justify them? If they are also justified by other beliefs, does this imply that we must have an infinite number of beliefs? Or could there also be beliefs that do not depend for their justification on other beliefs?

Clearly the most influential answer to this regress problem is the one given by epistemological foundationalism. According to foundationalism, our mediately justified beliefs depend for their justification on other beliefs, which may in turn depend on still other beliefs for their justification, but the regress must terminate in beliefs that are *immediately* justified, i.e., in beliefs that do not need other beliefs in order to be justified.

An important objection to foundationalism, most prominently raised in some recent papers by Peter Klein, says that it supports justificatory *arbitrariness*. Since foundationalism allows for some beliefs justifying other beliefs but themselves being justified even when no beliefs serve to justify them, so the argument goes, it allows for beliefs being held in an arbitrary fashion.

In my talk, it is this arbitrariness objection that I will evaluate. First, I will draw an often neglected distinction between foundationalism as a response to *dialectical* epistemic regresses and foundationalism as a response to *structural* epistemic regresses. Then, employing Klein’s concept of epistemic arbitrariness, I will assess the arbitrariness objection with regard to both dialectical foundationalism and structural foundationalism. After considering some accounts of dialectical foundationalism to be found in the literature, I will argue that Klein’s objection to this type of foundationalism does not succeed. However, with regard to structural foundationalism I will argue that the objection seems more promising. Despite several foundationalist attempts to prove otherwise, I will argue that it is very hard, if not impossible, to construct a version of this type of foundationalism that does not at least allow for some kind of epistemic arbitrariness.

Jie Gao

RATIONAL ACTION WITHOUT KNOWLEDGE (AND VICE VERSA)

It has been recently argued that knowledge is the norm of practical reasoning. A way of formulating this norm is the following bi-conditional: it is rational to treat p as a reason for acting if and only if you know that p . In my talk, I provide four new counterexamples to this claim. I show cases of rational action: i) performed in sceptical scenarios in which knowledge is absent, ii) based on scientific hypotheses, iii) motivated by imagining and iv) grounded in acceptance. These four types of cases constitute objections to the left-to-right direction of the bi-conditional: it is shown that it is appropriate for a subject to act as if p even if she does not believe (and thus does not know) that p . Furthermore, I suggest that the last two are also counterexamples to the right-to-left direction of the bi-conditional: it is shown that one can know that p but that it is inappropriate for her to treat p as a reason for acting. In the last part of the talk, I explore whether any other version of the knowledge norm could still be maintained and conclude that none of them is worth preserving.

Harmen Ghijsen

SENSATIONS, SEEMINGS, AND BELIEFS: THREE'S A CROWD

The distinction between perceptual experience and belief has by now been commonly accepted. One of the strongest arguments for this distinction comes from the fact that one can have an experience that p without having the belief that p . For instance, someone who is familiar with the Müller-Lyer illusion will no longer believe that the two lines in this illusion are of a different length, even if he still experiences them as having different lengths. However, some philosophers have argued that this twofold distinction between perceptual experience and belief is not sufficiently fine-grained to deal with some difficult issues in cognitive science and epistemology. Instead, they propose a threefold distinction between sensations, seemings, and beliefs, which supposedly (1) explains the cognitive phenomena of blindsight and associative agnosia, and (2) strengthens the case for dogmatism in epistemology by solving Ernest Sosa's version of the problem of the speckled hen. The distinction between seemings and sensations is important for (1), as patients with blindsight are supposed to have seemings but not sensations, and patients with associative agnosia are supposed to have sensations but not all relevant seemings. The distinction is important for (2), as it is maintained that only seemings provide justification for belief, and relevant seemings are absent in the case of seeing a many-speckled hen.

In this paper I argue that it is doubtful that sensations, defined as 'mental pictures', actually exist, and that seemings are phenomenologically and epistemologically redundant to perceptual experiences — even by the dogmatist's own lights. Seemings do not have any role to play in cases of blindsight, as subjects take themselves to be randomly guessing. Their absence is also not needed to explain cases of associative agnosia, since the presence of impoverished experiences is sufficient for that. This undermines (1) as a reason for introducing distinct seemings into the cognitive mix. Against (2) I argue that there are better solutions to the problem of the speckled hen, one of which is Sosa's own abilities-based approach, and another of which highlights the importance of a correct understanding of perceptual experience as a high-level representation of the environment. Once one allows that a perceptual experience can represent a hen as having many speckles without representing it as having a specific amount of speckles, then the speckled hen problem no longer arises. Of course this option only appears to be open if one acknowledges that perceptual experiences are not like mental pictures, but this gives us all the more reason to reject the threefold distinction between sensation, seeming, and belief.

Pieter van der Kolk

THE RATIONALITY OF PERMISSIVENESS

Assuming that evidence is a determinant of doxastic rationality, how permissive (or restrictive) are the constraints that evidence imposes on what it is rational to believe? Does evidence always completely determine what it is rational to believe? Or does evidence sometimes leave some (as Kelly (*forthcoming*, 1) puts it) *slack* for different but equally rational doxastic attitudes? A recent suggestion in this matter is the *Uniqueness Thesis* (White 2005, 445; Feldman 2007, 205; Matheson 2011, 360):

(UT): Given a body of evidence, there can be only one doxastic attitude that it is rational to adopt toward a proposition.

The denial of this suggestion has been called Epistemic Permissiveness (White 2005, 445):

(EP): Given a body of evidence, there can be more than one doxastic attitude that it is rational to adopt toward a proposition.

In this paper I will examine a seminal argument for (UT), which aims to demonstrate that, given the supposed nature of evidential support and doxastic rationality, there can be only one full belief state that it is rational for an agent to adopt regarding a proposition, given her evidential situation. I will first present and unpack this (somewhat broadstroke) argument, and tentatively fill in some of the omitted details. I will then confront this argument with the *problem of underdetermination*, which precisely raises the question against (UT) whether indeed there is such a uniquely rational belief state, given a piece of (conicting) evidence. This problem conveys that the assumptions behind the argument for (UT) are incomplete, and require precisification. I will then investigate some possible precisifications, borrowing primarily from the wealth of ideas and models about this issue in the more formal areas of epistemology, particularly *Bayesian Confirmation Theory* and *Belief Revision Theory*. This investigation suggests that the argument for (UT), as it stands, is inconclusive, and, furthermore, that there are good reasons to think that, contra (UT), evidence does not always completely determine what it is rational to believe.

I will end with some concluding remarks on the implications of this result for the recent debate about *peer disagreement*, which is closely connected to this paper's issue of whether evidence uniquely determines rational belief.

References

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Jakob Koscholke

EXPLANATORY COHERENCE – THE PROBABILISTIC WAY?

Recent formal approaches to coherence can be divided into two groups: probabilistic and connectionist. Probabilistic approaches – as proposed by Shogenji, Glass, Olsson, Douven & Meijs and others – are based on probability theory and allow us to calculate the degree of coherence of a set of proposition, given a probability distribution over these propositions. Connectionist approaches, also known as theory of explanatory coherence – as proposed by Thagard & Verbeurgt and further developed by Schoch – are based on methods used in computational modelling of neural networks. Here, binary coherence and incoherence relations based on explanatory, inferential, analogical or inconsistency relations between propositions are used to calculate the degree of acceptability of each proposition.

In my presentation I am going to discuss the differences between both accounts and show that they have a similar underlying structure. This fact allows for a comparison of both approaches with respect to their computational results concerning the degree of coherence of a set of propositions. This comparison in turn allows us to answer the question which of both approaches is more adequate to capture the concept of coherence.

Michael Schippers

COHERENCE AND (LIKENESS TO) TRUTH

Should coherence among theories be considered a scientific virtue in the sense that coherence implies verisimilitude? Are more coherent scientific theories more verisimilar? In this paper I will show that these

questions, even if construed cautiously in a *ceteris paribus* sense, must be answered in the negative. To do so, I introduce a Bayesian framework for comparing scientific theories and establish an impossibility result to the effect that more coherence among theories does *not* invariably lead to more verisimilitude, *ceteris paribus*. This result reinforces the impossibility results from the field of Bayesian epistemology (Bovens & Hartmann 2003, Olsson 2005).

In a second step I will argue that in order to understand the utility of coherence in the context of theory assessment, we are well advised to change the focus from coherence among theories to a notion of coherence being a relation between scientific theories on the one hand and observational data on the other. In this regard, I show that the class of *contrast coherence measures* I introduce are a useful guide in order to assess the virtue of scientific theories. In this sense, theories that cohere better with the observational data are indeed more verisimilar. This latter notion of verisimilitude, however, is an *epistemic* notion relativized to a scientific community.

Sidra Shahid

RELIGIOUS BELIEF AND TRANSCENDENTAL CERTAINTIES

My talk is concerned with religious belief and its relation to our epistemic and practical engagements. Is religious belief outside the scope of our rational and epistemic practices? If so, how can the tenets of such belief be revised or criticized? By examining Ludwig Wittgenstein's dispersed remarks on religious belief in connection with his last work *On Certainty*, I attempt to provide a more robust characterization of the linguistic and conceptual nature of religious beliefs and their relation to our (epistemic) practices.

While previous studies have addressed Wittgenstein's conception of religious belief with respect to its adherence to fideism or expressivism (Nielson, Barrett), few studies have systematically or extensively broached the relation between religious belief and certainties. There are indeed remarkable similarities between certainties and Wittgenstein's characterization of religious belief. Certainties are the framework presuppositions that form the bedrock against which the practices of a community are tested and established. Like religious beliefs, certainties exist outside the ambit of ordinary epistemic procedures such as doubting and justifying. However, there are also crucial differences. For instance, while certainties form the edifice of our broader epistemic and practical engagements, religious belief carries an existential facet that certainties lack. Ultimately, an examination of the connection between religious belief and certainties can illuminate both the nature and grounds of (epistemic) practices and religious attitudes.

I propose an examination of religious belief and certainty through the interpretation of Wittgensteinian certainties as transcendental (Rudd, Lear, William), arguing against a conflation of certainty and religious belief, I claim that the same certainties that enable ordinary belief also enable religious belief. Motivating my account on a transcendental reading of certainties, I argue that religious belief, while counting as an extraordinary doxastic attitude, can nonetheless be criticized on the basis of shared standards (Kusch). However, as Martin Kusch has argued, religious beliefs are incoherent to the non-believer and vice versa, what Bernard Williams calls a notional (as opposed to real) confrontation. The non-believer, in other words cannot "retain their hold" of the reality on the other side, and vice versa. With this picture in view, my talk attempts to provide an understanding of the terms of the debate between the believer and the non-believer.

Tom Sterkenburg

SOLOMONOFF'S THEORY OF PREDICTION

R.J. Solomonoff's theory of Prediction assembles notions from information theory, confirmation theory and computability theory into the specification of a supposedly all-encompassing objective method of

prediction. The theory has been the subject of both general neglect and occasional passionate promotion, but of very little serious philosophical reflection. In this talk, I present a more balanced philosophical appraisal of Solomonoff's theory.

Specifically, I propose an interpretation of Solomonoff Prediction in terms of the unifying principle of Universality. The essence of the theory is the statement, to be read in the context of the philosophical problem of induction, that in a universal setting, there exist universal predictors. The universality of the setting is grounded in the central assumption of computability: while this assumption is not uncontroversial as a constraint on the world, I argue that it is hardly a constraint at all if we restrict attention to all possible competing prediction methods.

Sander Verhaegh

QUINE'S ARGUMENT FROM DESPAIR

According to Quine, naturalism can be characterized negatively as the abandonment of a "first philosophy" prior to science. The Quinean naturalist recognizes that we ought to repudiate "the Cartesian dream of a foundation for scientific certainty firmer than scientific method itself" (1990, 19). Where traditional epistemology aspired to contain science by attempting to "construct it somehow from sense data", the naturalist rather sees epistemology as "contained in natural science" (Quine 1969, 83). But what exactly are Quine's reasons for rejecting first philosophy? Why, in other words, does Quine believe that he is bound to evaluate our epistemic practices from within, that he "is the busy sailor adrift on Neurath's boat" (1981, 72)? In the present paper, I will examine Quine's ideas about first philosophy and reconstruct his argument for dismissing the project.

Prima facie, Quine's argument against first philosophy seems to be pretty straightforward: we ought to abandon traditional epistemology because, historically, all attempts to ground our beliefs have failed. In his influential "Epistemology Naturalized", for example, Quine divides traditional epistemology into a doctrinal and a conceptual program and argues that neither of them can be carried out satisfactorily. In particular, he criticizes Carnap's project of rational reconstruction, arguing that it fails to "offer any key to *translating* the sentences of science into terms of observation, logic, and set theory" (1969, 77). As an alternative to these projects, Quine proposes his naturalized epistemology, the study of how theory and evidence are *actually* related: "If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology." (ibid., 78).

Let me call this the *standard conception* of Quine's argument against first philosophy. In the standard conception, Quine's argument is construed as a conditional argument: we can legitimately take on a naturalized epistemology only when we have demonstrated that we ought to "stop dreaming of deducing science from sense data" (1969, 84) and that we ought to "despair of being able to define theoretical terms generally in terms of phenomena" (1981, 72). The Quinean naturalist is not a 'busy sailor' from birth, but "someone who later elects to enlist, perhaps in reaction to some deep disappointment" (Maddy 2007, 85). Quine's argument, in short, is pictured as an *argument from despair*.

The standard conception is widespread among both Quine scholars and critics. In this paper, however, I will argue that the standard conception is mistaken. I will show that Quine's argument against the first philosopher is considerably stronger than the standard conception suggests. In his work both before and after "Epistemology Naturalized", Quine does not abandon traditional epistemology out of despair but because the project is flawed from the beginning. According to Quine, it is a mistake to believe that one can develop a self-sufficient sensory-language, epistemologically prior to our best scientific theories of the world. The first philosopher does not fail because he aims at Cartesian certainty, but because he presupposes that he can adopt some science-independent perspective. I will argue, in short, that "Epistemology Naturalized" misrepresents the strength of Quine's position. Naturalism is not merely the

“more sensible” option (1969, 78), it is the only option out there.

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Isabel Verkes

FALSE KNOWLEDGE, THE CASE OF EPISTEMIC CONTEXTUALISM

What is supposed to be knowledge in one context may be ignorance in another context. When someone who is cooking soup states that a tomato is a kind of vegetable,¹ we would probably consider him to know what a tomato is. But if he had made this statement during a scientific classification of biological kinds, we would say he does not know what a tomato is, as it is scientifically a fruit. Why is it that at one moment someone can be supposed to know, but when circumstances appear to be different, we may find he does not know?

My presentation will be about this intuition, which implies that in so far as epistemic standards may be different, our knowledge ascriptions are context-dependent. This idea corresponds to a collection of views called epistemic contextualism, which is advocated by authors like David Lewis, Keith DeRose and Stewart Cohen. According to their conception of knowledge ascription, the meaning of ‘knowing p’ is determined by the epistemic criteria in a specific context. In their argumentations it is assumed that someone can be considered to ‘know proposition p’ iff ‘p’ is considered to be the case in that context.² The context-dependent truth-conditions of a knowledge ascription concerning ‘p’, (applied by an ascriber in a certain context), determine the truth-value of the statement that someone knows ‘p’. Through this idea of context-sensitive truth conditions, an epistemic inquiry of knowledge ascription becomes a semantic search for the meaning of ‘to know’.

The point is that this turn to semantics should not result in an equalization of ascribing ‘to know’ and ascribing ‘knowledge’. The ascription of ‘knowledge of p’ and ‘to know p’ differ with regard to their alleged correctness or truth. Considering *S to know p* is the ascription of an act, performed by the putative knower in a certain context in which p is considered to be true. But whereas the truth-criteria of ‘*S knows p*’ are context-relative, the truth-criteria of ‘p’ itself may also be different in a different context or at a later stage. The truth of ‘to know p’ is thus relative to a context, timeframe and an actor. And if ‘p’ has a truth value, so has S’s ‘knowing p’. On the other hand, S’s *knowledge* of p is less bound to a specific context and time, and thus more general, which makes its truth-criteria not necessarily contextual and less sensitive to changing epistemic standards. A focus on semantics and thus on truth-criteria of knowledge ascription, must account for the difference between the attribution of ‘knowing p’ and ‘knowledge of p’.

What would people over 100 years say of what we ourselves suppose to know now? What happens to the knowledge ascriptions that will be reviewed in the future? Are we to be ascribed false knowledge or did we retrospectively *not* know?

¹ Assuming this statement to be about propositional knowledge.

² In that sense, it is assumed that knowledge involves a concept of truth, in contrast to belief.

Jan Willem Wieland
ACCESS, SHIRKERS, REGRESS

Surely there are restrictions on what we ought to do. Here is one candidate restriction (epistemic of sort):

Access: For any action x and agent S , S ought to do x only if S can know that S ought to do x .

Example: I am obliged to donate my inheritance to charity only if I can know that I am obliged to donate (Sorensen 1995, 'Unknowable Obligations'). This seems plausible. For if I am in no position to find out that I am obliged to donate, then we should not expect me to donate. Importantly, Access does not require that we actually know our obligations, only that it is possible to know them.

Still, Sorensen takes Access to be problematic because of the following consequence: If one eliminates one's possibility to know whether one has an obligation, then one eliminates one's (potential) obligation. That is, shirkers could abuse Access by making their obligations unknowable.

Example: Suppose I would like to buy new shoes. Unbeknownst to me, the shoes I am interested in are made by a kid in Indonesia. If I had watched the news the day before, I would have known that probably they were made under suspect circumstances. Yet, I did not watch, and in the days after the media lose their interest in this issue. In this case, Access predicts that I do not have the obligation to refrain from buying the shoes. This is a bad consequence of Access.

In response, Sider (1995) suggests that this problem does not follow if we assume that everyone has the obligation to refrain from making it impossible to know one's obligations.

According to Sorensen, in turn, this solution invites a regress. By Access, one has the obligation to refrain from making it impossible to know one's obligations only if that further obligation is itself knowable. Now surely shirkers will make it the case that this new obligation is not knowable to them. The question is: should we again say that they ought to refrain from making it impossible to know their new obligation? This, indeed, lands us in a regress of obligations.

Now both Sorensen and Sider agree that there is a regress here, but only the former takes it to be vicious, i.e. problematic for Access. The question is: who is right?

In this paper I will shed light on this intricate issue by invoking logical tools, that is, by employing (first-order) argument schemas for regress arguments. First, I will make the regress argument explicit along the lines of these schemas. Second, on the basis of my reconstructions I will show why the regress argument, though logically valid, fails to be sound. The right conclusion, as I will argue, is that the regress of obligations doesn't commit one to reject Access.

LOGIC

Sander Beckers
ACTUAL CAUSATION IN CASES OF PREEMPTION: THE CP-LOGIC APPROACH

The study of causality by formal means has gained wide acceptance within philosophy as well as artificial intelligence. Within this rapidly developing field, the problem of actual causation - i.e., determining what caused an effect in a specific scenario - has proven to be quite hard to solve. In this paper we will present the CP-logic approach to actual causation, by illustrating how it handles an important class of cases categorized under the name of preemption. Furthermore, we shall contrast and explain the effectiveness of the CP-logic solution with the strained efforts of some of the approaches that work within the structural models framework developed by Pearl. This leads us to conclude that CP-logic provides a more elegant and consistent language for studying questions of actual causation.

Catrin Campbell-Moore

TYPE-FREE THEORIES OF HIGHER ORDER PROBABILITY

Higher order probabilities, or *embedded probabilities*³ are probabilities of probabilities. An example of a second order probability statement is:

“There is 70% chance that the chance that it will rain tomorrow is 80%”

In this situation we have understood “probability” as chance, but we could also interpret it as epistemic probability or credence. Embedded probabilities are useful for philosophy, as we shall argue in our talk, and as such we should develop a theory to deal with them.

There are two ways to develop a theory of embedded probabilities: the first, and most common, is to understand probability as an operator; the second is to understand it as a predicate. An operator applies to sentences and leads to a typed theory, whereas a predicate applies to sentence-names and leads to a type-free theory. The following are examples of typical sentences from the operator and predicate proponent:

- Operator approach: there is 80% chance that it will rain tomorrow
- Predicate approach: “it will rain tomorrow” has chance 80%

The difference is substantial; for example, as standardly conceived, the operator approach cannot express

$$\forall x(\text{Theorem}_{\text{PA}}(x) \rightarrow \text{Pr}(x) \approx 1)$$

though this is a sentence acceptable in natural language. The predicate approach does not have such limitations. For this reason the predicate approach is favorable.

However, the worry with the predicate approach is that one can construct sentences which are self-referential, for example a sentence

(π) The sentence labeled (π) has probability less than 0.5.

Such sentences cannot be expressed within the operator approach because of its restrictive syntax. (π) is similar to the liar paradox, which says of itself that it is false; and it may lead to paradoxes given certain additional principles. For example, one cannot have that B is probabilistically coherent, and assigns a degree of belief to sentences, including the sentence (π) along with the claim that $B((\pi)|\text{Ch}((\pi)) \in \Delta) \in \Delta$ for Δ an interval $\subseteq [0, 1]$, which is an instance of a slight generalisation of Lewis’ Principal Principle from Lewis 1980.

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We argue for a predicate approach to embedded probability, and consider how one might develop a formalism for this. We also aim to connect this to work on formal theories of truth.

Peter Fritz

WHAT MAKES POSSIBLE WORLD SEMANTICS POSSIBLE?

Modal expressions and concepts are ubiquitous in everyday life as well as philosophy. Formalizing them has led to a wide variety of modal logics. It is well-known that many modal logics can be given a possible world semantics; i.e., a semantics based on the idea that the truth of a statement consisting of a modal expression modifying a complement clause depends on the truth and falsity of the complement

³We call this *embedded probabilities* instead of *higher order probabilities* because “higher order” suggests a typed language. We also simply refer to type-free theories of embedded probability by “type-free theories of probability”

clause in certain possible worlds. What is it about modalities that makes such a formalization possible? I consider this question in the context of consequence relations for propositional modal languages, and argue that there is nothing special about modalities, as *any* inferential behavior of an operator can be characterized by a possible world semantics, given only the assumption that the standard operators of negation, conjunction etc. are interpreted classically.

To formally substantiate this claim, I use ideas from the philosophy of logic to motivate a certain kind of algebraic semantics for propositional consequence relations. I show that this kind of semantics determines exactly the consequence relations which are closed under alphabetic variants, which leads to a class of consequence relations that is much wider than the ones usually studied in abstract algebraic logic. Applying this to consequence relations in a standard propositional modal language, I show that the relevant kind of algebraic models based on Boolean algebras give rise to exactly the modal consequence relations that are also closed under replacement of instances of classical equivalents. Using Stone's representation theorem, it is then straightforward to show that these algebraic models can be turned into a kind of possible world semantics.

While none of these findings are mathematically difficult or surprising, I show that they can be helpful in philosophically re-evaluating the status of possible world semantics. A first consequence is that algebraic semantics for modal logic is much more philosophically compelling than it is usually assumed to be. A second consequence is that possible world semantics is only more informative than algebraic semantics if there is a relevant pre-theoretic notion of possible worlds which can be understood to constitute an intended model for the language under consideration.

Ansten Klev

CATEGORIES AND LOGICAL SYNTAX

Aristotle's *Categories* is traditionally ordered as the first work of the *Organon*. Kant introduced the categories in a section of the *Critique of Pure Reason* called the *Transcendental Logic*. Yet it might not be obvious to a reader of Aristotle or Kant today what logic has to do with categories. This talk will highlight the close relationship between categories and logical syntax.

According to Aristotle what fall into categories are "things said without combination." One can point to several Aristotelian passages to argue that what is said *with* combination is a judgement. In Aristotle's logic the basic form of judgement is 'S is P'. It is therefore natural to conclude that the S and the P, the terms of a judgement, are what is said without combination. This conclusion is supported by several passages in Aristotle's writings.

Syllogistic judgements do not occur nakedly as 'S is P', but are determined, in Kant's terminology, as to quantity, quality, and modality. According to Kant these aspects (together with what he calls 'relation') belong to the form of judgement, and is contrasted with the matter of judgement, which he identifies as the terms (*Jäsche Logic* §18). In the *Critique* Kant argues by reference to the role of judgement in cognition that there are precisely as many categories as there are forms of judgement. Hence we can compare Aristotle's and Kant's notion of category by means of traditional logical syntax: for Aristotle it is the matter of an 'S is P' judgement which is categorized, whereas for Kant it is the form.

The Fregean revolution of logic is in large measure a revolution in logical syntax, with the introduction of the basic form Fa , function applied to argument. Here no distinction is made between form and matter, and so all symbols|not only the matter or only the form|are assigned a category. The basic categorial distinction in Fregean logic is that between saturated and unsaturated elements. Frege was careful (much more so than Aristotle) to distinguish between linguistic and ontological levels and assumed that (un)saturation at the linguistic level corresponds to (un)saturation at the ontological level. Thus we get categories at both levels, in fact infinitely many, since the unsaturated elements are stratified into orders.

What results is often known as a hierarchy of types, but may equally well be called a hierarchy of categories. These categories determine—and are determined by—the modes of combination of elements falling into them: e.g., a unary function of objects combines only with objects. This property, nicely illustrated by Ajdukiewicz’s definition of grammatical categories, does not hold of Aristotelian syntax, where the terms may in principle be of any category (although there is a certain bias toward S’s in the category of substance).

Sergi Oms Sardans

TOWARDS A CONDITIONAL FOR THE *Sorites* AND THE LIAR

When we have a truth predicate in a language that allows for self-reference, Liar-like paradoxes may easily arise. Suppose we have a sentence λ that asserts its own untruth; it is well known that aided with the T-schema

$$(T\text{-schema}) \quad A \leftrightarrow T^{\ulcorner} A^{\urcorner}$$

we can conclude $T^{\ulcorner} \lambda^{\urcorner} \leftrightarrow \neg T^{\ulcorner} \lambda^{\urcorner}$. This last sentence, being of the form $A \leftrightarrow \neg A$, leads us, in classical logic, to a contradiction. The way in which we can derive a contradiction in classical logic from a biconditional of the form $A \leftrightarrow \neg A$ uses the Law of Excluded Middle (The Law of excluded middle or LEM consists of the validity of all instances of the form $A \vee \neg A$ for any sentence A), hence in a paracomplete logic (that is, a logic without LEM), we cannot derive such a contradiction.

Another closely related paradox is Curry’s paradox. Suppose we have a Curry sentence γ ; that is, a sentence that asserts that if itself is true then A , for any sentence A . Then, given the T-schema and classical logic, (in particular contraction; the principle that says that from any sentence of the form ‘ $A \rightarrow (A \rightarrow B)$ ’ you can infer ‘ $A \rightarrow B$ ’), we can show that A . Clearly, we could have shown anything.

Another one of the top ten paradoxes is the *Sorites* paradox, which arises in situations where we are dealing with vague predicates. We all agree that a man without a single hair on his head is bald. We also agree that a single hair does not make any difference with respect to baldness, so if a man with no hairs is bald, so it is a man with 1 hair; and if a man with 1 hair is bald, so it is a man with 2 hairs; and so on. Following this line of reasoning we can conclude that a man with 10.000 hairs in his head is bald, which we are not willing to accept.

The logic that, a part from very basic and desirable elementary laws, can contain the T-schema (for it contains the principle $A \rightarrow A$), is contraction-free and LEM-free but is strong enough to serve as a logic for natural languages is generally called RWK. Following the recent work of Hartry Field, I want to present a three-valued paracomplete logic that captures in a reasonably intuitive way how we reason under the phenomenon of vagueness and is as close as possible to RWK. I am confident that a suitable truth predicate can be added to this logic, but further research is needed.

The main point of the logic is its conditional, which is defined intensionally over a set W of ordered three valued points in the following way, for any sentences A and B and each point $u \in W$:

- $|A \rightarrow B|_u = 1$ iff for all $w \in W$, if uRw and $|A|_w = 1$, then $|B|_w = 1$
- $|A \rightarrow B|_u = 0$ iff there is a $w \in W$ such that uRw , $|A|_w = 1$ and $|B|_w = 0$
- $|A \rightarrow B|_u = 1/2$ otherwise

The points in W are ordered by $<$ using inclusion of the extension and the anti-extension of the predicates at each point and the relation R is defined as follows:

$$w_1 R w_2 \leftrightarrow ((w_1 < w_2) \vee (\neg \exists w_3 (w_1 < w_3) \wedge w_1 = w_2))$$

Although *Modus Ponens* fails under a notion of logical consequence defined using models in the usual way (preservation of semantic value 1 in each model and each world), it can be defined a weaker notion of logical consequence (using R) in which *Modus Ponens* holds.

A couple of negation operators can be defined, although none of them is fully satisfactory; the best one allows us to have just one direction of the double negation principle ($A \rightarrow \neg\neg A$) and one direction of contraposition ($(A \rightarrow B) \rightarrow (\neg B \rightarrow \neg A)$). If we use the weaker notion of logical consequence we can have *Modus Tollens*.

I take this proposal to be a first step towards a satisfactory logic for the Liar-like and *Sorites* paradoxes. I think that, even if the logic just sketched is not strong enough or it cannot be fully extended with a truth predicate, some other logic in the same line should be adopted to reason in contexts with vague and truth predicates.

Marta Sznajder JUSTIFYING LOGICS

In this talk I want to discuss ways of connecting the arguments for rational choice of credence functions to the problem of justifying different logical systems, and therefore to the logical pluralism debate.

In an attempt to clarify the perceived connection between logic and norms of reasoning MacFarlane (unpublished) offers a detailed discussion of different forms that bridge principles – general statements linking facts of logic to norms for belief – could take. The main virtue of his study is supposed to come from the fact that we seem to have clearer pretheoretic intuitions about the nature of belief than we have about logical validity. This means that once the connection between these two domains is clarified we can resolve pressing questions about logic by assessing the norms for belief that that logic would give rise to.

Influenced by MacFarlane, Hartry Field (2009) argues for a specific bridge principle, the consequent of which is spelled out in terms of constraints on degrees of belief. Field's approach, in spite of its obvious shortcomings, introduces a whole new perspective when it comes to MacFarlane's initial motivation, i.e. clarifying the notion of logical validity – and the importance of this move needs to be stressed more. There is an abundance of arguments for specific theories and norms for subjective probability measures (cf. Leitgeb and Pettigrew (2010) seeking an objective justification of Bayesianism), some of which – unlike in the case of arguing for a choice of a specific logic – appeal to pragmatic notions (e.g. betting behavior of agents).

The question I am interested in is whether and how the possible arguments used for the subjective probability measures – through a suitable and well argued for bridge principle – can be in a way translated back into arguments for the choice of logic. Coming up with pragmatic justifications for logical systems seems like an extremely difficult task and finding a way of using the insight of this kind we already possess in another domain is a promising first step towards that goal.

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John Wigglesworth

LOGICAL LAWS

In our everyday lives we constantly encounter real possibilities. They are the alternative future possibilities we are facing in an indeterministic world. They are dynamic possibilities, arising from concrete local circumstances, and they are compatible with our laws of nature. Real possibilities are most adequately pictured within branching frameworks, which allow for a direct representation of alternative future continuations of one and the same moment. In my talk I will provide a dynamic modal explanation of branching models for real possibility that elucidates why the possible courses of events those models depict are compatible with our laws of nature. Branching will be explained in terms of the local arrangement of objects and their potentialities.

In the framework of branching time our world is pictured as a tree of histories branching into multiple possible futures. More formally, one posits a backwards-linear connected partial ordering of moments. The branching structures defined this way can be employed in the semantics of temporal-modal languages if a valuation on the structure is provided. Each branching structure allows, however, for several valuations, and not every valuation yields a model for real possibility. What makes a branching model a model for real possibility is that all its possible courses of events are compatible with our laws of nature.

In order to explain why a branching model is a model for real possibility some link between the structure and its valuation needs to be established. Given the valuation at a moment, we need a local explanation of the possible future continuations of that moment compatible with our laws of nature. I will suggest a modal explanation of branching in terms of potentialities. The fact that histories branch at a certain moment in a certain way will be accounted for by the potentialities of the objects existing at that moment and their local arrangement.

When talking about potentialities I use this term in a rather broad sense. Potentialities are properties of objects and comprise dispositions, powers, etc. Every potentiality comes with a set of manifestation conditions that concern the local arrangement its bearer finds itself in. Next to potentialities that always manifest themselves in case their manifestation conditions are fulfilled by the local arrangement, there must, in an indeterministic world, be potentialities that under one and the same circumstances can either manifest themselves or not. The latter give rise to branching.

By explaining branching in terms of the interaction of the local arrangement of objects and their potentialities, a link between the valuation at a moment and the local branching structure is established that elucidates why the possible future continuations are compatible with our laws of nature. The branching model as a whole is built up step by step from the local future possibilities grounded in the potentialities of objects. In this way, we obtain a dynamic picture of real possibility: Some possibilities disappear and new possibilities emerge as time progresses.

METAPHYSICS

Stefano Catelan

LESSER IDENTITY

In this paper I shall critically assess the need for lesser identity in solving the traditional paradoxes of identity. In recent times, philosophers, first and foremost David Armstrong, have drawn attention to both a loose as well as to a strict-but-*partial* senses of identity as crucial to solve traditional puzzles (e.g. multiple location, material constitution, temporary intrinsics, Bradley's paradox).

The paper is divided up into three sections: the first one, exegetic, drawing upon Armstrong's writings

of the last thirty years, aims to tease out: (i) his stand on both loose and partial identity, and its development over time; (ii) why strict (numerical) identity generates puzzles; and (iii) how they can easily be accommodated by resorting to lesser identity. By doing so, Armstrong's view will be briefly compared and contrasted with those of other thinkers who dealt with similar issues, such as Butler, Chisholm, and Baxter.

The second one, evaluative, dissects Armstrong's senses of identity to the conclusion that his rather casual approach harbours some inconsistencies and perplexities: (iv) despite a distinction of name, loose and partial identity are handled by Armstrong in a way which suggests that, at rock bottom, they may be the same thing after all. (v) the attempt to draw upon Butler's notorious senses of identity to legitimate his own view appears contrived, what Butler names 'loose and popular sense of identity' results from defective judgment, not something worth the effort to be added to strict identity. Lastly, (vi) the evidence for the need of lesser identity to prevent the generation of paradoxes is justified only in virtue of Armstrong's peculiar ontological stance.

The third one, revisionary, granting Armstrong's ontology, claims that: (vii) for genuinely settling the alleged puzzling nature of identity and thereby assessing the need for lesser identity some further distinctions are required. Identity comes in various forms, e.g. numerical and qualitative, which are not different kinds of identity relation, rather identity can relate different kinds of things. In the case of qualitative identity, what one is really talking about is whether two *qualities* are numerically identical. In addition to this, our understanding and conception of identity implies logical, metaphysical, and epistemological 'senses' of the term, with a view to showing that (viii) the paradoxical nature commonly attributed to identity is generated and strengthened by an occasional lack of awareness of what is truly at stake, and that its puzzling nature vanishes once the various confusion is overcome. Finally, (ix) in the light of the distinctions alluded to above, Armstrong's appeal to lesser identity appears by no means necessary: given his rich ontology which distinguishes between properties, 'thin' and 'thick' particulars, it would be more economical to endorse a view which appeals to strict identity only, although applied to different kinds of things in different contexts.

Juliusz Doboszewski

SPECIOUS PRESENT, METAPHYSICAL INDETERMINACY AND THE A-THEORY OF TIME

The specious present doctrine claims that present has a non-zero duration. Such a notion of present seems to be on the rise recently -examples include Hestevold [2008], Tallant [2010] and Orilia [2011] presentist applications idea, or Dieks [2006], Artur [2006] and Savitt [2009] use of specious present to approach the problem of becoming in relativistic physics.

But the notion comes with a price, it's main problems being:

1. The duration problem: how long should the specious present be? If one allows for duration to be equal to $d \in \mathbb{R}$, $d > 0$, surely it could be extended by a small enough $\varepsilon \in \mathbb{R}$, $d > 0$. But the resulting duration $d + \varepsilon$ can again be extended by a small enough number, and so on. But then, any two events can be joined by a specious present, and hence all events are present, which is taken to be *reductio ad absurdum*.
2. Violation of the McTaggart's Principle (which any A-theory of time is supposed to satisfy): A-properties (being past, being present, being future) are incompatible—that is, no event can be both past and present “at the same time”. But specious present seem to allow that, since if a and b are two different events joined by a specious present, then the distance from a to b is nonzero, hence

under reasonable assumptions concerning mathematical representation of time, a is both present and either past or future with respect to b .

Thus Power [2011] argues that although A-theory of time is inconsistent with the idea of specious present, B-theory is much more friendly and hence is an attractive metaphysical thesis.

My goal is to argue for the treatment of specious present as a vague property, and by that, for an A-theory compatible specious present. Vagueness often is invoked to deal with problems trapping temporal properties, for example in Fernando [2010] treatment of temporal operators as vague predicates, or in supervenience treatment of future contingents.

Suppose that temporal properties are vague. Then, specious present might be thought of as coming with a set of precisifications P , which make events co-present. Schema for ascription of temporal properties might look as follows: if U is a set of co-present events under a precisification P , then: (1) x is past w.r.t U iff $x \notin U$ and there is $u \in U$ such that x is past w.r.t. u (2) x is future w.r.t U iff $x \notin U$ and there is $u \in U$ such that x is future w.r.t. u . Then, under a given precisification, McTaggart's Principle is preserved, and, under certain reasonable assumptions, not everything will be co-present.

I will then argue that a viable treatment of specious present is to treat it as something metaphysically indeterminate, in the vein of Barnes [2010] and Barnes & Williams [2010]. Thus we come to the theory endorsing:

(SP) Present has a non-zero duration.

(A) Being present, past and future are real properties.

(MD) Temporal properties are vague properties.

Which is a consistent (and possibly even attractive) metaphysical position.

Ben Liao

ON THE QUESTION OF MODAL REALISM AND (ANTI-)HAECCEITISM

How should we represent the possibility that “I could have been that guy” or “I could have switched roles with my brother”? These are instances of haecceitistic possibilities. We might think that it is unproblematic to represent these haecceitistic possibilities via some theory of possible worlds and some sort of counterpart theory. Skow (2007) has argued against doing so on grounds of the theory-relativity of the definition of “haecceitism”. In particular, the prospect of a genuine haecceitism debate is jeopardized if definitions are tendentious in this way. My modest claim is that, contrary to Skow, there is a non-tendentious way of defining “haecceitism” in terms of possible worlds that is neutral with respect to the ontological commitments of at least two dominant versions of modal realism. After briefly presenting some background on the haecceitism debate and the challenge due to Skow (2007), I give a definition of haecceitism in terms of possible worlds - in which “possibilities” rather than “possible *worlds*” is explicitly mentioned.

Here is a more detailed plan: In §1, I briefly summarize the haecceitism debate and trace its source to the problem of transworld identity. Along the way, I distinguish the thesis of haecceitism from acceptance of haecceities. Further, I distinguish, *pace* Divers (2002), between the non-epistemic problem of transworld identity (due to Chisholm, 1967) and the epistemic problem of transworld identification (due to Kaplan, 1967). I argue that Lewis' primary engagement with Kaplan in his definition of “Lewisian anti-haecceitism” may explain the source of the trouble raised in Skow (2007). In §2, I will briefly summarize Lewis' theory of possible worlds and his counterpart theory, which features prominently in Skow's critique. I will explicate Lewis' motivation for accepting intra-world counterparts and outline his solution

for accommodating haecceitistic difference in his anti-haecceitist picture. In §3, I outline Skow's critique – there, I will go along with Skow and assume that his definition of anti-haecceitism correctly captures the issue in the haecceitism debate proper. To anticipate, Skow's argument involves the denial of the equivalence between two definitions of "anti-haecceitism" with anti-haecceitism proper. In §4, I present my definition in terms of possible-worlds, albeit couched in "possibilities" and explain its merits, in particular, how it can be non-tendentious (acceptable by realist theories and equivalent to anti-haecceitism proper).

Melissa MacAulay

IS THE FEELING OF TIME'S PASSING A REASON TO REJECT THE B-THEORY OF TIME?

Theories that conceive of past, present, and future as entirely mind-dependent (i.e., the B-theory of time) are often rejected on the grounds that they require us to regard time's passage as an illusion. These kinds of arguments maintain that one should sooner accept the objective reality of time's passing (i.e., the A-theory of time) than take this central feature of our experience to be such a ubiquitous and inexplicable illusion. I challenge this particular defence of the A-theory by arguing that the feeling of time's passing is just as much of a problem for the A-theorist as it is for the B-theorist. After identifying what I take to be the essence of the A-theory (and its variants), I challenge an assumption that is too often implicitly accepted by both sides of the debate—that is, that the A-theory adequately accounts for the feeling of time's passing. I argue that, in fact, the A-theory of time flies in the face of popular intuition just as much—if not more so—than does the B-theory of time. Firstly, I will show that the A-theorist is in no better position to account for the unique direction of time's passing; both theories account only for its structural asymmetry. Secondly, although it is true that the B-theory challenges our perception of time, the A-theory defies all intuitions regarding movement and change by asserting that these phenomena may exist independently of time itself. To account for these apparent absurdities, A-theorists tend to appeal to the allegedly in-eliminable peculiarity of time, for which there can be no further explanation. Both theories, then, present a challenge to our intuitive picture of time and movement, and so neither should be rejected on these grounds.

Rik Peters

OPTIMISM AND IRREVERSIBILITY: THE NECESSARY ILLUSION OF TELEOLOGY

The rejection of a theory of final causes is one of the founding gestures of modern science and philosophy. However, despite a centuries-old general consensus that teleological reasoning is invalid, teleology is far from dead. Not only in the popular world-view but also in scientific and philosophical thought the reasoning from aims and goals remains firmly in place, perhaps most conspicuously in the areas of adaptationism in evolutionary biology and of ecosystem ecology.

I will argue for a partly Kantian explanation of the persistence of teleology: we should consider teleological thought as a necessary illusion. I propose to explain the genesis of this illusion using the concept of irreversibility. In the presence of two parallel series, one of which is reversible, the other irreversible, there seems to be a very strong tendency to consider the irreversible process as the optimal situation, and the reversible process as a suboptimal series, which ultimately has to be reversed so as to be assimilated to the irreversible series.

Looking at the case study of ecology, I will argue that the notion that the earth in itself forms a homogeneous system which is disturbed by human activity ultimately rests on the notion that the irreversible and relatively stable process of the earth's homeostasis is preferable to the more reversible and instable

process of human interference with this homeostasis. The descriptive layer – natural homeostasis is less reversible than human technological activity – gets translated into a normative message: human activity should stop interfering with the harmonious whole of the ecosystem: the reversible process should be altered so as to resonate with the irreversible one.

What this translation yields is a philosophical optimism about the self-regulating capacity of the earth based solely on the brute fact of irreversibility. The fact that a process cannot be adequately controlled from outside engenders the necessary illusion that the process has an inner teleology. The translation of mechanical description to normative teleology can be explained from a Kantian framework. I will propose a reading of this process from the point of view of part 2 of the *Critique of Judgment*.

Research into this form of teleology is important because it forms the background to a number of supposedly self-evident propositions about life, nature, politics and economy. One of the tasks of theoretical philosophy should be to facilitate a discussion about the common presuppositions that remain unnoticed in everyday practice. Teleological optimism of the kind described is one of the central presuppositions of common sense cosmology. It should therefore be analyzed philosophically. Another reason this line of inquiry is important is because the chapters on teleology in the *Critique of Judgment* are often neglected. I hope to show that Kant's insights here are crucial to a philosophical understanding of contemporary thought, and that we still have a lot to learn from the discussions on teleology and optimism to which Kant responds.

Carlo Rossi

DEFINING ENDURANCE

Endurance is often defined as persisting by means of being wholly present at different instants of time. Nevertheless, it is also often objected to this account the rather obscure role that the clause “wholly present” has in it. One straightforward solution the endurantist might adopt is to dispense with the use of such clause in her definition and simply claim that enduring objects persist through time by means of existing at different times, but without being temporally extended. The contrast with perdurance then would not lie so much in the fact that enduring entities are wholly present at different times, as opposed to being present by means of a proper temporal part; but rather in the denial of temporal extension for enduring entities.

Although this intuition might be fundamentally correct—or so I shall argue, we would still need to say more about it, in order to fill in the details that a plausible account of endurance should offer. The strategy I will follow in this paper intends to provide these details, by engaging first with Parson's Extended Simplex account (2000, 2007) and secondly with both the criticism that Hofweber and Velleman have recently directed to this account (from now onwards HV) and the account they themselves develop (2010). Unlike Parson's account, I argue that ordinary enduring objects are constantly changing some of their spatial parts and therefore should not be treated as simples. Also, against Parsons, I claim that endurantists should accept that objects are extended through space—and only through space—by means of having proper spatial parts at different spatial regions.

On the other hand, HV's novel criticism is motivated by a deep suspicion about the way in which the disagreement between endurantists and perdurantists has usually been set up and particularly the aforementioned definition of endurance: the main point of their argument being that it is a conceptual truth that follows from the sheer fact of occupying an extended interval of time that objects have temporal parts. Now, the natural rejoinder that one might expect from the endurantist to HV's objection is to question the assumption that to persist is just to be extended in time. Notwithstanding this, the line of response I favour here is one that undercuts the very grounds of HV's criticism. This response comprises a twofold argument. The first part of the argument, which I take from Hawley's account of endurance

and perdurance (2001), consist in introducing a second notion of parthood that would precisely do the work of differentiating these two different views on persistence. The second part of the argument draws on Fine's distinction between existence and extension (2008) and in a sense contextualizes Hawley's proposal. What Fine's distinction attempts to do is to explain how enduring objects persist, from the contrast between these two modes of presence, without appealing to any notion of extension and parthood. If these proposals turn out to be plausible, the endurantist should be in a position to account for what it might be to persist without being temporally extended, thereby blocking HV's criticism.

Antje Rumberg

BRANCHING MODELS FOR REAL POSSIBILITY

In our everyday lives we constantly encounter real possibilities. Real possibilities are the kind of alternative future possibilities we are facing in an indeterministic world. They are dynamical possibilities, arising from concrete local circumstances, and they are compatible with our laws of nature. Real possibilities are most adequately pictured within branching frameworks, which allow for a direct representation of alternative future continuations of one and the same moment. In my talk I will provide a dynamic modal explanation of branching models for real possibility that elucidates why the possible courses of events those models depict are compatible with our laws of nature. Branching will be explained in terms of the local arrangement of objects and their potentialities.

In the framework of branching time our world is pictured as a tree of histories branching into multiple possible futures. More formally, one posits a backwards-linear, connected strict partial ordering of moments. The branching structures defined this way can be employed in the semantics of temporal-modal languages if a valuation on the structure is provided. Each branching structure allows, however, for several valuations, and not every valuation yields a model for real possibility. What makes a branching model a model for real possibility is that all its possible courses of events are compatible with our laws of nature.

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By explaining branching in terms of the interaction of the local arrangement of objects and their potentialities, a link between the valuation at a moment and the local branching structure is established that elucidates why the possible future continuations are compatible with our laws of nature. The branching model as a whole is built up step by step from the local future possibilities grounded in the potentialities of objects. In this way, we obtain a dynamic picture of real possibility: Some possibilities disappear and new possibilities emerge as time progresses.

Mattia Sorgon

WHAT BINDS STAGES TOGETHER?

Stage theory claims that material objects are three-dimensional concrete and instantaneous entities. In that way it succeeds to solve many puzzles about the persistence of material objects through time (mereological change, coincidence of two distinct objects and co-location of a whole and its proper part) claiming the non-metaphysical nature of those problems and reducing those difficulties to semantical issues. At the same time this theory claims that a stage can be related to other stages in an appropriate sequence which persists through time, allowing us to derive the truth conditions of those statements which seem to refer to persisting entities. Therefore, the notion of stage and the definition of the relation among stages of the same sequence are two fundamental assumptions of stage theory.

While the first one can be clearly defined (i.e. a stage is identified by a portion of space at a single instant t), the latter one must be deepened in order to avoid objections about the incompleteness of the theory, which it would not be able to provide a clear and detailed formulation of what links together the stages of the same sequence. The aim of this talk is to point out how stage theory is able to provide a complete account of such connection. Once highlighted the basic role played by this relation inside the theory, this talk will analyse and compare two of the most interesting definitions provided for its formulation: the *non-supervenient relation* (Hawley 1999, 2001,), instance of a “strong” metaphysical definition, and the *temporal counterpart relation*, instance of a “weak” metaphysical definition. Showing how both definitions succeed to provide a thorough formulation of the connections between stages and the development of sequences, it will thus focus on the main aspects of every solution, that is the notion of “immanent causation” claimed by the non-supervenient relation and the “temporal counterparts” asserted by the temporal counterpart relation.

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Martin Vacek

CONCRETE IMPOSSIBLE WORLDS

Impossibilities abound. We all believe that things could have been otherwise. Leeds could have been closer to Manchester than it actually is, there could have been more cars, etc. Philosophers call the ‘ways things could have been’ possible worlds. By parity of reasoning, talk of impossible worlds - various ways the world could not have been - seems to be nothing but a regimentation of our pre-philosophical opinions.

David Lewis's theory of possible worlds is considered as unable to accommodate impossible worlds into its ontology. He argues against concrete impossible worlds on the basis that a contradiction in the scope of the modifier 'at w ' amounts to an outright contradiction. His argument goes as follows:

1. Suppose that there exists an impossible world at which $(P \text{ and } \sim P)$.
2. $\text{At } w \sim P \text{ iff } \sim(\text{at } w, P)$.
3. $\text{At } w (P \text{ and } \sim P) \text{ iff } \text{at } w P \text{ and } \sim(\text{at } w P)$.

Therefore

$\text{At } w P \text{ and } \sim(\text{at } w P)$. (contradiction)

For Lewis, the operator 'at w ' is a quantifier-restricting modifier, working in the similar way as the modifier 'in Leeds' does. Thus, unlike (hyper)intensional operators, which do not pass through the truth-functional connectives the restricting modifiers do. Therefore, supposing that there exists a world at which $(P \text{ and } \sim P)$, given the way how restricting modifiers work and given the opinion that the Lewisian modal space is classical, Lewis has no use for impossible worlds.

However, Lewis does not object to impossible worlds in general. He does so as an extension of his own theory. Firstly, that is because in his own conception of how modifiers interact with 'at w ' the consequence of admitting an impossible world is an out and out contradiction (not just a contradiction within the scope of something). Secondly, Lewis does not accept that there are true contradictions, and to accept that there are would be a major violation of conservativeness. So the argument is not a demonstration of the non-existence of impossible worlds. It is a demonstration of the unacceptability of impossible worlds to anyone who has certain orthodox pre-theoretical opinions plus the Lewisian conception of analysis.

I argue that even if that were so, there is an option to weaken the argument. As it seems, there is no no-question-begging reason to think that reality is classical. How can we know, after all, which logic appropriately describes reality? In particular, if there are worlds that instantiate plain contradictions then it is only to be expected that extended modal realism - modal realism + contradictory worlds - violates some of the main features of the Lewisian realism.

In my paper, I challenge the proclaimed universality of classical logic and motivate a paraconsistent approach to modal reality. Although the consequences are very hard to swallow, I argue that it is because of the fact that we are unsure of our pre-theoretical opinions concerning the impossible in the actual world. In other words, if reality is inconsistent, then the theory appropriate for it must not be classical.

Martijn Wallage

SEEING FACTS AND OCCASION-SENSITIVITY

1. Introduction I aim to clarify and resolve a tension between occasion-sensitivity and the thesis that perception is a matter of "seeing that things are thus and so".

2. Seeing facts In *Mind and World* McDowell defends that we see the sort of thing that we can also judge to be the case: *that things are thus and so*. Early Wittgenstein thought so too. This view has the advantage that it explains how perception can make things available for us to think about: there is no ontological gap between the sort of thing we can see and the sort of thing we can think. But it is in tension with occasion-sensitivity.

3. Occasion-sensitivity Occasion-sensitivity is the view (developed by Charles Travis) that for any representing, when things are as represented to be depends ineliminably on the circumstances of representing. An utterance of “The jelly fish is dead” (or the speaker in uttering that) may represent a particular jelly fish, stranded on the beach, as dead. Say this one can still be reanimated with future technology. Are things as represented to be? There are reasonable views on what it is to be dead such that the jelly fish would so count and reasonable views such that it would not. What view is more reasonable on the occasion, and so if the stranded jelly fish should on the occasion count as dead, depends on the circumstances of representing.

4. The tension Say occasion-sensitivity is right. Then what the beach stroller sees may, for some purposes of counting, count as a jelly fish being dead and for some purposes not. But facts cannot for some purposes count as things being thus and so and for some purposes not. They are the wrong sort of thing to stand on the left side of the “counting-as” relation, which occasion-sensitivity needs. So what we see, what is before our eyes, cannot be that things are thus and so.

5. Resolving the tension What we are aware of in perceptual experience is the particular case. Awareness of the particular case can be expressed in the form “seeing things (being thus and so)”. The particular case is not thinkable. To “see that things are thus and so” is to see that the particular case is of a certain sort: a case of things being thus and so. Approached in this way, “seeing things (being thus and so)” could be said to be primary to “seeing that things are thus and so”.

But one can still claim that although what we are aware of is not thinkable, our awareness of that is conceptual through and through. That is, one can hold that the only way we can be perceptually aware of particular cases, so that they become available for us to think about, is by seeing that things are thus and so.

Erik van Zwol

KANT, CAUSALITY AND EMERGENCE

A key argument used to forward a physicalist metaphysics is the argument of the causal closure of the physical. Every event has a cause and every cause is physical, there is no room for non-physical events that are themselves uncaused physically or self-causing. Spontaneity, as self-activity, and underpinning a certain conception of the power to have done otherwise, are, in physical determinism, excluded; the cultural consequence of physicalism, as a metaphysical postulate, has a number of consequences for our understanding of ourselves and what we are capable of achieving. In this talk, I will attempt to give a rival explanation for the causal structure of our experience, one that, while accounting for the general structure of the physical realm, also allows room for spontaneity and the possibility of self-causation. With this in place, the possibility of a ‘dualism’ between the broadly physical world and possible forms of thinking manifest in the accidents of its mutually interacting structures allows for the passivity of matter and the spontaneity of thought; with the extended cultural consequences.

The source of the different metaphysical view is Immanuel Kant. I will, rather than focusing on the *First* and *Second Critique* for the overview of the argument, draw heavily from Eric Watkins’ *Kant and the Metaphysics of Causality*. Watkins’ text gives a clear understanding of a model of causality in opposition to Hume’s event-event based model. In this talk, rather than arguing that physicalism implicitly employs Humean model of causality and as such suffers the problems inherent in that model, I will focus merely on showing how for Kant the natural laws of physics, and as such, the causal determination of the physical, is not closed. Thus, in the talk I give a brief overview of Kant’s model of Causality, taking care

to distinguish between the causal necessity that is imposed by the understanding's necessary synthesis of appearances immanent to the faculty of sensibility, the activity of the understanding itself as a causal power, as well as the mutual interaction of substances and how sensibility comes to carry representations in the first place. In distinguishing between real grounds, substances and accidents, it becomes possible to present a picture of the physical world that, while accounting for the epistemic determinate relations, as understanding, has ontologically the problematic possibility of freedom.

The above gives an historical model of causality in relation and opposition to a current claim and problem in philosophy. However, to make my claim against physicalism more contemporary, with Kant's metaphysics of causality in tow, I will cash out Watkins' interpretation of Kant's story as the phenomenal realm expressing the conditions for what David Chalmers calls 'strong emergence'. High level real grounds that can actively determine the accidents of substances in their area of affect cannot, in principle, be explained by the mutual interaction of lower level substances. The spontaneity of certain faculties of the mind cannot therefore, as such, be explained in terms only of our physical understanding of phenomenon, they are *sue generis*.

PHILOSOPHY OF LANGUAGE

Katharina Felka

NOMINALIZING NUMBER WORDS

My talk concerns the question of what the correct linguistic analysis is of statements like 'The number of moons of Jupiter is four'. This question is important, since realists use a certain such analysis in an argument for the existence of numbers. According to the realists, the statement is an identity statement in which the copula is flanked by number-referring terms. Because of this analysis, realists claim that the statement commits us to numbers.

Two puzzles arise for the realist view. The *Commitment Puzzle* concerns the question of how it can be that 'The number of moons of Jupiter is four' commits us to numbers but not the semantically equivalent statement 'Jupiter has four moons'. *Frege's Other Puzzle* concerns the question of how it can be that 'four' functions in 'The number of moons of Jupiter is four' as a number-referring term but not in the semantically equivalent statement 'Jupiter has four moons'. Anti-Realists consider these two puzzles as evidence that the realist analysis of 'The number of moons of Jupiter is four' is false. Since this analysis is false, they argue, the statement does not contain number-referring terms and the two puzzles go away. In my talk I will argue that the realist analysis of 'The number of moons of Jupiter' is indeed false. However, as I will further show, the correct linguistic analysis of the statement can only resolve Frege's Other Puzzle but not the Commitment Puzzle.

The structure of the talk is as follows. In the *first part*, I argue for the thesis that 'The number of moons of Jupiter is four' is not an identity statement. My argument proceeds in two steps. *First*, I use an observation of Thomas Hofweber's to show that the statement is a so-called specificational sentence. *Second*, I give evidence for the thesis that specificational sentences are not identity statements. In the *second part*, I argue for the thesis that the correct linguistic analysis of 'The number of moons of Jupiter is four' cannot resolve both puzzles. Again, the argument proceeds in two steps. *First*, I argue that the correct analysis of specificational sentences is given by the question-in-disguise approach, which claims that specificational sentences are disguised question-answer pairs. *Second*, I will argue that the analysis afforded cannot resolve both puzzles. For, this analysis can only show that 'four' does not function in 'The number of moons of Jupiter is four' as a number-referring term. But it cannot vindicate that the statement does not contain the number-referring term 'the number of moons of Jupiter'. Therefore, the

analysis can only resolve Frege's Other Puzzle but not the Commitment Puzzle. That means, with the rejection of the realists analysis the antirealists do not achieve their main objective, namely to show that 'The number of moons of Jupiter is four' does not commit us to numbers.

Sanna Hirvonen

NONREPRESENTED CONSTITUENTS AND PERSPECTIVE DEPENDENCE

Perry [1986] famously argued for *unarticulated constituents* whose existence has since been one of the core questions in the debates on the limits of semantics vs. pragmatics. In the same article Perry also talked about truth-conditional constituents which are not merely unarticulated but also not represented in the minds of the speakers. His example of such a constituent was Z-land in the utterances of Z-landers, a tribe which by stipulation have no concept of a location.

In this talk I argue in favour of *nonrepresented constituents*: truth-conditional constituents that are not conceptually represented by speakers or hearers but which are invariantly provided by the speaker's *perspective*. Let us take an agent's perspective to be her mental and physical states at a time, in a location in a possible world. I will argue that we should understand for example the actual world or the speaker's taste as entering the truth-conditions of certain utterances as non-represented constituents. Generally, we should posit a nonrepresented constituent when a) it is required for an utterance to be truth-evaluable, b) it is not conceptually represented by the speaker, and c) it is a part of the speaker's perspective.

The structure of the talk is as follows. First I introduce unarticulated constituents and the motivations for positing them. Then I argue that there are incomplete utterances (i.e. utterances of sentences which are not truth-evaluable even after the context-sensitive expressions have been saturated) which intuitively are true, but where we are not justified in positing unarticulated constituents, context sensitivity, or circumstance sensitivity. The existing views on semantic sensitivity are all committed to what I call the Principle of Semantic Competence according to which speakers know the truth-conditions of the expressions in their language. Therefore the views cannot posit a missing constituent which the speakers would not take as part of the truth-conditions of their utterances.

I argue that the incomplete utterances under discussion are intuitively true because despite of the lack of intentions, the constituent plays a causal role in the speaker's utterance. Given the principle of charity and the systematic role played by such constituents we ought to take them as belonging to the truth-conditions of such incomplete utterances. I call the resulting form of semantic sensitivity *perspective dependence*. I suggest that we should take for example the actual world and the speaker's epistemic state and taste as nonrepresented constituents of certain utterances. I then discuss the predictions the view has regarding communication and the truth-value judgments of perspective-dependent utterances by speaker / hearers. I conclude that perspective dependence is a highly promising view which makes superior predictions to contextualist and relativist accounts of certain problematic expressions, including predicates of taste and expressions indicating epistemic states.

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Franziska Köder

REPORTED SPEECH AND PRETEND PLAY

If one compares an utterance in pretend play (example (1)) with a speech report in direct discourse (2a) or indirect discourse (2b), the resemblance becomes immediately apparent.

- (1) Pretend play (playing a patient): *I am sick*.
- (2) Reported speech
 - a. Direct discourse: *Mary said, "I am sick"*.
 - b. Indirect discourse: *Mary said that she is sick*.

In both pretend play and reported speech, there are two stances involved: a person's real world identity and the person she is playing or whose speech she is reporting. Another similarity is that both phenomena develop ontogenetically in the same time frame. At around 2 to 3 years of age, children start to report utterances of others and they also begin to speak for the characters they impersonate in role play (e.g. Wolf, Rygh, Altshuler 1984; Ely, McCabe 1992; Nordqvist 2001). Given these parallels, it is surprising that the relationship between reported speech and pretend play has not yet been investigated more thoroughly in psycholinguistics and philosophy.

In my talk, I will try to throw some light on the developmental and conceptual relationship between speech reporting and role play. I will defend the position that speech reporting and role play should be treated as two distinct concepts even though they have many features in common. First, I will differentiate between different forms of speech reports (direct speech vs. indirect speech) and different forms of role play (embodied role play vs. replica play vs. imaginary companions) and sketch children's development of these phenomena. Second, I will compare speech reports and role play on various levels and I will consider several possible criteria to distinguish the two (like the presence / absence of a reporting clause, the intentions of the speaker, the context of utterance, different degrees of identification with the reported speaker / impersonated character). Once a conceptual distinction is sufficiently established, I will apply it to the case of natural child language where the fragmentariness of linguistic forms makes a categorization especially difficult.

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Karolina Krzyżanowska WHAT "MUST" AND "SHOULD" CAN MEAN

The meaning of epistemic modals like "must" and "should", although widely discussed, is far from being well understood. *Prima facie*, "must" seems to indicate the necessity of whatever it precedes. Yet, it seems perfectly appropriate to assert "It must be raining" when we only see people carrying around wet umbrellas, but not when we have a direct perceptual evidence, that is, when we can actually see that it is raining. This observation motivated the view that preceding an assertion with "must" makes it weaker (see e.g. Karttunen (1972) or Kratzer (1977)).

By contrast, von Stechow and Gillies (2010) argued that "must" functions as an evidential marker signaling the presence of an inference. According to these authors, what has been erroneously taken as weakness is simply indirectness of the evidence, and hence there is no reason to doubt that "must" is a strong necessity modal. Although, as I shall argue, the first part of their claim is most likely correct, von Stechow and Gillies jumped to their conclusion too quickly, because they failed to take into account the

variety of possible inferences. Might it not be the case that “must” indicates only one particular type of inference while, for instance, the weak necessity modal “should” signals a different type?

I will present a theoretical and empirical support for the claim that “should” can be thought of as an evidential marker signaling the presence of induction, whereas “must” usually indicates that the assertion is based on an abductive inference. Furthermore, I will discuss possible consequences of this finding for the semantics of epistemic modals.

Johannes Marti

TWO-DIMENSIONALISM AND INTERPRETATION

Stalnaker’s metasemantic interpretation of two-dimensional semantics provides a formal framework for the interpretation of utterances in a discourse (Stalnaker 1978, 2004). The original purpose of the framework was to explain how sentences expressing necessary truths can be informative. To this aim the truth value of an utterance is relativized to possible worlds in two different dimensions. First, the truth value of the utterance depends on the facts holding at a possible world in the standard sense in which the truth value of the proposition expressed by the utterance can depend on the facts that the proposition is about. Second, what proposition is expressed by an utterance can itself depend on the semantic facts that hold in the context of the conversation. Examples of this second kind of dependence that were given in the literature are: Indexical expressions, a Kripkean causal mechanism that selects the referents of proper names depending on the causal history of the name at the context of utterance, or conversations in which the participants have a misunderstanding about the meaning of words.

In my presentation I suggest to use the two-dimensional framework under a metasemantic interpretation as a, no doubt vastly oversimplified, formal model for the problem of radical interpretation. The problem of radical interpretation is to give an account of how an interpreter can learn about a subject’s beliefs and language given only observational evidence and no prior knowledge of her beliefs and language (Davidson 1984, Lewis 1974). In this application of the two-dimensional framework sets of possible worlds represent the totality of the subject’s beliefs. This is a standard procedure in epistemic logic and a natural adaption of Stalnaker’s idea to use sets of worlds for common ground in a conversation. I argue that then the two dimensions of two-dimensional semantics correspond to the two the two unknowns, beliefs and language, in interpretation. In the first dimension the basic facts of a possible world determine the truth value of a proposition and give the content of the subject’s basic beliefs. In the second dimension the semantic facts of a possible world determine what proposition is expressed by an utterance and how the subject uses language. As the basic evidence available for interpretation Davidson has proposed the notion of the subject holding a sentence true in her own language. I show that if we model this notion in the two-dimensional framework it corresponds to the expression of the diagonal proposition which Stalnaker proposed for the reinterpretation of certain pragmatically problematic assertions. Finally, I raise some questions about the nature of the semantic facts that are modeled by the second dimension of the framework if it is used in the context of radical interpretation.

Andrei Nasta

LOGIC IN NATURAL LANGUAGE

This paper argues for the logic in natural language thesis, i.e. a thesis that posits a tight connection between natural language and logic. Versions of this thesis have been recently contested by a number of theorists. I first make plausible the logic in natural language thesis by investigating two study-cases. The study-cases concern the syntax and the semantics of natural language, respectively. I thus give an informal logical treatment of (some aspects of) the syntax and of the semantics of natural language. I

then discuss five objections against the main thesis and offer replies. My conclusion is that the logic in natural language thesis withstands criticism, at both an empirical level and a methodological level.

Naomi Osorio-Kupferblum

IDENTITY CONDITIONS OF LANGUAGES

In his paper “There’s Glory for You”, Jeff Ketland presents a formal approach to language in the Lewisian tradition. Lewis distinguished between “language”, understood as the human ability and its exercise to communicate using strings of sounds, and “languages”, understood as systems of words and rules of transformation and composition. Likewise, Ketland distinguishes between the relation words have to what they denote, which he calls the “semantic relation”, and the relation speakers have to the language they speak, the “cognizing relation”. He takes it to be a contingent fact *which* language speakers cognize – they might have cognized any other language instead – but *necessary* that strings of sounds mean what they mean in the language they are part of – had they meant anything else (or nothing at all), they would not be part of that language. Ketland’s languages thereby behave just like sets do in that they are abstract objects whose identity is exhaustively defined by the elements they contain. Any change whatsoever to the elements that make up what a speaker cognizes, e.g. the acquisition of a new word, mean that the speaker is cognizing a different language.

Ketland’s view is paradigmatic of a school of thought which has been the mainstream in philosophy of language since the late 1970s and is only slowly losing ground. While much of the discussion that has made it crumble is about the semantic / pragmatic distinction, which runs along similar lines as Ketland’s semantics / cognizing distinction, it seems to me that these views are mistaken on a much profounder level: they disregard the metaphysical difference between “languages” used in mathematical logic and human language, and therefore get the identity conditions of natural languages wrong. In arguing against Ketland’s explication, I want to show where the mistake in all related views lies.

Identity conditions define sameness of objects (concrete or abstract) in (space-)time. For concrete objects, identity conditions are different for (i) things we regard as the same while what they are made of remains the same – philosophy’s pet example is a lump of clay –, and for (ii) things whose substance (for want of a better word) is essentially subject to change – all living things, for instance. The former can easily be treated like sets and defined by something akin to the axiom of extensionality. For the latter, people have often resorted to time-slicing. The trouble is that this approach, which is so helpful in explicating modality, is inadequate if we want to get a formal grip on things that change essentially. For them, the relation between time-slices would be part and parcel of their identities; slicing must therefore be carried out parallel rather than orthogonally to time. I will argue that natural languages should not be regarded as abstract objects because they are ontologically dependent on their speakers and the interaction between them. But if we want to abstract from their speakers, as is necessary for much linguistic, AI and logical work, we have to formalise them in a way that integrates change into their identity conditions.

Stellan Petersson

EXPLICATURES, SPEECH-ACT PLURALISM AND THE ATTITUDE OF HOLDING A SENTENCE TRUE

Arguments for the existence of explicatures have the following paradigmatic form: first the linguist/philosopher describes a non-linguistic context C in which a dialogue occurs between two speakers (let us call them Peter and Mary) as well as a short linguistic interchange in which Peter asks Mary a question Q and Mary replies with a short utterance R. Then the linguist/philosopher describes how Peter interprets what Mary said (or asserted) by using R (cf. Hall & Carston 2012).

The use of locutions such that ‘said that’ in contextualist arguments has, however, been criticised. Cappelen & Lepore (2005) argue that there is not a single proposition that captures what a speaker said by an utterance of a sentence in a given context of utterance. The truth of an indirect report, where the verb ‘say’ is used, is dependent on facts about the context of the report, and there is an indefinite number of true reports of a given utterance of a sentence (in a given context).

In this talk, I will defend the paradigmatic form of arguments for explicatures. I will argue that such arguments are warranted, if we think of them as evaluations of counterfactuals, in the sense of Williamson (2007). The counterfactuals should not, however, express indirect reports, but they should contain statements concerning which sentences speakers would treat as semantically equivalent in a given context. The Davidsonian notions of ‘radical interpretation’ and ‘the attitude of holding a sentence true (at a time)’ (Davidson 1974) are appealed to, in order to shed light on both one of the problems identified by contextualists and the object of research that the notion of explicature is relevant for. I will argue that arguments for explicatures may be thought of as attempts to clarify, from the perspective of the radical interpreter, why speakers have the attitude of holding true directed at literally false or trivially true sentences, and that the notion of explicature is relevant for the observation that speakers treat sentences with different contents as semantically equivalent in some contexts.

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Giulia Pravato

FAULTLESS DISAGREEMENT WITHOUT RELATIVE TRUTH

Many philosophers have recently claimed that some classes of predicates – here I’ll be concerned mainly with evaluative, notably aesthetic, predicates – display *faultless disagreement* effects, which we should account for when modelling their semantic behaviour (cf. Kölbel 2003 and MacFarlane ms). The key thought is that when a predicate F of the right sort (‘tasty’, ‘funny’, ‘beautiful’) is involved, *disagreement* and *mutual correctness* can be compatible properties of thought (beliefs) or talk (assertions). As a toy example, consider A and B, who respectively affirm and deny (1):

- (3) Ginger is delicious.

A and B, so a common argument goes, are not merely talking past each other or having a verbal disagreement – they are genuinely disagreeing. The disagreement over (1) is said to be different from a case in which one party utters ‘I’m hungry’ and the other utters ‘I’m not hungry’. At the same time, though, the disagreement appears to be faultless: *ceteris paribus*, A and B seem to be free of faults in their respective assertions. Thus the disagreement over (1) is said not to be like a simple factual dispute over, say, ‘Ginger is a root’.

How can we theoretically make sense of this set of conflicting intuitions? One preferred route is an *inference to the best explanation* in favour of Truth-Relativism. In a nutshell, the argument goes like this. While other semantic frameworks—Contextualism, Realism and Expressivism—end up dropping

disagreement or faultlessness or a truth-conditional semantics respectively, Truth-Relativism preserves all these nice features by relativising the truth of a complete propositional content to a subjective parameter, e.g. a judge, a perspective or an assessor. Furthermore, Truth-Relativism doesn't require a revision of common sense or of classical logic. The conclusion is that Truth-Relativism is the best choice in terms of a cost-benefit analysis.

My main concern in this paper is to reject this abductive inference. The cogency of the argument can be contested along three possible routes: (i) by denying that there is such a thing as faultless disagreement in the first place, (ii) by contesting the adequacy of the truth-relativist explanation and (iii) by rejecting the *indispensability* claim. My general plan is to pursue a strand of this third theoretical move. In particular, I present an alternative approach which makes use of *indeterminacy in truth-value* rather than relative truth in order to make sense of faultless disagreement. Why are taste judgments like (1) indeterminate in truth-value? The indeterminist, like the relativist, takes the truth-value of (1) – rather than the content or the proposition expressed – to be sensitive to standards of taste. Unlike the relativist, though, the indeterminist claims that the truth-value of (1) is determined by a kind of supervaluation: roughly, (1) is true if it is true on all admissible standards, false if it is false on all admissible standards and indeterminate otherwise. Since the parties to a disagreement over (1) have standards that yield conflicting opinions, (1) is indeterminate in truth-value.

In the second part of the paper I consider the virtues and vices of the *indeterminacy view* and I offer some replies to possible objections. To conclude, I present and characterise two versions of the indeterminacy view and I show how the choice between the two depends on one's overall metaphysics and epistemology of aesthetic value.

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Stefan Rinner

RUSSELL'S THEORY OF ORDINARY PROPER NAMES

According to philosophers like Gottlob Frege and John Stuart Mill ordinary proper names like 'Paris' and 'Napoleon' are used referentially. That is to say that ordinary proper names have a referent. Therefore, both Millians and Fregeans have to face the problem of empty names: If names are used referentially, how can there be empty names?

E.g. we can say something true with sentences like 'Atlantis does not exist', although 'Atlantis' does not have a referent. For that reason according to Bertrand Russell ordinary proper names are used attributively. That is to say that names are used as disguised descriptions. E.g. 'Atlantis' could be a disguised description like 'The sunken city (lying in front of the Pillars of Hercules)'. Therefore, according to Russell a sentence like

1 Atlantis was a naval power.

has the same truth conditions as a sentence like

2 There is only one sunken city, and every sunken city was a naval power.

In my presentation I will argue against Russell's theory of ordinary proper names. As will be seen following Russell (3) could be true.

3 According to the saga of Atlantis, Atlantis was a naval power.

Here (3) is true if and only if within the saga of Atlantis the truth conditions of (1) obtain. It is a common view among philosophers that within a saga or a story we are only pretending that the actual conditions obtain. Therefore, we can say that within a saga or a story the truth conditions of a sentence *S* obtain if within the saga or the story we are pretending that the truth conditions of *S* obtain.

According to Russell the truth conditions of (1) obtain if and only if there is only one sunken city, and if every sunken city was a naval power. Hence according to Russell (3) is true if within the saga of Atlantis we are pretending that there is only one sunken city, and that every sunken city was a naval power. Therefore, according to Russell (3) could be true namely if within the saga of Atlantis we are pretending that there is only one sunken city, and that every sunken city was a naval power.

As will be seen even if (1) would express a proposition, (3) could not be true. From this it will follow that even if (1) would express a proposition, (1) would not have the same truth conditions as a sentence like (2).

We can get to the same result via Kripke's modal argument. As has been shown by some philosophers Russell could block the modal argument namely if 'Atlantis' is a disguised description like 'The actual sunken city'. Therefore, concluding I will argue that Russell can not block the argument I am going to present in a similar way.

Ravi Thakral

A PROBLEM FOR HORWICH'S ACCOUNT OF INDIRECT DISCOURSE

According to Paul Horwich, the compositionality of meaning places no constraint on how the meaning properties of words are constituted. There is a constraint on lexical meanings but this is given by the features of our acceptance practices, not compositionality. Call this view 'trivialized compositionality.' My investigation will focus on opaque contexts (cases where there could be failure in substituting co-referential expressions *salva veritate*) and whether or not Horwich's theory can account for them. An account of opaque contexts, for Horwich, will plausibly consist in the following theses:

(TC) Trivialized compositionality

(RSA) Reference shift analysis (in indirect contexts)

(PROP) Propositions are the bearers of truth

Horwich's preferred semantic analysis of that-clauses is as follows: the propositions we refer to by our deployment of that-clauses express structured entities whose reference shifts relative to linguistic context. (PROP) is a central feature of Horwich's deflationary theory of truth and his trivialized picture of compositionality.

I will argue that this theory of compositionality fails in opaque contexts. This, I argue, is due to Schiffer (2003)'s discussion of the exportation problem. Briefly put, theories that hold (RSA) have trouble accounting for the principle that it is legitimate to move a name from a narrow-scope position with respect to an attitude verb or operator to a wide-scope position.

The upshot of the exportation problem is that it presents a challenge for theories that posit propositions as the referents of that-clauses. Drawing on this challenge, I show how we can force a tension for (PROP), which is an essential feature of Horwich's view.

I discuss the options available to Horwich. An obvious option is to remove propositions from this semantic theory, but, as I argue, the costs are too high. His use-theory of meaning (Horwich 2008) in particular could suffer consequences if we remove propositions. Another available alternative is to opt

for a different account of propositions. But I show that these different accounts conflict with fundamental assumptions about (TC). Hence, it seems as though there are issues with holding that compositionality is a trivial principle while also maintaining that propositions play a central role in a semantic theory.

Jacek Wawer

BIVALENCE OF FUTURE CONTINGENTS AND ITS METAPHYSICS

On the branching-time picture of reality, possibilities resemble a tree—every possible moment has a single past, but a number of future continuations. In this setting, a problem of future contingents is particularly vivid: how can we ascribe truth values to sentences about the contingent future if different possible future branches suggest different verdicts? I argue (Wawer, 2013), against Belnap and Green (1994) Belnap et al. (2001), and MacFarlane (2008, 2013) that there is a sound way to do it. After Belnap and Green (1994), I call such an attempt a Thin Red Line theory.

I take the branching-time picture at its face value—as a representation of possibilities of the world. The possibilities might (Belnap et al., 2001), but need not be identified with the world itself. If one is an actualist, a presentist, or a growing block theorist, one might be willing to discriminate among the possible moments and distinguish those which “match” the concrete reality. To explicate the assumptions in philosophy of time and possibility which are smuggled into the semantic analysis, I introduce a notion of presemantics. This concept extends the dichotomy of postsemantics and semantics proper proposed by MacFarlane (2003, 2008, 2013). Formally speaking, presemantics dictates how to map the contexts—parts of the concrete reality at which utterances occur—to the branching tree of possible moments; semantics proper provides truth conditions for tense and modal connectives; postsemantics decides how to apply the semantics proper to ascribe truth values to utterances used in a given context. Equipped with this trichotomy, we can represent each theory of future contingents as a conjunction of presemantic, semantics and postsemantic assumptions. In particular, a theory according to which every use of a future contingent in a given context has a truth value can be summarized as follows:

1. The collection of all possibilities can be represented as a tree-like structure which can be used as a semantics for our tensed, modal language. (Semantic proper assumption).
2. Only one of the branches accurately represents our world. (Presemantic assumption).
3. When we assess a truth value of a sentence used in a given context, we should evaluate this very sentence at a possible moment designated by the context, at a possible history which represents the actual world. (Postsemantic assumption).

Such a Thin Red Line theory can be seen as a natural outcome of a simultaneous belief in actualism and eternalism. I argue that this view can be soundly defended against the objections found in the literature (Belnap et al., 2001; MacFarlane, 2013) and it gives a better account of a modal indexical ‘actually’ than any other branching theory presented in the literature.

The paper sums up fragments of my article “The truth about the future” published in *Erkenntnis* (available on-line with open access, DOI : 10.1007/s10670-013-9454-3).

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PHILOSOPHY OF MIND

Benjamin Andrae

DIFFERENT KINDS OF ERROR IN A STRONG TELEOSEMANTIC THEORY OF REPRESENTATION

Explaining what it means that the mind is ‘about’ its environment is one of the most interesting tasks of a modern philosophy of mind. It is also one of the most thematically diverse topics, in that there are multiple, radically different answers. Instead of quarreling which of these different takes is correct, it is probably wiser to offer detailed formulations of the different versions, and only afterwards compare their relative merits and flaws.

In this spirit, I offer a detailed formulation of one of the many theories of representation: Teleosemantics, i.e. the idea that representation is based on a notion of fitness-to-survive. Here, I use a formulation of this idea which relies on the notion of a function that takes environmental states E_i as arguments and returns one-dimensional fitness values $F(E_i)$, interpreted as probability to survive until the next timestep. Also, a distribution of environmental states (called ‘niche’), weighted for the probability for the organism to encounter them, is employed. The definition offered and to be defended is:

Whenever – due to the organism’s input-output behavior – the expectation value of $F(E_i)$ obtained for the ‘niche’ (which must be equilibrated on its time-scale) of the organism is high, compared to the values resulting from any random or fixed input-output behaviors, the organism attempts to represent the part of E_i which is (as provided by counterfactual analysis) responsible for the high expectation value of $F(E_i)$. If, in the actual case, the value of $F(E_i)$ really is high, compared to the values resulting from other available input-output behaviors, the organism represents E_i correctly, and whenever it is low, compared to the ones resulting from other available input-output behaviors, the organism misrepresents E_i .

Note that this is a ‘strong’ teleosemantics, in that it claims to reduce representation to survival, rather than explaining survival as a result of good representation. As such it does not require a separate theory of what representation actually is, and can count as a theory of ‘aboutness’ on its own. The price to pay is that it might sometimes seem that it ‘has things backwards’ – since it seems to reverse the order of explanation.

Rather than focussing the general debate on whether such a ‘strong’ theory is adequate, I want to point out one of its strengths, implicitly challenging discursive opponents to do the same for their theories. This method, I believe, will result in greater theoretical advance than head-on confrontation.

The strong point of the teleosemantic approach provided above which I choose to highlight is this: It has the ability not only to deal with error – or misrepresentation – but also to differentiate between many different mechanisms of error. These kinds of error are sufficiently diverse in both cause and form to justify speaking of altogether different errors. This ability to differentiate is definitely a point of merit for

the teleosemantic framework I present, especially since many of the different errors correspond nicely to the intuitively very different situations in which we commit errors in our everyday lives.

Elvira di Bona **ON HEARING SOUND SOURCES**

My overall question is if it is possible to hear sounds as well as to hear the objects which produce them. I approach this question by elaborating an argument based on considerations about the function of audition, the quality of timbre and the metaphysical status of both sound and sound sources. I shall articulate my talk into three passages which support the final claim that we can hear sound sources.

1) I shall firstly evaluate how timbre, more than loudness and pitch, conveys information on both the material constitution of sound sources and the producing conditions of sound. Timbre is commonly defined as the feature of sound which explains the dissimilarity between two sounds which have same pitch, same loudness and same duration (Sethares 2005: 27). For example, suppose you hear two sounds, one produced by a violin and the other by a flute. Both sounds last 3 seconds, have a pitch of 440Hz and a volume of 75dB. Those sounds are alike with regards to pitch, volume and duration but they differ in terms of timbre. There are various acoustics cues which determine timbre (the spectral shape, the envelope, the pattern of inharmonic sound, the transition and overlap between sounds, timing and rhythm: Handel 1995). All these cues point at both the way in which sound sources are stimulated and the material composition of sound sources (ibid; Giordano and McAdams 2010).

2) Then, I shall focus on the function of audition. As Nudds (2010, 2007) suggests, the primarily function of audition is to tell us about objects in our environment (2010: 109). In fact, he affirms that our auditory experience represents, in addition to sound, sound sources. He claims that we firstly detect the frequency components which are present in the vibration of the sound waves and then we group together the components that have been produced by the same source (ibid: 113).

3) In the third place, I shall define what is the metaphysical status of both sound and sound sources. I define sound as an event-like individual located at sound sources (Casati and Dokic 1994; 2005; O'Callaghan 2007; Matthen 2010) and I distinguish two components which constitute sound sources, namely the event source (the jiggling) and the *thing* source (the key) (Casati, Di Bona, Dokic 2013). Then, I identify sound with the event source (ibid), specifying that the audible qualities of sound sources become audible by virtue of the occurrence of the event in which the thing source is involved.

Considering that 1) timbre has the specific role to convey information about objects in our environment, that 2) the function of audition is to let us represent the environmental objects which produce sound and that 3) the identity between sound and the event source (where the event source is the way in which audible sound sources properties become audible) allows us to attribute timbre to the event source, I shall finally conclude that in hearing sounds we hear also sound sources. As a corollary, I shall add that the multimodal perception of the production of sound (Nudds 2001; O'Callaghan 2011) is not the only way for us to perceive sound sources, since my account is an attempt to furnish a possibility to *auditorily* perceive sound sources.

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Patrick Butlin

TELEOSEMANTICS AND THE DIRECTION OF FIT OF DESIRE

The teleosemantic tradition offers us both an attractive general approach to the problem of naturalizing direction of fit, and a widely agreed-on criterion for indicative content. However, there is more dispute about imperative content—in particular, Price (2002) rejects the criterion for imperative content that seems to be adopted by both Millikan (1984) and Papineau (1993). This paper defends a criterion for imperative content that fits into the teleosemantic framework, but which is distinct from either Price’s criterion, or the original criterion proposed by earlier writers. The criterion I defend has the striking consequence that, on some plausible empirical assumptions, it implies that desires lack imperative content.

The new criterion is inspired by Lewis’ (1969) proposal that what distinguishes indicative signals from imperatives is whether the producer or the consumer has discretion in how they are used. Signals have indicative content when their producers lack discretion about the circumstances in which they are to be produced, while signals have imperative content when their consumers lack discretion about what to do when they receive them. In teleosemantic terms, the criterion for imperative content is that an signal’s consumer should have the function of behaving in a specific way when adapted by that signal.

Presumably, desires are consumed by a system that has the function of calculating what action should be performed, based on the range of desires and instrumental beliefs that are occurrent at the time. When any give desire adapts this system, what it will have the function of doing depends on what other desires and instrumental beliefs are then occurrent. There is consequently no specific way in which the consumer system has the function of behaving when adapted by a given desire, so the consumer has discretion, and desires lack imperative content.

The main advantage of the discretion criterion is that it captures a distinction between two ways in which signals can be useful to their consumers. One way is if the consumers lack discretion; then signals help their consumers determine what to do in the simplest possible way. The other way is if the signals have indicative content; signals can be useful if they co-occur with some relevant condition sufficiently reliably for consumers to adapt their behaviour to this fact. If these are the only two ways in which signals can contribute to their consumers, the discretion criterion seems to give an account of direction of fit that reflects a deep distinction. So a substantial part of my task is to show that there is no other distinction of similar significance, which is captured by one of the other possible criteria for imperative content, and gives us more intuitive results about the direction of fit of desires and related signals.

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Frank van Caspel

DYNAMICAL FUNCTIONALISM AND THE ONTOLOGY OF THE MENTAL

How we humans are able to navigate the social world has been the focus of debate among philosophers for quite some time. Human behavior is enormously complex, yet we are able to understand and predict with great accuracy that someone will start cursing in certain circumstances, or that they will get up and take some milk out of the fridge. Our basic intuitive explanation of this remarkable skill, often called mindreading, is that we understand others because we recognize them as exhibiting mental phenomena: someone cursed because he was *angry* after losing a game of chess, and when someone *wants* to drink some milk, he'll probably get up and walk to the fridge to get it. Indeed, "it is hard for us to make sense of behavior in any other way than via the mentalistic (or 'intentional') framework . . . It is our natural way of understanding the social environment." (Baron-Cohen 2001, pp. 3-4)

The current debate on mindreading is shifting from a focus on the clash between theory theory (TT) and simulation theory (ST) to a debate about whether mindreading is as pervasive as both TT and ST assume. Other 'non-mentalizing' accounts of social cognition have been put forward, which do not rely on the (pervasive) ascription of mental states for their explanation of our daily social capacities. However, a critical look at the metaphysics underlying the concept of 'mindreading' – that the mental consists of mental *states* – has the potential to show that the question whether mindreading (thus understood) is ubiquitous is an empty question. The surreptitious assumption that beliefs, desires, etc. are *states* underlies many issues in the mindreading debate, and therefore merits scrutiny. I propose an alternative metaphysical basis for mindreading, *dynamical functionalism*, which may turn the debate away from its current problems into a more productive line of research.

From a dynamical functionalist perspective mental phenomena are not defined as functional *states* ('a belief', 'a desire') but as *functions* (believing, desiring). 'Believing' is a certain function defined in terms of what the organism that believes *does*, not what it *has*. This way we can understand that a person is believing, without ascribing to him 'a belief', just like we can understand that a plant is growing without ascribing to it 'a grow'. If we accept that mental phenomena are essentially functional, issues concerning mental states and the question whether we ascribe them all the time are rendered obsolete. There are no such states, so there is nothing to know about them. Accepting this view clearly has major implications for the debate as it touches on the very nature of what mindreading is. No longer do we 'read minds' by somehow ascribing mental states, but by recognizing functions.

In my talk I aim to explain the ontological status functions and how Dynamical Functionalism takes this as a novel starting point for approaching the mental.

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Leon Geerdink

EMBRACING THE PLURALITY: INTERPRETING REASONING TASKS FROM A LOGICAL PLURALIST PERSPECTIVE

In this presentation I challenge the common misconception within the psychology of reasoning according to which there is only a single correct answer to the question which arguments are valid. Psychologists have been searching for this answer because it is supposed to provide a clear cut criterion which determines how well participants perform in reasoning tasks. I will argue that this is an oversimplified view

of the notion of validity. Although I agree that a theory is necessary in order to interpret experimental data gathered in reasoning tasks, this does not necessarily mean that this theory will have to give a unique answer to the question whether a conclusion is a *logical consequence* of given premises. In recent years a position in the philosophy of logic known as *logical pluralism* has been defended (see for instance (Beall & Restall, 2011)). This position holds that there is not one single correct concept of logical consequence, but instead argues that there are several different notions. These different notions do not rival each other and so it is wrong to think that one of these relations is more correct than the other, although it is the case that some might be more informative in specific situations.

Now, each of these different notions could be employed by participants during reasoning tasks when they are asked to judge what conclusions *follow from necessity* from given premises. The same participant might not even be using the same notion across different tasks. This possibility that participants might not be using the same notion of logical consequence in each and every reasoning task has been largely overlooked in the literature. An exception is the recent book *Human Reasoning and Cognitive Science* by Keith Stenning and Michiel van Lambalgen (2008). But although Stenning and Van Lambalgen do provide a mental mechanism for how participants are able to use different notions of logical consequence by arguing that participants can assign different logical forms to premises and can assume different structural rules depending on the task they want to perform, they do not sufficiently argue why participants have the right to do this from a normative point of view. This normative ground can be provided by logical pluralism.

Jasper van den Herik

THINKING BEYOND THE NEO-CARTESIAN DUALISM THROUGH THE STUDY OF EMOTIONS

The cognitivist theories that were dominant in the philosophy of mind in the latter half of the twentieth century promised to take our inner life serious again after the mental had been thoroughly ignored by the behaviorist theories that preceded them. Since its conception however cognitivism has struggled using its paradigm of abstract symbol manipulation to give a phenomenologically sound theory of human experience. The source of these problems can be traced back to the Neo-Cartesian dualism that is presupposed between the abstract-intellectual world of the mind, materialistically conceived of as the brain, and the physical body. The struggle that cognitivist theories have with human experience can be seen in the debates concerning the explanatory gap or the hard problem of consciousness.

The recent surge of interest in embodied cognition theories can be seen as a reaction to this Neo-Cartesian dualism. The cognitivist idea of the brain as being both a necessary and sufficient condition for cognition and consciousness is contested. This makes the emotions a privileged phenomenon in this new paradigm, because they seem to involve both bodily feelings as well as cognitive appraisals. This leads to a renewed interest in the phenomenology of human experience drawing heavily on the notion of the lived body of Merleau-Ponty. The body is emancipated from its meaningless existence because it is at the very core of how we perceive the world around us.

In this paper I take the work of Colombetti, a philosopher of affective science working within the embodied mind field, as a starting point. The lived body means that we do not just perceive our body qua body (e.g. a racing heart) in an emotional episode, which Colombetti calls foreground bodily feelings. We also perceive our body as that through which the emotional interpretation of situation is possible. These are the background bodily feelings. Colombetti introduces the metaphor of coloured glass: our evaluations of the world are coloured through these background bodily feelings. The third, most fundamental bodily feeling is the existential background feeling. This is the way we find ourselves in the

world. Mostly we are unaware of this, but in the case of depersonalization or derealization we can see that the world is 'different'.

I argue that these three types of bodily feelings should not be viewed as being separate entities, because this would lead us back to the Neo-Cartesian dualism we are trying to overcome. If our sensation was actually coloured through our body, then we could also imagine the colouring effect to be absent. Instead we should see our experience as coloured in our body, as coming forth from the body itself. Emotions should not be seen as a separate affective phenomenon, but as eruptive peaks on a continuum of background existential feelings. In this way we have overcome the problematic Neo-Cartesian dualism, because the emotions are shown to arise from the bodily processes themselves.

Kinga Jęczyńska

BLOCK'S DISTINCTION BETWEEN ACCESS AND PHENOMENAL CONSCIOUSNESS IN THE FACE OF EMPIRICAL DATA

Ned Block introduced a distinction between access consciousness and phenomenal consciousness. Access consciousness includes mental states whose content is available to information processing systems (memory, planning, assessing alternatives, decision making etc.) and can be used to control reasoning and behaviour. Phenomenal consciousness is an experience - it includes impressions of "what it is like to experience something." Block has presented various theoretical arguments to emphasize the conceptual difference between the two kinds of consciousness, namely zombie argument, a hypothetical condition referred to as superblindsight based on an actual condition called blindsight, and a possible interpretation of phenomena occurring in everyday sense data perception. The theoretical arguments put forward by Block reveal at most logical possibility of two different kinds of consciousness. However, Block claims that the introduced distinction is not purely conceptual. It is possible that these two concepts pick out two different things, although they may be closely related to each other, for instance phenomenal consciousness may smooth the way to mechanisms of access consciousness. He states that the distinction may play an important role in scientific investigations of neural correlates of consciousness. He advocates the possibility that there are separate neural correlates of phenomenal and access consciousness. It seems, however, that these two concepts refer to the same empirical phenomenon. Block quotes empirical data such as experiments within signal detection theory, which allegedly constitute evidence for the existence of two kinds of consciousness. Still, it is possible to present a different interpretation of these data, which does not support the claim that phenomenal consciousness could occur without access consciousness. The data include experiments within exclusion failure paradigm conducted by Visser & Merikle, Snodgrass & Shevrin and Haase & Fisk. As also indicated by Irvine, the empirical data do not prove the existence of two specific kinds of consciousness. It is possible to explain the data with reference to differences in awareness or merely conscious and unconscious processes.

Jens Van 't Klooster

PRACTICAL SELF-UNDERSTANDING AND THE CIRCULARITY OF CONSCIOUSNESS

Psychological theories aim to understand human life in terms of psychological mechanisms, one of the most important being the human ability to act. My presentation will investigate the consequences of recent agent-knowledge theories of human action for the possibility of psychological explanation.

Agent knowledge theories understand intentional action in terms of the perspective of the agent on the relation of the action and the ends that the agent hopes to achieve. Only when an agent has 'practical knowledge' of his action, can the movement of the individual be considered intentional. Practical knowledge is, in Aquinas' slogan, "the cause of that what it understands".

When this analysis of action is correct, a psychological theory of action should find the mechanism responsible for allowing us to act while understanding the action as valuable in some sense. But it is not clear at all how this could be done when intending an action and understanding oneself to intend the action cannot be analyzed separately. Is an explanation of the human ability to act that is not in a problematic way circular still possible?

While a number of authors have used specific 'normative attitudes' to explain the human ability to act, I will argue that for a real psychological theory these attitudes themselves should be considered as explanandum and cannot one-sidedly be used as explanans.

By considering the development of human agency, the threat of circularity can be avoided.

Jakub Ryszard Matyja

MOVEMENT TO MUSIC AND THE THEORETICAL CONCEPT OF MUSICAL AFFORDANCES

Although enactivism is currently one of the "hot" topics in philosophy of mind and cognitive science, the application of this paradigm to the philosophy of music is - at best - in its childhood years (e.g. Krueger, 2009). On the contrary to traditional theories in philosophy of music (e.g. Kivy, 2002), enactivists suggest that perception of music is not solely a matter of manipulation of internal mental representations of musical piece, but is a sort of skillful exploratory sensorimotor *activity* of our bodies that are engaged in interacting with external world.

Arguably, on an enactive view, we tend to act upon what music *affords* us (e.g. emotional engagement with it), rather than react to music on the basis of sophisticated and abstract operations on abstract mental representations of music. That is, enactivists claim that we interact with music on the basis of (musical) affordances. Indeed, the fact that enactive music cognition draws heavily on the notion of musical affordances is of special theoretical importance. Yet, interestingly, given the variety of theories (e.g. Windsor de Bézenac, 2002), the very notion of musical affordances begs for precise theoretical specification, since it remains unclear what they are exactly and what is the scope of this notion.

Thus, the aim of my talk is twofold: I want to (1) very briefly introduce some of current theories of musical affordances and (2) suggest a more narrow (and motor - based) interpretation of the notion of affordances. I want to rely here on the recent research on enactive understanding and motor intentionality, as supported by research on human mirror neurons system (Sinigaglia, 2008) and the hypothesized function of these neurons in terms of movement-based musical therapy (Overy & Szakacs, 2009).

I conclude with the claim that such motor - based understanding may serve as a basis for the unified theory of musical affordances.

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Stefan Pliquet

EMBODIED SUBJECTS AS TEMPORAL OBJECTS

The aim of the paper is to explore the conceptual connections between embodiment and temporality in Kantian conceptions of subjectivity. I proceed through a critical discussion of Cassam's materialist conception of self-consciousness in *Self and World*. Cassam's central argument for a materialist conception of self-consciousness is the following. Being presented to oneself as a physical body among other physical bodies is a necessary condition of self-consciousness. Cassam claims that subjects' bodies are not only subjects, but also objects of experience. More precise, subjects are aware of their own bodies as physical objects *qua* subject of experience. This two-fold character grants bodies a privileged role in human cognition as they serve as a linchpin which connects subjective and objective perspectives on the world.

I concur with Cassam that awareness of one's own body is constitutive for self-consciousness, but disagree with his straightforward application of a Lockean conception of physical objects to human bodies. The reason is that Cassam's limited conception of bodies as shaped, located and solid entities leads to an untenably static view of the cognitive subject. The root of the problem is that Cassam unquestioningly adapts a presupposition deeply entrenched in contemporary analytical philosophy—the priority of space over time.

As an alternative I argue that Kant's view of the relative importance of the forms of intuition should be reinstated. For Kant time has priority over space. The clearest exposition of this preference can be found in the *Schematism* and the *Principles* in the first *Critique*. In these chapters Kant expounds fundamental principles of cognition in terms of the determination of time by the categories. Time is the all-encompassing dimension of experience and the identification of temporal relations is indispensable for the cognition of an objective reality. I contend that, on an embodied account of cognition, this paradigmatically holds for the cognition of subjects' own bodies. Temporality hence is a necessary condition for subjectivity and precedes embodiment in the order of explanation.

The conceptual connections between embodied subjectivity and temporality, however, are not limited to the constitutive role of temporality for the embodied subject; viewing the subject as embodied also has repercussions for the adequate description of time. This can be shown using a modified argument from *Self and World*. Cassam compellingly argues that an embodied conception of Kantian subjectivity entails that persons are an irreducible part of the world. He further concludes that first person descriptive vocabulary cannot be eliminated from a complete description of reality. This leads Cassam to defend the irreducibility of 'egocentric space' (the term is Evans') to public space. I explore the temporal analogue of this argument to criticize Kant's predominant use of 'objective' B-series predicates (earlier - simultaneous - later) in his descriptions of temporal relations and suggest that some aspects of Kantian philosophy can be more adequately discussed in 'subjective' A-series terminology (past - present - future).

Rebecca Spindler

SUBSTANCES VERSUS REAL PATTERNS

In "Reading Mother Nature's Mind" Ruth Millikan discusses the conflict of opinion between herself and Daniel Dennett regarding their corresponding concepts of substances and real patterns. In trying to focus on their differences, she expresses her opinion that the argument goes back to their diverse notions concerning the design stance and the Intentional stance.

Both stances, initially described by Daniel Dennett, constitute different cognitive interfaces an observer can use in order to interpret a behavioral pattern of an observed system as intentional or as functional. Thus, the Intentional stance provides both the logical and linguistic framework to identify inten-

tional behavior and to recognize exactly those patterns that are accountable for intelligence. Accordingly, the Intentional stance does not enable an observer to explain the underlying cause of the behavior; it rather requires him to take out a “loan of intelligence” while interpreting the patterns – a loan that needs to be repaid by an explanation of the underlying intelligent mechanisms. This is where the design stance comes in: an intelligent mechanism has to be explained as a function the system is designed (i.e. has evolved) to perform.

Because of this loan of intelligence, Millikan considers the design stance to be more basic than the Intentional stance. She argues that patterns of behavior interpreted using the design stance are more local than those interpreted using the Intentional stance. Because of that they are to be regarded as mostly stable and determined in content, relative to certain boundaries. This is why she prefers the term substance to real patterns as she considers a substance to be a natural unit in nature which can be traced over time. As such, substances are ontologically real and distinguished entities which stay, dependent on natural laws, predominantly the same, regardless of the perspective from which they are observed.

In contrast, Dennett’s real patterns ought to be understood as abstract entities that are real in that they really exist, but indeterminate in that they are solely definable in abstract terms and present themselves in different ways from diverse perspectives.

I intend to argue with Wittgenstein and Ryle that the notion of substances as real entities might be considered a category mistake, because no function can ever register an icon as substance. Accordingly, the notion of real patterns seems to be more viable, because:

1. Millikan needs all interpreting functions on every possible level to be fallible to explain hallucinations or the acquisition of empty concepts.
2. Even on the most basic level the interpreting function gets informed through representational icons (pushmi-pullyu’s) – the only entities it can perceive. These entities always need to be interpreted (by the function) by reference to the semantics of the function itself. As Millikan herself notes: the meaning of an intentional icon doesn’t really matter – what matters is solely the semantics of the function which are always context-relative.

PHILOSOPHY OF SCIENCE

Giovanni Ciná

A FORMAL ANALYSIS OF THE BEST SYSTEM ACCOUNT OF LAWHOOD

This talk belongs to a research project aiming at exploring the representational power offered by new developments of formal logic. In particular, in this work I attempt a reformulation of Lewis’ Best System Account, explicitating the underlying formal conception of scientific theories and trying to define explicitly the concepts of simplicity, strength and balance. This presentation is divided in three sections. In the first one I introduce the Best System Account of natural laws and formulate the need for its improvement. In the second section I outline a formal framework where the notions of deductive system and scientific theory can be defined precisely. In the last section the notions of simplicity, strength and balance are analyzed. Several possible definitions are outlined and discussed. To conclude I argue that the framework proposed does indeed provide the precision required. In addition, it also offers interesting insights on the plurality of concepts of simplicity, strength and balance, and on the general enterprise of formalizing scientific theories. The talk is tailored for an audience with at least a basic background in Logic and Philosophy of Science.

Patryk Dziurosz-Serafinowicz
ARE HUMEAN CHANCES FORMALLY ADEQUATE?

It is generally agreed that any concept of chance (objective probability) should be *formally adequate*. Under the predominant view, formal adequacy requires chance to obey the classical Kolmogorov's axioms of probability (see, e.g., Salmon 1967, Eells 1983, Schaffer 2007). This demand has been met by the frequency theory (in its finite and hypothetical version) and by various kinds of the propensity theory. Surprisingly, so far little attention has been paid to demonstrate that the Humean theory of chance (Lewis 1986, Hoefer 2007) is formally adequate.

To alter this situation, this paper argues, in a fairly rigorous way, that the Humean theory of chance meets the demand of formal adequacy. It presents an argument to the effect that once Humean chance is understood as an occupant of the role of an expert whose cognitive goal is predictive accuracy, it follows that it should obey the classical axioms of probability on pain of falling short of this goal. More precisely, the argument goes along the following lines:

1. It is argued that David Lewis' Principal Principle provides a *functionalist* analysis of chance. That is to say, according to the PP, chance is a functional property and is to be identified by playing the role of an expert for agents whose evidence is admissible. I call such chance the *expert chance*. I take it that Humean chances, i.e., regularities figured in stochastic laws of the best-system, are possible *realizers* of the role of an expert chance.
2. To specify the notion of an expert chance, it is claimed that primarily chance should be treated as a cognitive expert, i.e., expert that aims at predictive accuracy. Roughly, the predictive accuracy is taken to be a matter of reducing a 'distance' between an expert's prediction and the possible truth-value assignments (or possible worlds) for a proposition describing some expected outcome of a chancy process. It is, then, argued that an expert chance is predictively accurate if and only if it cannot be outperformed by any other chance in reducing the distance from all possible worlds.
3. If (1) and (2) are correct, then we can prove the following result: Humean chance fills the role of an expert chance properly if and only if it is formally adequate. That is to say, it is predictively accurate if and only if it is formally adequate.

By weaving together chance functionalism and the requirement of predictive accuracy, the argument avoids certain quibbles surrounding Lewis' argument for formal adequacy. While Lewis (1986) argued, in a controversial way, that the vindication of formal adequacy requires to equate chances with *objectified credences*, the argument I defend is silent on the ontological status of chances. This is because once we accept chance functionalism as the correct view on chance, the temptation to think of a chance as reducible to only one sort of entity loses its allure; as far as the functional role is concerned it is left open what the nature of occupants of this role is.

Alexander Gebharter
**MUTUAL MANIPULABILITY: SUFFICIENT FOR UNCOVERING CONSTITUTIVE REL-
EVANCE RELATIONS?**

In this paper we demonstrate on an exemplary mechanism *m* that Craver's (2007a, 2007b) mutual manipulability approach for uncovering a mechanism's constitutively relevant parts (i.e., its components) leads within an interventionist framework à la Woodward (2003)—for which it is explicitly designed—either to false results or to the controversial consequence that a mechanism's behavior is caused by the behavior

of its parts (interlevel causation). We follow (and finally exceed) Woodward (2011) and show step by step how his original interventionism can be extended in such a way that the mutual manipulability approach actually detects the correct parts of our exemplary mechanism as its components, while the counterintuitive consequence that constitutive relevance relations are causal relations is omitted. Though the mutual manipulability approach works as intended within this expanded interventionist framework applied on our exemplary mechanism *m*, we argue that mutual manipulability is generally not sufficient for the existence of a constitutive relevance relation. Because of this we propose a weakened version of the mutual manipulability approach and supplement it by an additional criterion which generates empirical evidence for constitutive relevance relations and allows for confirmation and falsification of hypotheses postulating the existence of such relations.

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Mads Goddixsen

STUDYING DIFFERENCES IN EXPLANATIONS ACROSS SCIENTIFIC DISCIPLINES: AN EMPIRICAL APPROACH

Different disciplines have different standards for what a good explanation is. Educators in interdisciplinary educations need to teach about these differences to improve their students' interdisciplinary skills. This presents philosophers of science with the challenge of spelling out the detailed ways in which explanatory standards differ among disciplines. To answer this challenge, philosophers need to adopt a methodology which enables them to map the explanatory standards that are adopted in actual scientific practice. I argue that the available literature on scientific explanations does not provide such a mapping and therefore needs to be supplemented with studies based on a more empirical methodology. I present an outline of such a methodology and discuss how it can be used to address a number of questions regarding scientific explanations.

Ronnie Hermens

REDUCTION IN GIBBSIAN STATISTICAL MECHANICS

Abstract Statistical mechanics is the branch of physics which uses probability theory to describe and explain the macroscopic behavior of many particle systems (such as gases and fluids) in terms of the mechanical behavior of their constituents. Part of this macroscopic behavior is known to be captured (independently) by the laws of Thermodynamics. In this talk I will lay out some difficulties with the purported explanation of these laws in Gibbsian statistical mechanics.

Outline A central role in Gibbsian statistical mechanics is attributed to the concept of an ensemble. This is an infinite collection of fictitious copies of the system under investigation, in states similar to the state of the actual system. The behavior of the actual system is then described using the average or expected behavior of the ensemble (ascribing to each of the copies an equal probability). The probability of a particular event is then the relative frequency with which the event occurs in the ensemble.

A consequence of this approach is that the probability depends on the precise description of ‘similarity’ adopted when determining the ensemble. This constitutes a form of underdetermination which appears to invite a subjective element into statistical mechanics. In some approaches this subjectivity is embraced. The choice of a particular ensemble then reflects our ignorance about the pure mechanical state of the system rather than some objective feature of this system. A fictitious state is then similar to the actual one if it is compatible with the available information about the actual state.

However, there is a tension with this view and most programs that consider the reduction of thermodynamics. In the latter, probability is adopted as a primitive concept. This allows for the explication of thermodynamic concepts not only in mechanical terms, but also in terms of probability distributions on mechanical states. This is in particular the case in Gibbsian statistical mechanics in which both temperature and entropy are associated with the ensemble description rather than with the mechanical state description. And this seems to be in sheer conflict with the subjective approach unless one is willing to accept that temperature and entropy depend on one’s ignorance. It would thus seem that, if one wants to explain thermodynamics by coupling thermodynamic concepts to probabilities, one has to view these probabilities as objective chances that in some sense exist independent of the observer.

This approach however faces its own problems. The most prominent being the question of how these objective chances are to be reconciled with the deterministic laws of mechanics. Often, these chances are understood as actual relative frequencies in a particular reference class (as opposed to the fictitious relative frequencies mentioned earlier). However, one then faces the problem of how to select an appropriate reference class. In essence, this boils down to motivating the use of a particular ensemble. If this choice is based on whether it can be used to capture the laws of thermodynamics, this capturing provides a poor explanation of these laws. Thermodynamics is then used to explain the probabilities in statistical mechanics: a reversal of the reduction. The upshot is that, in Gibbsian statistical mechanics, the choice between explaining probabilities or the laws of thermodynamics constitutes a proper dilemma.

Stefan Mendritzki

A TALE OF N SOLITUDES? TRADE-OFF INDUCED PLURALISM AND NON-UNIFICATORY INTERACTION

This paper argues that the existence of modelling trade-offs in the social sciences implies that strong pluralism, in the sense of unification, is off the table. However, the lack unifying interaction does not imply a retreat to isolated domains, as several non-unifying forms of interaction are still available.

I have previously argued for the applicability of the concept of trade-offs (Matthewson 2011) to economics, and the social sciences more generally. The argument goes that ontological heterogeneity implies a trade-off between generality and empirical fidelity. Models which aim for different criteria (because they solve the generality-empirical fidelity trade-off differently), cannot be given differential normative status in any general sense. This paper will take these arguments for granted and focus on the implications of trade-off driven criteria pluralism.

One of the more unpalatable outcomes of criteria pluralism is what Marchionni (2006) has called ‘weak pluralism’. The separate but equal approach to pluralism seems to imply the isolation of given domains from interaction. If different ways of solving the trade-off (i.e. particular trade-off constrained choices of criteria) define unique domains, then the results of other domains, even if they share an overlapping subset of targets, are largely irrelevant. Then, for example, general results in models of consumer behaviour have little bearing on the evaluation of models of coffee consumption and vice versa. In this view, one has a potentially infinite array of non-interacting domains. The picture is of *n* solitudes.

I will argue that this picture is overly bleak. The pessimistic argument only holds if interaction is interpreted in the strong sense of unification. Trade-offs do indeed suggest that generality focussed models

will not unify more empirically-focussed models in a strong sense (e.g. derivation). There are other pathways of mutual influence which are open in spite of irreducible disunity. For example, influence can be read out in terms of constitutive or grounding relationships in the spirit of Darden and Maull (1977). That is, a more general model can be the constitutive basis of less general models. This constitutive basis can be drawn a number of different ways, including background assumptions, concepts, computational approaches, etc. Here, more general representations constitute a grammar that can be applied to more specific cases, without any assurances that the grammar is complete or appropriate thereto. Traveling in the opposite direction, more empirically focussed models can provide grounding to those that are rather more abstract. If concepts (inferences, etc.), which have undergone several layers of abstraction, can still be fruitfully mapped onto the more concrete targets of descriptive models, it provides some reason to believe that the robustness of the inferential transfer between model and target remain relatively intact.

Clarification of what non-unifying interaction looks like might help foster more productive interactions. Rather than convincing empirically-focussed modellers that a general model is correct (or even the best available, currently or ideally at a given level of generality), there might be a more tool-box, piecemeal approach. Conversely, rather than convincing more generality-focussed modellers of the details of an approach, one might focus on the degree of compatibility of specific instances with potential general approaches.

Insa Röpke

MECHANISTIC DESCRIPTIONS AND PERSPECTIVAL KNOWLEDGE

The notion of a “mechanism” is much discussed in recent philosophy of biology. It is said to be central to an adequate philosophical understanding of the biological sciences (e. g., Machamer et al. 2000: 3) because mechanisms are used for scientific explanations in, at least, neurobiology, molecular biology (e. g., Machamer et al. 2000, Bechtel and Abrahamsen 2005) and cognitive neurosciences (e. g., von Eckhardt and Poland 2004, Craver 2007).

Several approaches try to define an appropriate notion of a mechanism, but none of these have brought about a consensus yet. However, the aim of this talk is not to give a comparison of these accounts or to endorse any of them. Rather, I will examine the role of perspective in thinking about mechanisms and I will discuss how to deal with this perspectival element.

In the talk, I will first provide short summaries of three recent mechanism accounts (Bechtel and Abrahamsen 2005, Glennan 2002 and Machamer et al. 2000) before demonstrating in how far perspective plays a role in thinking about mechanisms. Here, it is argued that mechanistic descriptions are inherently perspectival. In contrast to Lindley Darden (Darden 2008: 960) and Carl Craver (Craver 2012), I will not claim the reason behind this to be that describing mechanisms depends crucially on us deciding that a phenomenon is of interest. Rather I will argue that it is perspectival because the parts and activities of a mechanism and thus the mechanism itself can only be identified by means of the phenomenon of interest. Conceptually, discovering and describing mechanisms requires that we first select a phenomenon/behavior or the function of interest. We can identify the parts and activities of a mechanism and thus the mechanism itself only with regard to that phenomenon. Hence, claims about the parts and activities observed cannot be detached from the means of observation, being the phenomenon of interest. They are essentially relative to it. Finally, I will discuss two ways of dealing with this perspectival element. Based on the work of Matthew Ratcliffe (Ratcliffe 2000), the first option could be to treat mechanistic descriptions as methodological devices. This option will be rejected. One reason behind this is that mechanistic descriptions are not exposed to arbitrariness because they are constrained by certain aspects. Another is that we can test their adequacy empirically. Instead, I will argue that the influence of perspective could be acknowledged by using Ronald Giere’s concept of “perspectival knowledge” (Giere

2006). In doing so, we can capture both the perspectival element of mechanistic descriptions and the idea that they nonetheless describe a part of the causal structure of the world. This means, in effect, that we can gain knowledge by describing the world in terms of mechanisms even if that knowledge is only perspectival.

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Nahuel Sznajderhaus

THE NONEXISTENT COPENHAGEN INTERPRETATION OF QUANTUM MECHANICS

The so-called “Copenhagen Interpretation” (CI) appears in the literature as the interpretation of quantum mechanics supported by the majority of physicists and due mainly to Niels Bohr. However, according to Don Howard (2004), the fundamental ideas about this “interpretation” of the quantum formalism differ significantly from Bohr’s own ideas. Furthermore, Howard stresses, there was no such unitary point of view among the founding fathers and the CI was invented by Heisenberg in 1955 and then promoted years later by the opponents of the Copenhagen spirit. In this work we will review the historical arguments posed by Howard, and then we will support the thesis of the implausibility of a commonly accepted interpretation of the quantum theory from a philosophical point of view, discussing the deep differences in the thought of Bohr, Pauli and Heisenberg. Finally, we will analyze and criticize how the CI is exposed in the main texts currently used at graduate courses of Quantum Mechanics.

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Melissa Vergara-Fernández

DEALING WITH INCONSISTENCY IN THEORIES AND MODELS

Inconsistency is obviously regarded as an undesirable feature of theories and models for results derived from them are unreliable; any other result could have been derived as well. This thus seems like a rather straightforward manner to distinguish between theories and models that are useful from those that are not. In fact, the distinction is made by scientists—and was made by the logical positivists as well—in order to make normative claims about what is to be accepted as the methods of enquiry of the disciplines.

There is, however, a twofold problem with taking such a straightforward manner to judge between useful and useless theories and models. Firstly, the history of science has shown us that there are some inconsistent theories that have allowed scientists to make accurate predictions or provide accurate explanations about a particular phenomenon. Two examples are Newtonian cosmology and classical electrodynamics. The second, related part of the problem is that the conception of theories and models has changed from that the logical positivists used to have. This not only means that the concept of consistency/inconsistency has been modified, but also that it is more difficult to detect.

In consequence, in this paper I provide a new taxonomy of inconsistency, based on several distinct cases of inconsistency as discussed in the literature and in a recent view of theories and models (Cartwright, Giere). Such taxonomy tries to capture the different cases of inconsistency that might arise in theories and models. Furthermore, I argue that how problematic the issue of inconsistency is, depends on the specific case of inconsistency. In particular, I distinguish between theories and models and between internal and external (in)consistency. I argue that internal inconsistency and model inconsistency are the most problematic cases, yielding the internal inconsistency of models the most problematic case. This, however, does not mean that internally inconsistent models ought to be discarded, as there are cases like the Bohr model of the atom that despite having an internal inconsistency, yielded a correct description of the energy levels in the atom.

In order to illustrate the taxonomy more clearly and to defend the claim that how problematic the case is depends on the type of inconsistency, I use an example from economics. More specifically, I examine a criticism made by Hoover (2010) of the use of the assumption of the representative agent in perfectly competitive markets as an inconsistent assumption in business cycle models. Here I argue that despite the

inconsistency prima facie looking like a model-internal inconsistency—the most problematic case—it is in fact a model-external consistency. This, overall implies that if my taxonomy is at least a starting point to discuss consistency in a more general way, rather than on a case by case basis, we can be more precise about its implications and the ways in which we can trade it off with other epistemic values—if at all.

Philippe Verreault-Julien

THE EXPLANATORINESS OF ECONOMIC MODELS: A CASE FOR UNDERSTANDING

Some economic models, despite being heavily idealized or unrealistic, are considered explanatory. They seem to capture something right about the world we live in, like Schelling's (1978) model of residential segregation. However, their controversial capacity of faithfully representing the world and the fact that our best theories of scientific explanation precisely require such faithful representation has been qualified as the 'explanation paradox' (Reiss 2012). There thus seems to be a gap between the different beliefs we hold concerning the explanatoriness of economic models.

I argue that the alleged explanatoriness of economic models can be accounted for in terms of understanding. While an explanation was traditionally seen as a sufficient and necessary condition for scientific understanding, recent accounts both in epistemology and in philosophy of science suggest that a wedge can be driven between explanation and understanding. If this is correct, understanding could be obtained without passing through an explanation. Drawing on a typology proposed by de Regt and Dieks (2005) and de Regt (2009), I first show that typical economic models provide a feeling of understanding and include a pragmatic dimension, hence contributing to the conflation between understanding and explanation. Indeed, the feeling or sense of understanding and the ability to use economic models should not be considered as sufficient conditions for genuine scientific understanding. Models also have to meet other epistemic requirements like providing knowledge of causes, unification, necessity, or possibility. In the second place, I show that such economic models can yield a type of knowledge that is given by scientific explanations, namely knowledge of possibility. Unlike genuine explanations, however, the knowledge gained is not necessarily actual of our world, therefore making the explanation only potential. Economic models can thus be conceived as potential explanations that depict more or less relevant possibilities about the world. Following Grüne-Yanoff (2009) and Lipton (2009), I argue that these possibilities, while not true of our world, can nevertheless improve our understanding. This is because knowledge of possibility informs us about the modality of the actual explanation. Knowing a possibility can either enrich the list of different alternative explanations we have or contradict an impossibility thesis scientists hold about a given explanation. In both cases knowledge of possibility can yield understanding without actually explaining phenomena.

If the preceding is correct, the notion of understanding could be used to illuminate how economic models can afford some presumed cognitive benefits without actually explaining economic phenomena. This would shed light on the alleged explanatoriness of economic models, an ongoing and controversial issue.

Terry A. Wilkinson Jr

EXPLANATION, VARIABLES, AND POSSIBLE WORLDS SEMANTICS

Both the standard Deductive-Nomological model and the Inductive-Statistical model of explanation have several misgivings when coming to terms with the scope and role of explanatory generalizations. Several analytic methods have been proposed to delineate the boundaries of these general statements, but arguably the most successful of these methods involves limiting the domain of a generalization's representative variables through adherence to various forms of counterfactual statements. In this way, the explanatory

depth of a generalization is restricted, or expanded, to the domain of object members for which some given counterfactual statement is satisfied.

For generalized statements, the traditional counterfactual method encompasses a very large object domain, represented by “other object” counterfactual statements such as, “If some object *o* were to have property *A*, then it would have property *B*”, which corresponds to the lawlike statement “All *A*’s are *B*’s”. However more recently, James Woodward and Christopher Hitchcock have suggested that the more limited object domain of the intervention counterfactual is a better indicator of the proper limits of explanatory depth. Take for example the intervention counterfactual, “If the value assigned by the variable *X* to an object *o* were to be changed via an intervention, then the value assigned by *Y* to *o* would change in some way predicted by the corresponding generalization.” This counterfactual is only true for a single object *o* in many possible worlds where the variables *X* and *Y* exhibit the relationship described in our generalization, whereas the previous “other object” counterfactual is satisfied by a much larger domain of objects in the many possible worlds where those objects have the properties *A* and *B*.

Because of the wide range of domain sizes with regard to objects and possible worlds within these counterfactual statements, and because the use of these statements is motivated by the need to find the explanatory limits of our generalizations, it seems quite natural to expect that giving a modal account of the variables used in our explanatory generalizations may help to better define the involved object and possible world domains. In so doing, we may also make clearer the boundaries of explanation for the corresponding generalized statements.

Hence, in this presentation I will propose a way of viewing variables in a possible worlds semantics that encompasses the intentions of both “other object” and “interventionist” counterfactuals by showing that there exists an important connection between the domain of such variables and the explanatory scope of their corresponding generalizations.

List of speakers

page

Keynote Speakers

2

Patrick Blackburn	University of Roskilde
John Dupré	University of Exeter
Hannes Leitgeb	LMU Munich
Ruth Millikan	University of Connecticut

Epistemology

4

Alba Amilburu	University of Basque Country
Coos Engelsma	University of Groningen
Jie Gao	University of Edinburgh
Harmen Ghijsen	KU Leuven
Pieter van der Kolk	University of Groningen
Jakob Koscholke	University of Oldenburg
Michael Schippers	University of Oldenburg
Sidra Shahid	University of Amsterdam
Tom Sterkenburg	University of Groningen
Sander Verhaegh	University of Groningen
Isabel Verkes	Utrecht University
Jan Willem Wieland	Ghent University

Logic

11

Sander Beckers	KU Leuven
Catrin Campbell-Moore	LMU Munich
Peter Fritz	University of Oxford
Ansten Klev	Leiden University
Sergi Oms Sardans	University of Barcelona
Marta Sznajder	LMU Munich
John Wigglesworth	City University of New York

Metaphysics

16

Stefano Catelan	Durham University
Juliusz Doboszewski	Jagiellonian University
Ben Liao	University of St Andrews
Melissa MacAulay	Western University
Rik Peters	University of Amsterdam
Carlo Rossi	University of Cambridge
Antje Rumberg	Utrecht University
Mattia Sorgon	University of Milan
Martin Vacek	Slovak Academy of Sciences
Martijn Wallage	King's College London
Erik van Zwol	University of Amsterdam

Philosophy of Language

25

Katharina Felka	University of Hamburg
Sanna Hirvonen	University College London
Franziska Köder	University of Groningen
Karolina Krzyżanowska	University of Groningen
Johannes Marti	University of Amsterdam
Andrei Nasta	University of East Anglia
Naomi Osorio-Kupferblum	University of Vienna
Stellan Petersson	University of Gothenburg
Giulia Pravato	Ca' Foscari University of Venice
Stefan Rinner	University of Salzburg
Ravi Thakral	University of St Andrews
Jacek Wawer	Jagiellonian University

Philosophy of Mind

34

Benjamin Andrae	Munich School of Philosophy
Elvira di Bona	Jean Nicod Institute & Vita-Salute San Raffaele University
Patrick Butlin	King's College London
Frank van Caspel	Open Universiteit in the Netherlands
Leon Geerdink	University of Groningen
Jasper van den Herik	Erasmus University Rotterdam
Kinga Jęczyńska	University of Warsaw
Jens van 't Klooster	University of Antwerp
Jakub Ryszard Matyja	Polish Academy of Sciences
Stefan Pliquet	University of Amsterdam
Rebecca Spindler	Humboldt University

Philosophy of Science

42

Giovanni Ciná	University of Amsterdam
Patryk Dziurosz-Serafinowicz	University of Groningen
Alexander Gebharer	Heinrich Heine University of Düsseldorf
Mads Goddixsen	Aarhus University
Ronnie Hermens	University of Groningen
Stefan Mendritzki	Eindhoven University of Technology
Insa Röpke	Bielefeld University
Nahuel Sznajderhaus	University of Leeds
Melissa Vergara-Fernández	Erasmus University Rotterdam
Philippe Verreault-Julien	Erasmus University Rotterdam
Terry A Wilkinson Jr	Tilburg University

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