Philosophy of Science in a Forest (PSF 2013)

Thursday afternoon 23 May – Saturday morning 25 May 2013
International School for Philosophy (ISVW)
Leusden (near Amersfoort)
The Netherlands

NVWF

NEDERLANDSE VERENIGING VOOR
WETENSCHAPSFILOSOFIE

Conference Information

Organisation and Programme Committee
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Directions

Public Transport
From train station Amersfoort Central Station please take:

A regional cab service.
The so-called *Regiotaxi* costs about € 5,00 per person per ride. You can call them up to 1 hour before your arrival at the taxi stand before Amersfoort station (tel. 0900-1122 445), or wait in front of the station: taxi's will arrive there frequently, but expect to wait at the stand 15–30 minutes. For return to the station, please ask at the desk of the ISVW. One taxi is permitted to have 3 passengers, no more.

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You can rent a bike at the bike shop of *Eckman* on the station square for € 6,- per day. You will need your ID and a € 50,- deposit. Use Google Maps for the route or ANWB route planner (http://route.anwb.nl/routeplanner/) to Address of ISVW (see above).

Car
Use your TomTom to go to the Address of ISVW, or else use route planner of the ANWB (http://route.anwb.nl/routeplanner/): Dodeweg 8, 3832 RD, Leusden, The Netherlands.

Overview Programme

Thusday 23 May 2013

12:30 Arrival
13:00 Opening Conference
13:10 Plenary Lecture I (T. Knuuttila).
14:15 coffee and tea [15 min.]
14:30 Parallel Session A [2 parallel sessions of each 3 talks]
16:45 drinks and snacks [15 min.]
17:00 Parallel Session A cont. [2 parallel sessions of each 2 talks]
19:30 Dinner
21:00 Philosophy at the bar

Friday 24 May 2013

08:00 Breakfast
09:00 Parallel Session B [2 parallel sessions of each 2 talks]
10:30 coffee and tea [30 min.]
11:00 Parallel Session B cont. [2 parallel sessions of each 3 talks]
13:00 Lunch
14:00 Plenary Lecture II (S. French)
15:00 coffee and tea [15 min.]
15:15 Parallel Session C [2 parallel sessions of each 2 talks]
16:45 drinks and snacks [15 min.]
17:00 Parallel Session C cont. [2 parallel sessions of each 3 talks]
19:30 Dinner
21:00 Algemene LedenVergadering NVWF (General Assembly DSPS, in Dutch).
21:30 Philosophy at the bar

Saturday 25 May 2013

08:00 Breakfast
09:00 Parallel Session D [2 sessions of each 3 talks]
11:15 coffee and tea [30 min.]
11:45 Plenary Lecture III (C. Werndl)
12:45 Closing of the Conference
13:00 Lunch

Plenary Lectures in Van EedenZaal.

I. Tarja Knuuttila (University of Helsinki, Finland).
Models As Experimental Objects: Constructing Genetic Circuits.
Chair: M. Boon.

II. Steven French (Leeds University, United Kingdom).
Between Braque and Beethoven: Theories as Representation and as Objects.
Chair: F.A. Muller.

III. Charlotte Werndl (London School of Economics, United Kingdom).
Calibration and Confirmation of Climate Models.
Chair: E. Schliesser.

Thursday 23 May 2013

Parallel Session A

	■ **MennickeZaal (Zaal: Room)**	● **ReimanZaal (Zaal: Room)**
A1 14:30 – 15:15	J.-W. Romeijn *Cormorbidity and Psychiatry*	P.P. Kirschenmann, *Is Life Essentially Semiosis?*
A2 15:15 – 16:00	I. Votsis *Objectivity in Confirmation*	J. Witteveen *Metaphysics of Type-Specimens*
16:00 – 16:15	15 min. break	
A3 16:15 – 17:00	S. Lutz *Quasi-Truth and Ramsey Sentence*	A. Borghini *Fruits and Natural Kind Pluralism*
A4 17:00 – 17:45	S. Wenmackers *Determinism and Norton's Dome*	L. Fahrbach *Confirmation without Rivalry*

Friday 24 May 2013

Parallel Session B

	■ **MennickeZaal**	● **ReimanZaal**
B1 09:00 – 09:45	D. Aguilar Gutierrez *Aharonov-Bohm Effect & Non-Loc.*	K. Watanebe *Moral Judgments, Kinds of People, ...*
B2 09:45 – 10:30	J.R. Mendes *New Experimentalism*	A. Ruzzene *Knowledge Use and Case Studies*
10:30 – 10:45	15 min. break	
B3 10:45 – 11:30	M.H.H. Tromp *Artificact Design*	H. Philipse *Science versus Religion*
B4 11:30 – 12:15	W. Houkes *Robustness and Techn. Models*	Th.A.F. Kuipers *Nomic Truth-Approximation*
B5 12:15 – 13:00	W.J.A. van der Deijl *Sounds Evidence without Theory*	H.W. de Regt *Understanding without Realism*

Parallel Session C

	■ **MennickeZaal**	● **ReimanZaal**
C1 15:15 – 16:00	V.A. Gijsbers, L.C. de Bruin *Agency-Interventionist Causation*	R.C. Hillerbrand *Computer Simulations …*
C2 16:00 – 16:45	**cancelled**	J. Diekemper *Spatial and Temporal Reductionism*
16:45 – 17:00	15 min. break	
C3 17:00 – 17:45	I. Mihai *Euler and d'Alembert on …*	J.J. Everett *Cassirer and Structural Realism*
C4 17:45 – 18:30	M.E. van Strien *Origins Laplacian Determinism*	S. Krebs, B. Jiménez, *Romanticism and A. von Humboldt*
C5 18:30 – 19:15	K. Verelst *Kantian A Priori in Exp. Observ.*	M. Poznik *Varieties of Misrepresentation*

Saturday 25 May 2013

Parallel Session D

	■ **MennickeZaal**	● **ReimanZaal**
D1 09:00 – 09:45	M.P.M. Franssen *Systems in Technology …*	F. Pero *50 Years of the Semantic View …*
D2 09:45 – 10:30	C. Matta, *Ontic Structural Realism in Ed...*	D. Zarebski *Dimensions of Internal Reprsesent..*
D3 10:30 – 11:15	J. Rosaler *Reduction, Emergence in Physics: Dynamical Systems*	J.M. Sprenger *Popper a Bayesian?*
11:15 – 11:45	30 min. break	

☛ Chairs of Parallel Sessions

Chairs of Talks in Parallel Sessions: Speakers themselves, by cyclic permutation per part of a parallel session between begin and break, and between break and end, per Room.

- Rule of thumb: you chair the talk after your own talk.

- If your talk is just **before** a break in the midst of a Parallel Session, then you chair the very **first** talk of the Parallel Session.

- If your talk is the **last** one of the entire Parallel Session, then you chair the **first** talk after the break (A, B, C), or the very **first** talk (D).

Example

Parallel Session A: A1, A2, A3, break, A4, A5, in ■ MennickeZaal.

Talk A1 chaired by speaker of A3.
Talk A2 chaired by speaker of A1.
Talk A3 chaired by speaker of A2.

break

Talk of A4 chaired by speaker of A5.
Talk of A5 chaired by speaker of A4.

The same for ● **ReimanZaal**, and the same for Parallel Sessions B, C and D.

✸ If for some reason or other you cannot chair a talk, or do not want to chair a talk, then please contact F.A. Muller or E. Schliesser (who will be around).

ABSTRACTS OF PLENARY TALKS

I. Tarja Knuuttila (Thursday, Van EedenZaal)
Models As Experimental Objects: Constructing Genetic Circuits.

There is a growing body of literature that considers the features which modeling and experimentation share with each other. I will discuss whether *models* can be understood as *experimental objects* through the practice of synthetic modeling in biology. The synthetic models are engineered genetic circuits that are built from genetic material and implemented in a natural cell environment. As such they can be considered as experimental objects that share the characteristics of both models and experiments. Thus the analysis of this hybrid nature of synthetic models reveals some distinctive features of modeling and experimentation, respectively.

I also show how the triangulation of mathematical and synthetic modeling has led to new insights that would have been difficult to generate by either modeling or experimentation alone.

II. Steven French (Friday, Van EedenZaal)
Between Braque and Beethoven: Theories as Representation and as Objects.

What are *theories* like? The current cottage industry in accounts of representation assumes they are like paintings, with similarities and examples thrown back and forth (although some, like Muller, have questioned the relevance of examples from art when it comes to representation in science). On the other hand, some, like Popper, famously, have taken them to be like musical works, in the sense of abstract yet causally effective objects 'living' in World Three.

Here I explore the similarities between theories and artworks as objects in this context, emphasising the problems generated by accommodating the heuristics of theory development. I shall suggest that one way to avoid these problems is to stop thinking of theories (and artworks) as objects at all, building on recent work by French and Vickers (*British Journal for the Philosophy of Science*, 2012). I shall then return to consider again the nature of scientific representation following this suggestion before concluding with a general reflection on the traffic between the philosophy of art and the philosophy of science.

III. Charlotte Werndl (Saturday, Van EedenZaal)
Calibration and Confirmation of Climate Models.

I argue that concerns about double-counting – using the same evidence both to calibrate or tune climate models and also to confirm or verify that the models are adequate – deserve more careful scrutiny in climate modelling circles. It is widely held that double-counting is bad and that separate data must be used for calibration and confirmation.

I show that this is not true, and that climate scientists may be confusing their targets.

My analysis turns on a Bayesian/relative-likelihood approach to incremental confirmation. According to this approach, double-counting is entirely proper. I go on to discuss plausible difficulties with calibrating climate models, and I distinguish more and less ambitious notions of confirmation. Strong claims of confirmation may not, in many cases, be warranted, but it would be a mistake to regard double-counting as the culprit.

ABSTRACTS OF CONTRIBUTED PAPERS

In order of appearance during parallel sessions in ■ MennickeZaal and ● ReimanZaal.

Parallel Session A (Thursday Afternoon 23 May)

■ MennickeZaal, A1, Thursday, 14:30 – 15:15 h.

Jan-Willem Romeijn
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What Comorbidity Tells about Diagnoses in Psychiatry

The frequent occurrence of comorbidity – the presence of two or more disorders in one individual – is one of the issues puzzling professionals and researchers in psychiatry. High rates of comorbidity are reported regularly. Epidemiological studies suggest that up to 45% of psychiatric patients satisfy the criteria for more than one disorder within one year (Bijl 1998, Jacobi 2004, Kessler 2005). Examples of disorders co-occurring frequently are depression and generalized anxiety disorder (Andrews 2002). Patients suffering from both of those disorders tend to have a poorer prognosis and a disproportionally higher functional disability than patients suffering from only one disorder (Schoevers 2005). Comorbidity's high prevalence and its influence on disease severity and treatment programmes make it an important subject to study.

Comorbidity is indeed hotly debated in psychiatry. One debate concerns the question whether comorbidity is problematic for the validity of the current diagnostic system, the DSM-IV (Kendell & Jablensky 2003), and whether it can be used to reclassify disorders (Andrews 2009). In a previous paper we showed that all parties in this debate share particular assumptions on disease models and causality (Van Loo 2012). A related debate concerns the reality or artificiality of comorbidity. Some authors argue that high comorbidity rates are a by-product of our current diagnostic system only, and can be traced back to conventions in the classification choices (Maj 2005, Vella 2000, Aragona 2009). For instance, if we make our classification system more fine-graned and include more diagnoses, it becomes more probable that individuals have more than one disorder (Batstra 2002, Vella 2000, Maj 2005). Against this, other researchers in psychiatry contend that comorbidity is a real phenomenon tied up with the nature of psychiatric disease itself, pointing to commonalities in the causal background of different disorders (e.g. Andrews 2009). According to these authors high comorbidity rates are "real expectable features of [the] psychiatric domain" (Zachar 2009, 13; Zachar 2010).

Our paper focuses on the question to what extent comorbidity is due to conventions in the classification system, or a real phenomenon in the psychiatric domain. We will argue that neither view can fully explain the high rates of comorbidity, and that a middling position provides more insight into the nature of psychiatric diagnosis. We contend that the status of the DSM is best compared to that of geometry for physical space: it offers a robust picture of reality, but only relative to a number of coordinative definitions (cf. Reichenbach 1958). This position is illustrated by an empirical study: using data from the Netherlands Mental Health Survey and Incidence Study (NEMESIS, Bijl 1998) we show that comorbidity cannot be the result of classification choices only, nor of causal structures underlying psychiatric disorders. Finally, we confront these insights with the opposition between realists and constructivists (cf. Hacking 1999) concerning mental health, and argue that our middling position provides a more fruitful starting point for improving treatments and furthering research than positions towards the endpoints of the realist -constructivist spectrum.

References

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● **ReimanZaal, A1, Thursday, 14:30 – 15:15 h.**

Peter Kirschenmann
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Is Life Essentially Semiosis?
Biosemioticians oppose the dominant physico-chemical molecular-biological approach to life. They regard many, if not all, organic processes as semiotic processes, processes involving "signs", "information", "representation" or even "interpretation". I am rather skeptical or critical about their views. Given the growing diversity of their specific views, I can consider only a few of their ideas, some being all-encompassing, others more detailed.

I criticize the global idea that "all life is semiosis" and also the view, used to back up this global idea, namely that the concepts of function and semiosis are coextensive. Among other things, I suggest that such views confuse means and ends. A related and very intriguing idea is that all biological and psychic processes, as teleological processes, have a quasi-semiotic relationship to an "absent content". I argue that explanations should refer to actual, present factors. Another proposal, which is meant to avoid bothering questions of where there could be interpretation in "biological semiosis", is to regard biological processes like protein synthesis as "manufacturing semiosis". I oppose this view as well as the other biosemiotic views with my own ideas about emergent forms of structural determination and co-determination in biology.

■ **MennickeZaal, A2, Thursday, 15:15 – 16:00 h.**

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Objectivity in Confirmation
The study of confirmation is the study of the conditions under which a piece of evidence supports, or ought to support, a hypothesis as well as of the level of that support. There are two major kinds of confirmation theories, objective and subjective. Objective theories hold that confirmation questions are settled via purely objective considerations. Subjective ones hold that at least some non-objective considerations come into play. With some exceptions (see, for example, Williamson 2010), most confirmation theorists nowadays opt for subjective theories. The pessimism over objective theories is most probably due to the fact that it has proved very hard, some may even say impossible, to find reasonable principles that decide every question about confirmation in purely objective terms. The aim of this talk is to reverse some of that pessimism by putting in place some cornerstones in the foundations for an objective theory of confirmation. This is achieved by considering lessons not from the failures of subjective theories, which, no doubt, there are many, but rather from the failures of predictivism, a mini theory of confirmation that is typically conceived of as objective.

We begin the discussion with a widely accepted challenge, to find out what is needed in addition to the right kind of inferential relations in order for a hypothesis to earn some, or more than it would otherwise have, support. The predictivist view is then presented as a way to meet this challenge. In its generic form the view holds that novel predictions ought to provide more, or indeed the only, confirmational support to the hypotheses that issue them. Two families of predictivist views are examined, namely temporal and use-novelty, and dismissed on account of their inability to cope with a number of objections. Particular attention is paid to Worrall's (2006) view of use-novelty, as it appears to be the most sophisticated of the lot. Despite its faults, Worrall's view turns our heads in the right direction by attempting to remove contingent considerations from confirmational matters. This turn culminates in the abandonment of the aforementioned challenge. The talk ends with a proposal of some desiderata that an objective theory of confirmation would need to satisfy if it is going to succeed, desiderata which are motivated by lessons learned from the failures of predictivism. I here cite four:

1. *All* validly formulated questions about confirmation must be supplied unambiguous answers.
2. Confirmational judgments must remain invariant under anything other than the evidence and the hypothesis (plus any auxiliaries) in question.
3. All positive and negative instances of a universal hypothesis possess some confirmational weight.
4. Confirmation from a true evidential proposition E propagates only to those propositional parts of a hypothesis whose truth-value changes if $E's$ truth-value were different.

References

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● **ReimanZaal, A2, Thursday, 15:15 – 15:45 h.**

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The (Historical) Metaphysics of Type-Specimens

In recent years, the notion of a 'type-specimen' in biological taxonomy has come under close scrutiny by historians and philosophers of science. Historians have argued that the function and meaning of type-specimens changed significantly around the late nineteenth century, when type-specimens acquired the "puzzling, even paradoxical" metaphysical status they continue to have to this day (Daston, 2004). Philosophers, who have – independently – also zoomed in on the status of type-specimens in contemporary taxonomy, have been reaching similar conclusions (Haber, 2012; Levine, 2001). The type-specimen, it seems, fulfills a role in taxonomic practice that sits uneasily with popular philosophical accounts of reference and meaning.

I argue that these historical and philosophical conclusions are false. I will show that:
(1) a close examination of the actual taxonomic practice in which type-specimens are involved evinces that there is nothing puzzling about their current metaphysical status, and that

(2) the historical route by which type-specimens acquired this status has been misrepresented in recent historiography of science.

With help of a novel, historical-philosophical account of the type-specimens concept I will demonstrate where misinterpretation has arisen and apparent paradoxes have emerged. Along the way, this account uncovers some important points of broader historical and philosophical interest. On the historical side, it repairs a critical equivocation in the influential historical-epistemological framework formulated by Daston & Galison (2007). On the philosophical side, it teaches us a few new things about the application of the causal theory of reference in scientific practice and about the multiple meanings of a reference standard. Overall, it shows what a fruitful marriage of history and philosophy of science can look like.

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■ **MennickeZaal, A3, Thursday, 16:00 – 16:45 h.**

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Quasi-Truth as Truth of a Ramsey Sentence

The partial structures approach is in the vanguard of the Semantic View on scientific theories and models, and it is one of the main reasons why the Received View on scientific theories as developed within logical empiricism is considered inferior to the semantic view. In my talk I will show that the core notion of the partial structures approach, *quasi-truth*, can be captured very naturally within the Received View. Two other important concepts of the partial structures approach, *partial homomorphism* and *partial isomorphism*, can also be expressed with in the Received View.

These results show that the tools developed within logical empiricism are at least as powerful as those of the partial structures approach. I will further outline generalizations of partial structures within the Received View, allowing imprecise (rather than partial) functions and constants.

[truncated abstract due to overwhelming presence of logical symbolism and WORD conversion troubles.]

● **ReimanZaal, A3, Thursday, 16:00 – 16:45 h.**

Andrea Borghini
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Fruits and Natural Kinds Pluralism

Philosophers of science and scientists have investigated natural kinds for a long time and from different angles. Yet, they have neglected to look into one of the most obvious spheres: foods. In this paper I use the case of fruits to provide a fresh take on pluralism about natural kinds, a take I regard better suited to deal with the nuances that kinds of food confront us with. Fruit systematics is one of the most complex labyrinths in plant biology: it wasn't indeed until 1994 that a comprehensive classification of fruits was put together, thanks to Richard Spjut. Take even just a basic distinction, such as the one between angiosperms and gymnosperms: traditionally, it has been taught that gymnosperms do not bear fruits, yet in the most recent and comprehensive works of Spjut and Stuppy this predicament has been reversed – gymnosperms bear fruits. In this paper I develop a pluralist perspective about natural kinds by analyzing two case studies (out of a few palatable ones) involving, respectively, plants in the *Solanum* genus and nuts. The view will be developed in confrontation and contrast with the literature on pluralism about natural kinds (John Dupré, Paul Griffiths, Ian Hacking, Philip Kitcher, Joe Laporte, …) as well as the literature on social kinds (races, genders, social ontology), showing how both extant versions of pluralism fall short of providing a satisfactory explanation of the case studies.

The *Solanum* genus includes tomatoes, eggplants, and potatoes among others. These are close evolutionarily, although they are quite different for morphology and ontogeny; indeed, various species of tomato plants used to be considered a separate genus and are now regarded as a subgenus of the *Solanum*. What's the real classification of plants included in *Solanum* – the one based on evolution, the one based on ontogeny, or the one based on nutritional properties? Each of those is grounded on genuine causal processes it seems, and giving more priority to one sort of process rather than another seems specious. Nuts too pose some conundrums for the purposes of systematics and taxonomy. Here is a quote from a recent volume by Stuppy and Kesseler (*Fruits*, 2008: 73) exemplifying the issue:

> Many of the fruits that we have just classed as proper nuts qualify only if nothing but the qualities of the mature ovaries are taken into account. For example, fresh walnuts look more like drupes. They are covered by a freshly green husk that peels off easily when the fruits are ripe. […] This may seem to be a rather exceptional case but pseudo-drupes are also typical of members of the oleaster family (Elaeocarpaceae) such as sea buckthorn (*Hippophae rhamnoides*.)

Most fruits escape the pluralist conceptions of natural kinds since they are domesticated species or varieties. At the same time, they arguably are part and parcel of the natural world: the identity of fruit kinds strictly depends on a selected sort of causal processes (such as reproduction, development, digestion), more so than – say – racial kinds, and probably even more so than sexual categories. The goal of the paper is to suggest a theory of natural kinds that is able to accommodate the case of fruits.

■ **MennickeZaal, A3, Thursday, 17:00 – 17:45 h.**

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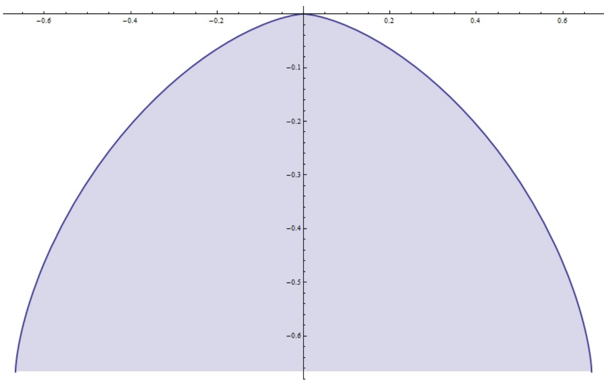
A Deterministic Model for Norton's Dome

1. Norton's Dome

Norton has given a physical interpretation to a non-uniquely solvable Cauchy problem (Norton, 2008), thereby presenting us with an example of indeterminism in Newtonian physics. It involves a gravitational field in which a mass is placed with velocity zero at the apex of a dome, which has the following shape:

$$y(x) = -2/3(1 - (1 - 3/2|x|)^{-2/3})^{-3/2} .$$



For this situation, Newton's second law yields a differential equation involving a non-Lipschitz continuous function. The resulting Cauchy problem has a one-parameter family of solutions, which can be represented geometrically as a "Peano broom":

$$x(t) = 0 \text{ for } t \leq T, \quad \text{and} \quad x(t) = (t - T)^4/144 \text{ for } t \geq T ,$$

where T is a positive real number, which can be interpreted as the time at which the mass starts moving.

The next best thing to a deterministic trajectory would be a probabilistic description for the possible trajectories of the mass. This approach may appear alien to Newtonian physics, but at least it is accepted in classical physics (*viz* in statistical mechanics). However, even such an approach is not available here, since there is no uniform probability distribution over the parameter T, because it has an infinite support, and the assumption of any non-uniform distribution seems *ad hoc*.

2. Hyperfinite Dome

It has been suggested that physical praxis is related more closely to nonstandard analysis than to standard calculus (see *e.g.* Albeverio et al., 1986). Following this suggestion, we can give a hyperfinite description of Norton's dome and show that it yields a deterministic model for a mass on such a surface. Moreover, non-standard analysis allows us to formulate an alternative probability theory in which it is possible to describe a uniform distribution over a variable with infinite support (Benci *et al.*, 2012).

By discretizing the time parameter ($t = n\Delta t$), the second-order differential equation for the function $r(t)$ can be transformed into a second-order difference equation for the sequence rn. This is standard praxis in numerical analysis, which is often used in physics. Given the two initial conditions, $r(0) = 0$ and $r(1) = 0$, the solution is unique: it is the constant sequence $r(n) = 0$, for all n, which corresponds to the trivial solution $r(t) = 0$ in the continuous case. If we give different initial conditions, it produces a different, unique solution.

Within the scope of standard analysis, the discrete approach using difference equations is only an approximation to the continuous case described by differential equations—an approach that improves as the time step Δt decreases. If we adopt a non-standard approach, however, we can choose an infinitesimal Δt, smaller than any strictly positive real number. In such a hyperfinite model, we may consider initial conditions such that $r(0)$ and $r(1)$ are infinitesimal rather than zero (although the standard part of such an infinitesimal is zero). Given that any physical measurement is only finitely precise, it is not possible to distinguish experimentally between zero and infinitesimal quantities. Nevertheless, the resulting trajectories are all different and their standard parts do agree with the family of solutions found in the standard model. Moreover, we are now in a better position to assign probabilities to the standard solutions.

3. Conclusion

The non-standard model can be interpreted as identifying a hidden variable in the standard model, suggesting that reality—at least the reality of classical physics, which is itself an idealization—is 'actually' deterministic. However, this is not our conclusion. Instead, we follow the suggestion by Sommer and Suppes (1997) that non-standard models and models based on the standard reals are empirically indistinguishable from each other. Hence, we have to conclude that (in-)determinism is a model-dependent property. Observe that Werndl (2009) reaches the same conclusion for a different source of indeterminism.

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● **ReimanZaal, A4, Thursday, 17:00 – 17:45 h.**

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How to Confirm a Theory without Considering Rival Theories
Is it possible to confirm a theory by some observation without knowing or considering any concrete rival theories of the theory? Scientists certainly seem to think so. For example, when a theory successfully predicts the precise outcome of an experiment, scientists often judge the theory to be strongly confirmed by the correct prediction, even if they don't consider any concrete rival theories of the theory. However from a Bayesian standpoint this practice seems problematic. One way to formulate the problem is as follows: Given a theory T and some observation D, how can we determine a value for the likelihood $\Pr(D \mid \neg T)$ without considering the concrete theories that make up $\neg T$. In my talk I present and discuss a simple Bayesian analysis that is meant to show how this might be done. The analysis concerns two important kinds of cases involving independence of observations and precision of observations.

My account differs from other Bayesian accounts by using odds of probabilities rather than probabilities themselves, and by focusing on powers of ten, for the reason that I am only interested in rough estimates of probabilities, not in precise numbers. Probabilities and odds are related approximately as follows:

P(T)	…	.0001	.001	.01	.1	.5	.9	99	.999	.9999	…
Pr(T)/Pr($\neg T$)	…	.0001	.001	.01	.1	1	10	100	1000	10000	…

We need Bayes's theorem in ratio form:

$$\frac{\Pr(T|D)}{\Pr(\neg T|D)} = \frac{\Pr(T)}{\Pr(\neg T)} \cdot \frac{\Pr(D|T)}{\Pr(D|\neg T)}$$

For example, let the likelihood ratio $\Pr(D \mid T)/\Pr(D \mid \neg T)$ equal 1000. Then the posterior odds of T are three powers of ten bigger than the prior odds of T, i.e., three steps to the right of the prior odds in the table above.

Independence of observations. Let D_1 and D_2 be two independent observations, e.g., the outcomes of two entirely different kinds of experiments. It is then natural to interpret the independence of D_1 and D_2 as implying that D_1 and D_2 are probabilistically independent conditional on T, and also probabilistically independent conditional on $\neg T$ (e.g., Sober 1995, Fitelson 2001, *pace* Myrvold 1996). This provides a partial answer to the question how $\Pr(D \mid \neg T)$ can be determined without considering concrete rivals of T: if D_1 and D_2 are independent from each other, and we can somehow determine values of the likelihoods $\Pr(D_1 \mid \neg T)$ and $\Pr(D_2 \mid \neg T)$, then we can also get a plausible estimate for the value of $\Pr(D_1 \wedge D_2 \mid \neg T)$, namely $\Pr(D_1 \mid \neg T) \cdot \Pr(D_2 \mid \neg T)$. Then the likelihood ratio of $D_1 \wedge D_2$ is given by:

$$\frac{\Pr(D_1 \wedge D_2|T)}{\Pr(D_1 \wedge D_2|\neg T)} = \frac{\Pr(D_1|T)}{\Pr(D_1|\neg T)} \cdot \frac{\Pr(D_2|T)}{\Pr(D_2|\neg T)}$$

Precision of observations. Assume, for example, that D reports the result of a measurement which is guaranteed to be of a certain order of magnitude and has a precision of 3 decimal places. We can then reason as follows. On the assumption of $\neg T$, any possible measurement result looks like any other possible measurement result. There are 1000 of them. Hence, a reasonable estimate for $\Pr(D \mid \neg T)$ is 10^{-3}. This is a version of the indifference principle, so this is the price we have to pay to get an estimate of $\Pr(D \mid \neg T)$ in this way.

Finally, if we are willing to pay this price, we do get a reward: The combined effect of independence and precision can be very strong. For example, let the prior of T be 10^{-10}, and assume that T correctly predicts four independent observations each with a precision of three decimal places. Then the odds of T receive a boost of three powers of ten by each piece of observation. Because of independence, the four boosts add up to an overall boost of 12 powers of ten resulting in a posterior for T of .99. This shows how it is possible that probabilities of theories move from very small priors to very high posteriors even if concrete rivals of T are not considered. In my talk I discuss the assumptions and limits of this analysis.

Parallel Session B (Friday Morning 23 May)

■ **MennickeZaal, B1, Friday, 09:00 – 09:45 h.**

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Non-Locality and the Aharonov-Bohm Effect: Considerations and Implications
Predicted theoretically in 1959 by Y. Aharonov and D. Bohm, and experimentally confirmed a couple of years later, the Aharonov-Bohm effect (AB effect for short) has just until recently been on the focus of philosophers of science. Interest in this phenomenon is well motivated; the effect, along with its non-abelian correlates lies at the core of any gauge theory and seems to be central to their understanding. For instance, in modern day physics, the description of all the physical interactions believed to be fundamental is given in terms of gauge theories; this makes a complete study of the conceptual implications of the effect especially important for philosophers of physics.

Among the many lines of philosophical inquiry derived from the existence of the AB effect the problem of non-locality seems to be a central one. Not only this problem eludes a clear physical explanation but a conceptual enquiry about it overlaps with one of the central problems in philosophy: the problem of physical causation. The latter is, of course, a vast and complex one. However, the study of the AB effect allows focusing on a more specific question: how can two, apparently unrelated, objects affect each other when there is a spatial or temporal distance, or both, between them?

Healey (Healey, 1997) suggests a solution for the problem of non-locality in terms of a notion of metaphysical holism based on the physical model proposed by Wu and Yang. Opposed to this view, stands the physical model advanced by Aharonov (Aharonov, 1969) and defended by Popescu (Popescu, in terms of 'non-local' modular variables which point in a different direction in interpretative terms. The question arises when we ask about the relation between these two apparently different notions. Surprisingly, one of the first answers point to the existence of two different concepts in which something, such as a process, a system or an interaction, can be said to be non-local. I'll aim here to delineate first a natural interpretation of the AB effect in terms of modular variables; I'll then contrast both proposals and show how a conceptual underdetermination arises from both possible interpretations and sketch an alternative in structural terms.

● **ReimanZaal, B1, Friday, 09:00 – 09:45 h.**

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Moral Judgments, Intentional Actions, and Kinds of People

In a series of his works under the research project of 'Making up People', Ian Hacking has elaborated the concept of 'human kinds' (or 'interactive kinds') as objects of human or social sciences, which sheds light on a curious nature of investigations of our own lives or social phenomena. According to Hacking, one of the most interesting characteristics of such inquiry into human behaviors is that there often occurs a 'looping effect' between the people who classify, typically social scientists who deal with certain categories that serve for their particular studies, and the people who are classified into such categories. This, in a sense, makes human kinds distinct from natural kinds since, in principle, there is no such interaction in natural sciences.

However, critics such as Bogen, Ereshefsky, Cooper, Khalidi have argued against Hacking, doubting that we could draw a clear boundary between human kinds and natural kinds. Although each of their arguments has some good cogency, and therefore they could be able to blur the boundary between those two sorts of kinds, I do not think that this kind of criticisms could have any significant impact on Hacking's entire project. For he even suggests that the concept of 'natural kind' is neither well-defined nor useful and that we should delete it from our philosophical discussions. To draw a clear boundary between human and natural kinds is not his main interest in the first place.

My aim of this talk is to expound on an important feature of human kinds, and indeed Hacking's project, which has unfairly obtained less attention from those critics in spite of Hacking's constant emphases. What makes human kinds interesting is, I contend, the fact that they so often take on moral connotations. This is supported by multiple quotes from Hacking's texts though they certainly need further considerations and explanations.

What I want to show is that the mechanism of the process of 'looping effects' of human kinds could be illuminated further by recent psychological findings. As Joshua Knobe concludes based on his experiments on people's concepts of folk psychology, there are strong reasons to believe that our moral considerations about an agent's behavior affect our judgments as to whether or not the behavior is to be considered 'intentional.' Considering Anscomb's theory of intention on which Hacking depends when he gives an account of 'looping effects', this well explains in detail how our grouping things could 'make up people'. And finally, I will illustrate this mechanism by addressing some empirical findings on gender differences and their implications.

■ **MennickeZaal, B2, Friday, 09:45 – 10:30 h.**

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The Dawning of New Experimentalism in Hacking's Part B of Representing and Intervening

The expression "New Experimentalism" – coined by Robert Ackermann (1989) – been used to refer a movement (e.g., Mayo 1994: 270 and 1996: 58), a tendency (e.g., Chalmers 1999: 182) or a current of thought (e.g., Mosterín e Torretti 2002: 401) that introduced a turn of attention and change of concern in post-positivistic historicist philosophy of science since the first half of the 1980´s, namely from issues about the dynamics of scientific theories to questions regarding the structure of scientific praxis.

Although a stronger consensus about which authors should be counted in as new experimentalists should yet to be reached, we find a sort of entente in the critical literature on the subject (e.g.: Mayo 1994: 270 and 1996: 58; Mosterín e Torreti 2002: 401-2; Bartz-Beielstein 2002: 152) that the philosophers of science with historical proclivity Ian Hacking, Nancy Cartwright, Ronald Giere, and Rom Harré, as also the historians of science with philosophical aspirations Allan Franklin and Peter Galison, represent all six the prominent figures of the movement.

Nevertheless, those authors not only they have different academic backgrounds, but also diverse intellectual projects, with special particular agendas of problems, distinct methods to try to solve them and rather idiosyncratic conclusions. That way, we cannot prevent ourselves to ask: what is characteristic of that movement and makes its identity as such? A possible answer to the question, a persuasive one, I think, states that we can identify a number of fundamental theses shared by all the six referred authors and show how they prevail over whichever doctrinal divergences that may exist between those authors, allowing them to have a joint philosophy of science. Off course, a full demonstration of such view is beyond the scope of my presentation. Instead, I want to expose in it the following more modest argument: Ian Hacking`s part B of *Representing and Intervening* (1983) can be thought of as a sort of programmatic manifesto comprising five theses that became the core of the philosophy of science of the New Experimentalism, after himself, the Canadian philosopher, and the rest of the referred authors developed them through diverse ways, with different focuses, in distinct degrees.

Three of those theses are epistemological in nature: (1) the parity of theorization and experimentation: experimentation is a basic independent domain of scientific activity that has, at least, the same epistemic dignity as theorization; (2) the autonomy of experimental knowledge: science practitioners have at their disposal practical strategies for the legitimation of experimental knowledge that are distinct from those used in high level theorization; (3) the cumulative progress of experimental knowledge: experimental knowledge follows an internal logic of cumulative progress. The other two are ontological: (4) the creation of experimental phenomena: scientific experimental activity creates most of the phenomena it deals with; (5) the implication or realism in scientific experimental activity: science experimental activity entails embracing some kind of realism.

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● **ReimanZaal, B2, Friday, 09:45 – 10:30 h.**

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Making Knowledge Usable by Means of Case Studies
Case studies are typically understood as being helpful either in the context of justification or in the context of discovery (Yin 1994). In the former case they test theories and refute causal hypotheses; in the latter, they point out unexplored theoretical avenues and suggest novel concepts and hypotheses (Morgan 2012). This distinction, however, obscures the fact that case-study research can sometimes be used to refine causal hypotheses that, while regarded as well supported by the available evidence, are poorly informative when practical goals are at hand. Pointing out this third possibility besides the heuristic and testing use of case-study research is beneficial for fields such as development economics that are practice-oriented, and in which the use of case studies is still hesitant and poorly understood (Gerring 2007). The case study is here typically seen as providing evidence either in favour or at odds with causal relationships that are established by means of cross-countries econometrics. In the former case the study is taken to offer additional support to the hypotheses in question; in the latter it would generate evidential dissonance that signals the need of closer scrutiny and further exploration (Rodrik 2001). It is disputable, however, whether evidence from a single study confirms or refutes hypotheses that measure average causal effects across a whole set of countries. Evidence that the causal relation holds in the single case, in fact, tells us whether it is an outlier or not, and says very little about the general claim. It seems that we are left with a case study that suggests at best a new hypothesis, the generalizability of which is, however, strongly disputable.

A third avenue is open, however, to case-study research. It amounts to treating the single case-study as an "entry point" to a typology that partitions cases into types sharing specific combinations of factors (Stinchcombe 1968, Bennett and George 2004). The single case study would in fact provide the evidence from which mechanism-types can ultimately be inferred, and on this ground types of variance in the dependent and in the independent variables categorized. Whereas typologies so constructed have been typically defended as descriptive tools or tools for causal inference (George and Bennett 2004, Gerring 2004), I suggest that they also constitute prima facie evidence for more refined causal claims. Hypotheses established by way of cross-country regressions tend, in fact, to be too coarse for being of help to policy-makers. They typically exhibit what can be described as a failure in proportionality. That is, they fail to convey accurate information about the causal structure and, in so doing, omit relevant details about the alternative states of the cause and the outcome (Diablo 1992, Kendler 2005, Woodward 2010). If they were to ground policy recommendations, then, states of the cause would be regarded as effective that in fact are not, whereas states of the cause that are effective in fact would escape cognisance. Such a failure in proportionality signals the need of a more fine-grained knowledge of the causal structure. The necessary refinement can be accomplished by way of typologies based on case studies which, in so doing, contribute to create usable knowledge for policy-making.

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■ **MennickeZaal, B3, Friday, 10:45 – 11:30 h.**

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Artifact Design, Explored from the Interface of Philosophy of Technology and Philosophy of Cognition

My purpose is to present a framework to improve the understanding of human design of artifacts by searching for answers to questions as: What are the basic cognitive capacities that enable humans to create new artifacts, from hand axes to increasingly complex functional systems? The analysis to be presented builds on the results from the philosophy of technology and on the approach in philosophy of cognition more specifically the action theory. Evolutionary theories of artifact developments are not uncommon, these theories take as a starting point that *most artifacts are designed on the basis of existing artifacts.* Although it can be questioned whether a true evolution concept is applicable in philosophy of technology this starting point is quite commonly accepted. (among many others: (Jonas 1979), (Basalla 1988), (Preston 2009) (Houkes and Vermaas 2010)). This makes designing an activity of transformations and combinations. Before addressing the processes of design transformations the philosophical notion of the artifact as the input of these design processes should be established. The plural functionality analyses of artifacts, the functional -, intentional -, dual nature -, behavior approach of artifact analyses, and the notion of *use plan* as introduced by Houkes en Vermaas, each contribute as input for this analysis. However these inputs are mainly oriented to an external, or a users perspective, or both, and fall short as a base for a more specific cognition oriented analysis of design activities and processes, they need some additions or modifications.

The cognitive knowledge base of artifact design is commonly recognized although the tacit elements did get less recognition, but design is primarily to be considered as an action oriented activity. Action theory is applicable both in the users perspective and in the design activity. The applied action theory in this analysis follows the line of causality as developed by Donald Davidson, Fred Dretske and Allicia Juarrero. (Davidson 1987), (Dretske 1991), (Juarrero 1999). Within that line final cause is identified as a main causal factor in action theory.

However, final cause is insufficient for a causal explanation of artifact design. With a number of examples, from different domains, it will be argued that , in addition to the final cause, the Aristotelian material -, form and unity cause can be identified as factors playing a causal role in the cognitive processes of artifact design. In addition to these well known causalities, net value is identified as an integral decision factor.

In combination with other surrounding conditions a causal cognition reference model will be developed that can be used for further analyses.

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● **ReimanZaal, B3, Friday, 10:45 – 11:30 h.**

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The Real Conflict Between Science and Religion
In his book *Where the Conflict Really Lies*, published in 2011, Alvin Plantinga defends two complementary and thought-provoking claims, to wit:

(a) "*there is superficial conflict but deep concord between science and theistic religion*",

whereas

(b) there is "*superficial concord and deep conflict between science and naturalism*".

In the talk, I shall contest the first conjunct of (a), that is, that there is merely a superficial conflict between science and theistic religion. By focussing on the logical relations between scientific theories and religious beliefs, Alvin Plantinga overlooks the real conflict between science and religion. This conflict exists whenever religious believers endorse positive factual claims to truth concerning the supernatural. They thereby violate an important rule of scientific method, according to which factual claims should be endorsed as (approximately, probably, etc.) true only if they result from validated methods or sources of knowledge.

■ **MennickeZaal, B4, Friday, 11:30 – 12:15 h.**

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Robust tools: An Alternative Assessment of Evolutionary Modelling of Technological Change
The last two decades have seen increasingly influential efforts within the behavioural and social sciences to model processes of long-term change as 'cultural evolution'. Technological change is no exception. Models have, for instance, been built to account for the effects of population size on the accumulation of cultural complexity, with special attention for technological toolkits (Henrich 2004; Powell, Shennan, and Thomas 2009); for analysing differentiation patterns in steam technology (Frenken & Nuvolari 2004); and for examining the relation between competition and overall technological diversity (Saviotti & Mani 1995). These models, like 'cultural evolutionary' models in general, are supported by analogies between natural and cultural items; or by arguments that the basic evolutionary 'algorithm' applies literally, even if cultural and biological change are driven by different micro-mechanisms (e.g., Mesoudi, Whiten, and Laland 2004; Mesoudi 2011). As I will discuss, both maneuvers immunize the models constructed, to a large extent, from the usual conceptual criticisms that notions such as 'selection' and 'variation' cannot have the

same meaning when applied to living and artificial items.

In this paper, I therefore propose a new, more productive method for critically assessing cultural-evolutionary models, as applied to technological change. This method focuses on features specific to technologies which, rather than comprise the conceptual integrity of evolutionary models, may affect their *robustness* – specifically: their structural and representational robustness (Weisberg 2006; Weisberg & Reisman 2008; Houkes & Vaesen 2012). This continues and generalizes a line of inquiry in which Henrich's (2004) and Powell et al's (2009) models of cultural complexity are assessed with respect to their structural robustness (Vaesen 2012).

After outlining the method, I consider one feature of technological change as an illustration: the extent of the 'pool' of existing technologies on which one may draw in both incremental and radical design. These include technologies that plausibly form a lineage, such as several 'generations' of car designs produced by the same firm, but also technologies outside this lineage (rather different car designs produced by another firm), and even remote ancestors - both within and outside lineages (e.g., materials or fuel-saving techniques taken from the airplane industry). Some conceptual criticisms have therefore focussed on the different meanings of 'retention' and 'transmission' when applied to genetic replication and social learning - and have evoked counterarguments that appeal to hybridization in micro- organisms and the transition from prokaryotes to eukaryotes. Even conceptually, the breadth of this feature is better brought out by problematizing the notion of 'generation' or 'lineage'. In terms of modelling, the 'pool-size' phenomenon requires a fundamental adjustment in modelling technique - analytic models, for instance, cannot be straightforwardly based on the recursion equations of population genetics and evolutionary game theory (e.g., the replicator dynamic). More importantly, the phenomenon requires a demonstration of robustness of any evolutionary model of technological change under changes in pool size: implications derived from models that ex ante exclude this feature cannot be relied on, and the models themselves should only be taken as explanatory with substantial qualifications.
Whether or not any evolutionary models are robust with respect to this feature of technological change remains to be seen; yet assessment of models requires specification of appropriate robustness checks - a constructive way to assess evolutionary approaches to technology, to which philosophers of science and technology can actively contribute.

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● ReimanZaal, B4, Friday, 11:30 – 12:15 h.

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Nomic Truth Approximation by Revision of Inclusion Claims

In *From Instrumentalism to Constructive Realism* (2000), I have shown how an instrumentalist account of empirical progress can be related to truth approximation. However, the author assumed that a strong notion of nomic theories was needed for that analysis. In a recent paper I have shown, in terms of truth and falsity content, that his analysis already applies when, in line with scientific common sense, nomic theories are merely assumed to be *exclusion* claims, that is, claims that exclude certain conceptual possibilities as nomic possibilities. In the present paper I will show that truth approximation is also possible by revision of *inclusion* claims, that is, claims to the effect that certain conceptual possibilities are nomic possibilities. It turns out that this form of truth approximation is formally and conceptually even more simple, both in its basic and refined form. Moreover, it will be indicated what kind of scientific theories fit in this framework.

■ MennickeZaal, B5, Friday, 12:15 – 13:00 h.

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Sounds Evidence without Theory

Since the birth of econometrics, practitioners and methodologists have been painfully aware of the limitations of data analysis. In the paper, I discuss one particular limitation: in order to have good evidence for a theory, it is required that we first have a developed theory for which our findings are evidence. It is the goal of my paper to question the soundness of this rule. In order to do so I describe the case of the general-to-specific approach to automatic data mining in econometrics (henceforth *Gets*) which is a breach of this requirement. However, as I shall argue, it is still methodologically sound.
In a first step, the paper examines the view that theory is required for sound evidence. As an example of this view, the Use Novelty charter of John Worrall is discussed. This requirement states that "for data *X* to support hypothesis *H*, *H* should not only agree with the evidence *X, X* must itself not have been used in *H*'s construction." This requirement is motivated by the underdetermination problem of Duhem: an endless amount of theory can explain the observed evidence. In economics, Tjalling

Koopmans ('Measurement without Theory', *The Review of Economic Statistics* **29**(3), 1947) famously defended the necessity of theory for data analysis.

In a second step, *Gets* is discussed. The *Gets* methodology was developed in the past decade and a half, and is an automatic data mine mechanism. Data mining is the practice of studying a data set in order to discover statistical relations in the data. Whereas most automatic data mining algorithms perform very badly in simulation experiments, *Gets* has been very successful in determining the underlying structures within data sets. *Gets*, like other methods of automatic data mining, requires very little pre-formed theory. This has the advantage that it can be used in learning from the data in fields of study where no good theories are available to guide the data analysis.

The main argument in the paper is that *Gets* can provide sound evidence even though it is a breach of the Use Novelty requirement and any other methodological principle that states that data analysis requires a developed theory. Its tremendous success in simulation experiment raise doubts that underdetermination is a genuine problem for *Gets*. If *Gets*' promising simulation results are in anyway indicative of how it performs in real data sets, *Gets* is very successful in finding the right theory that describes the observations. In the paper, Koopmans' worries are discussed and it is argued that they do not establish convincingly that this type of methodology is unsound.

The paper ends with reflection on the importance of the Use Novelty charter in many instances of scientific research, but concludes that it does not work as a general requirement of sound evidence.

● **ReimanZaal, B5, Friday, 12:15 – 13:00 h.**

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Scientific Understanding without Scientific Realism

Scientific realists often claim that the widely accepted view that science provides explanatory understanding commits one to a realist position. They assume that scientific understanding can only be achieved through explanations of which the *explanans* is (approximately) true, which would imply that the theories figuring in the *explanans* must be interpreted realistically. In my paper I will argue against this idea of a necessary connection between understanding and realism.

Study of scientific practice reveals that understanding is often obtained via theories and models that are unrealistic or simply false. For example, many scientific disciplines concerned with complex systems (such as economics or climate science) use highly unrealistic models to achieve understanding of phenomena. The same goes for many mechanisms that figure in biological and neuro-scientific explanations. Feynman diagrams, which are used to understand phenomena in the domain of quantum electrodynamics, cannot be taken as correct representations of reality. Moreover, the pessimistic meta-induction from the history of science forces us to take seriously the possibility that our current best theories are false, which would imply that we do not have explanatory understanding at all.

So we face the dilemma of either giving up the idea that understanding requires realism, or allowing for the possibility that in many if not all practical cases we do not have scientific understanding. I will argue that the first horn is preferable: the link between understanding and realism can be severed. This becomes a live option if we abandon the traditional view that scientific understanding is a special type of *knowledge*, namely knowledge of an explanation (*S* understands *P* iff *S* knows that *T* explains *P*). While this view implies that understanding must be factive because knowledge is factive, I avoid this implication by identifying understanding with an *ability* rather than with knowledge. I will develop the idea that understanding phenomena consists being able to use a theory to generate predictions of the target system's behavior. The crucial condition is not *truth* but *intelligibility* of the theory, where intelligibility is defined as the positive value that scientists attribute to the theoretical virtues that facilitate the construction of models of the phenomena. Intelligibility is not an intrinsic property of theories but a context-dependent value related both to theoretical virtues and to scientists' skills.

I will show, first, that my account accords with the way practicing scientists conceive of understanding, and second, that it allows for the use of idealized or fictional models and theories in achieving understanding, as well as for wholesale anti-realist (or constructive empiricist) interpretations of scientific theories. Contra Van Fraassen, however, I argue that explanatory understanding is an epistemic aim of science. I conclude that scientific understanding is an epistemic aim of science, but that understanding does not require realism. Understanding of phenomena can be obtained via theories or models independently of whether these are true representations of an underlying reality.

Parallel Session C (Friday Afternoon 24 May)

■ **MennickeZaal, C1, Friday, 15:15 – 16:00 h.**

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An Agency-Interventionist Account of Causation
How do we acquire causal knowledge? An influential answer to this question that has become quite popular recently can be found in James Woodward's interventionism (Woodward 2003). At the core of Woodward's theory is the idea that causation should be analysed in terms of intervention: roughly, *X* causes *Y* if and only if there is a possible intervention *I* on *X* that changes the value of *Y*. Much of the theory's work is done by conditions which spell out exactly which interactions count as interventions.

Despite the fact that interventionism is closely related to earlier agency theories of causation (e.g., Collingwood 1940, Von Wright 1971, Menzies & Price 1993), it distinguishes itself from these theories by avoiding the concept of agency. This makes it immune to what has generally been seen as the main problem faced by agency theories, namely, that it is not clear how a theory that analyses causation in terms of agency can handle causal relations between events that humans could not possibly cause, such as earthquakes or, even more radically, the Big Bang (Menzies & Price 1993, section 5; Woodward 2003, pp. 123-127; Woodward 2008a, section 3). However, Woodward's own account is beset by a problem of circularity: the analysis of causes is in terms of interventions, and the analysis of interventions is in terms of causes. Woodward recognises this circularity and argues that it is not vicious. However, several writers point out that even if it is not vicious in certain respects, it nevertheless raises tough problems for his theory (Glymour 2004; De Regt 2004; Baumgartner 2009).

The aim of our talk is to show that interventionist theories of causation are best understood as based on a simple agency theory of causation – both from a conceptual and an ontogenetic point of view. Our central argument is that the ability to gain causal knowledge of the world, and in fact even the ability to conceptualise causation, is inextricably bound up with the ability to do things, to perform experiments.

This argument will be presented in two steps. *First*, we demonstrate that the circularity in the interventionist theory is indeed problematic, but that it is not a problem of analysis, but a problem of genesis. That is, we will argue that although Woodward can hold that his theory captures the meaning of causation, the theory nevertheless makes it highly mysterious how we could ever acquire such a concept and start gathering causal knowledge. Since we appear to have causal knowledge, this is problematic. The existence of such a mystery casts doubt upon the analysis itself, especially since other theories of causation – for instance, theories that are based on observed correlations – do not have a problem of genesis.

Second, we present a solution to this problem by showing how the interventionist notion of causation can be rationally generated from amore primitive agency notion of causation. The agency notion is easily and non-circularly applicable, but fails when we attempt to capture causal relations between non-actions. We show that the interventionist notion of causation serves as an appropriate generalisation of the agency notion. Furthermore, the causal judgments based on the latter generally remain true when rephrased in terms of the former, which allows one to use the causal knowledge gained by applying the agency notion as a basis for applying Woodward's interventionist theory.

● **ReimanZaal, C1, Friday, 15:15 – 16:00 h.**

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Computer Simulations: Explanatory versus Ontological MicroReduction
In many natural, social and engineering sciences, reductionistic approaches take center stage. Scientists often aim to understand or to predict a system's macro behavior by modeling its micro constituents. The description of phase transitions, of fluid flows, or agent-based models within economics are just some examples. The *micro*-reductions do not necessarily involve macroscopic or microscopic levels in the literal sense. For instance, global climate models use 'micro' evolution equations on spatial scales as large as 200 km in or- der to predict 'macro' quantities like global mean temperature. This paper aims to formulate a criterion as to when micro reduction models, which are mainly investigated numerically, may work. This criterion is argued to be the separation of scales in the

mathematical formulation.

The increase in computational power and improved numerical methods over that last decades boosted the vogue of micro-reductionistic descriptions in science and engineering. A lot of problems that formerly could only be treated via phenomenological laws on a macro level, can now be described by evolution equations of the micro constituents with the help of computer simulations.

While the philosophy of simulation mostly zoom in on questions as re- gards the epistemic status of numerical data (e.g. Giere 2009, Morrison 2009, Beisbart and Norton 2013), this paper rather focusses on the structure of the micro models that are numerically implemented. The to be addressed question on the explanatory relevance of these models dates back to the early philosophical analysis of simulation models. As early as 1974 M. Smith pointed out that simulation models aim to be “maximally faithful repliquas”. However a map maximally faithful to the landscape it sets out to represent is not of much use. It stands to reason as to whether and how the detailed micro models may enhance our understanding.

In particular, this paper focusses on micro-reductionistic models that aim to predict the evolution of macro quantities. Global circulation models within climatology and models of turbulent flows provide concrete study cases. In these cases, the *ontological* reduction is not under dispute: A gas or fluid is composed of its atoms, the climate systems is made up of its constituents, i.e. atmohydro, bio-, kryo- and litosphere. However it is suggested in this paper to distinguish carefully ontological reduction from *explanatory* reduction in these cases. I argue that the latter requires a separation of the relevant micro scales within the mathematical model that is implemented numerically. The relevant scales may be time, length, or energy. In conclusion, even without claiming emergent features, information on the macro variables via micro-reduction may be impossible to obtain. This questions the use of some current computer modeling within science and engineering.

■ **MennickeZaal, C2, Friday, 16:00 – 16:45 h.**

talk of Bert Leuridan cancelled

● **ReimanZaal, C2, Friday, 16:00 – 16:45 h.**

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Spatial and Temporal Reductionism
My goal in this paper is to determine the existence conditions for space and time. I do so by considering whether it is possible to reduce space and time to relations among material substances and events (respectively), and if so, what these reductions must look like. The reductionist is motivated by the intuitive thought that the postulation of time and space as entities which exist independently of their 'basic individuals' is ontologically otiose. Given this intuitive thought, the reductionist denies not only that space and time, as wholes,

could exist without material substances and events (respectively), but also that there are or could be regions of empty space and intervals of changeless (i.e. eventless) time. The reductionist, therefore, must either be able to show that such regions and intervals really are impossible, or she must be able to ontologically reduce such regions and intervals to relations among substances (for space) and events (for time). In the case of space, I follow Newton-Smith (1980) in arguing that the reductionist can only account for actually existing empty spatial regions by offering a modal reduction, whereby space is reduced to the relations among actual and possible material substances. Furthermore, the modality in question is a physical one and must be specified in terms of a physical geometry. This modal reduction is a kind of halfway position between a fully reductionist account of space and a 'Platonist' account, according to which space is entirely ontologically independent.

In the case of time, however, I part with Newton-Smith and instead endorse the full reduction whereby time can be reduced to relations among actual events only. Newton-Smith and Shoemaker (1969) argue that changeless temporal intervals are possible by appeal to 'fantasy' worlds. These worlds depict scenarios in which their inhabitants would have good (albeit indirect) empirical grounds for positing an interval of changeless time. From this we are meant to conclude that there is no logical inconsistency in changeless time. I argue that though these worlds are strictly logically possible, inasmuch as they are inconsistent with certain metaphysically necessary theses about causation and identity over time, they are not metaphysically possible. And since the question as to whether time can be reduced is a metaphysical question, only metaphysically possible worlds can be employed in arguing for the possibility of changeless time. So time, both as a whole and as temporal intervals, cannot exist without events; whereas in the case of space, though there could be no empty spatial worlds, there can be (and indeed are) empty spatial regions. I also argue that the latter reduction is neutral on the question of substantivalism about space, whereas the temporal reduction rules out substantivalism about times.

The upshot of all this is that space and time cannot simply be different aspects of a fundamentally unified entity, since they admit of different reductions and have different modal status.

■ **MennickeZaal, C2, Friday, 17:00 – 17:45 h.**

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D'Alembert and Euler on the Application of Mathematics and Knowledge Growth

I argue that knowledge resulting from the application of mathematics to nature is assimil- ated in accordance with accepted patterns of knowledge growth. I emphasize that constraints which locally inhibit the assimilation of new phenomena into physico-mathematics belong to broader philosophical views concerning the growth of scientific knowledge. To this purpose, I discuss the mid eighteenth century controversy on the vibrating string between Euler and d'Alembert, and examine their views on knowledge growth and the application of mathematics to nature. I distinguish between two models of knowledge growth: an accumulation model (in d'Alembert's writings) and a falsification model (in Euler's), and show how these relate to the two authors' acceptance of new mathematical knowledge about nature.

Both d'Alembert (1746) and Euler (1748) offer mathematical solutions to the problem of the oscillatory motion of the vibrating string. However, d'Alembert does not accept Euler's more general solution, which also accounts for a larger range of phenomena associated with the sound produced by a vibrating string. D'Alembert writes that "analysis did all it was capable of," and, as a consequence, "physics needs to take care of the rest." D'Alembert favours a 'knowledge by accumulation' model. He recasts the chain of being in epistemological terms to yield the chain uniting all sciences. Because human beings have limited intellectual abilities, we only see continuous, albeit separated parts of the chain, and these represent the different sciences. Plurality of the sciences is thus necessary, for we do not possess the unique principle of the whole of knowledge. Forcing two parts of the chain together, as when applying mathematics to physics indiscriminately, is not the solution to make knowledge grow, for other gaps will widen in other places of the chain; and this inevitably dis- torts our knowledge. Instead, what one is expected to do is work at the ends of each continuous chunk of the chain, adding more and more of the missing links, with the caveat that the chain will never be complete.

In contrast, in Euler's view, knowledge grows by comparing the available alternatives and choosing the best of these; this process is repeated every time a new piece of information is to be taken into consideration as valid. The contrast between the two models of knowledge growth of d'Alembert and Euler is particularly prominent with regard to hypotheses in science: whereas Euler finds them essential for physics, d'Alembert rejects the appeal to hypotheses; the same holds when mathematical abstractions are used as hypotheses about the world.

Because mathematics and physics are two different domains in d'Alembert's chain of knowledge, d'Alembert needs methodological guidelines for the use of techniques from one domain to the other; this is the purpose of what he calls 'the application' of algebra, analysis or geo- metry to physics. New mathematical knowledge about nature needs to be legitimated by this procedure. In Euler's model of knowledge growth the requirement for accepting a new piece of knowledge obtained by mathematical means is to compute without error following the rules of calculus; the only limitations arising in applying mathematics to physics are the result of the mathematical tools themselves. Euler's trust in his algebraic analysis lies in the generality of its symbols and formulas.

● **ReimanZaal, C2, Friday, 17:00 – 17:45 h.**

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Cassirer and Structural Realism

In recent years there has been growing interest in Cassirer's philosophy of science. In part, this is because Cassirer seems to have anticipated certain features of structural realism. For instance, Gower (2000) argues that Cassirer's emphasis on structural continuity across theory change means that he should be read as an ally of Epistemic Structural Realism (EpSR), while French (Cei and French, 2009; forthcoming) argues that elements of Cassirer's structuralism can be stripped free of their Kantian roots and deployed in the service of Ontic Structural Realism (OntSR). The argument of this paper has two stages. *First* I clarify the nature of Cassirer's structuralism and argue that he is more naturally interpreted as anticipating features of OntSR. However, I suggest that it is not as easy to separate Cassirer's structuralism from his idealism as French suggests. *Second*, I argue that Cassirer's structuralism can be interpreted in such a way that it does endorse certain key realist intuitions: i.e. it provides an account of both truth and reference.

Cassirer thought that philosophy of science should begin with a detailed analysis of scientific theories. At the heart of Cassirer's account, then, is an analysis of general relativity and quantum physics: he argues that the epistemologically significant feature of both theories is that they are built upon the "function-theory of concepts". Cassirer is never explicit, though, about how he understands this theory of concepts. I suggest that there are two aspects to the function-theory of concepts: (1) relational concepts are utilised so, e.g., the concept <natural number> is understood relationally rather than as an individual, and (2) the starting point of scientific analysis is the concept <objectivity> rather than the concept <object>.

The idea that the analysis of science should begin with the concept <objectivity> has been taken up by OntSR: furthermore, both OntSR and Cassirer understand that which is objective according to a theory is given by the invariants of the symmetry group of the theory. However there is a crucial difference between the two views. Cassirer contrasts the function-theory of concepts with the substance-theory. The substance-theory is marked by the idea that truth and objectivity are both to be explained in terms of correspondence to a metaphysical world of objects. The function-theory of concepts reverses the order of explanation entirely, so that truth is to be understood in terms of objectivity. I.e., a central tenet of Cassirer's philosophy is an internalist account of truth of the kind that Putnam would come to defend in his (1981).

OntSR, by contrast, takes objectivity to be explained in terms of truth: i.e., the symmetries of a theory (partially) refer to metaphysically real structures. OntSR, then, does not make objectivity fundamental in the same way that Cassirer did. I argue though that, if—like Cassirer—we take objectivity to lay down a criterion for truth we may still construe scientific theories as making claims about existent objects. I suggest, then, that Cassirer's philosophy may still motivate a contemporary (modest) form of scientific realism.

■ **MennickeZaal, C4, 17:45 – 18:30 h.**

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On the Origins and Foundations of Laplacian Determinism
In this paper, I examine the foundations of Laplace's famous statement of determinism in 1814, and argue that this statement depends on Leibnizian metaphysics. In his *Essai philosophique sur les probabilités* (1814), Laplace writes that an intelligence with perfect knowledge of the present state of the universe and perfect calculating capacities can predict future states with certainty. It is usually supposed that Laplace derived this statement from his physics, specifically, the statement is thought to be based on the fact that classical mechanics is deterministic: each system in classical mechanics has an equation of motion which has a unique solution. However, Laplace did not prove this result, and in fact he could not have proven it since it depends on a theorem about

uniqueness of solutions to differential equations that was only developed later on: it was developed by Cauchy in the 1820's and further refined by Lipschitz in 1876. (Furthermore, the theorem left open the possibility of indeterminism in systems that are not ‘Lipschitz-continuous’). I argue that on the basis of his physics, Laplace could not be certain of determinism. However, there were metaphysical principles which supported Laplace's determinism. In fact, the only motivation that Laplace explicitly gives for his determinism is Leibniz' principle of sufficient reason; from this principle it follows that each event must have a cause which immediately precedes it, and from this it follows that the state of the universe at a certain instant is the cause of the state of the universe at the next instant.

Examining the eighteenth century context in which Laplace's determinism first appeared gives a more clear understanding of this argument. In fact, Laplace was far from the first to argue for determinism, and as I show in this paper, he was also far from the first to do so in terms of an intelligence with perfect knowledge and calculating capacities. Particularly relevant is Condorcet's defense of determinism, which is very similar to that of Laplace and in which he makes an appeal to the law of continuity. This law was an important metaphysical principle in eighteenth century physics; it was attributed to Leibniz and in turn thought to be derived from the principle of sufficient reason. According to this law, "a being never passes from one state into another, without passing through all the different states that one can conceive in between them" (D'Alembert). This law ruled out discontinuities in physics, such as sharp angles and discontinuous changes in the direction of motion of bodies (therefore, according to this law, there could be no perfectly hard bodies); and it was only when such discontinuities were ruled out that one could reasonably expect that the equations of physics had unique solutions.

Determinism in physics thus depended on metaphysical principles that originated in Leibniz. This is not to say that Laplace's determinism was unfounded or based on prejudice; one has to be aware that in eighteenth century physics, metaphysical principles played a vital role and there were often legitimate reasons to appeal to such principles.

● **ReimanZaal, C4, Friday, 17:45 – 18:30 h.**

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Romanticism, Alexander von Humboldt and the distinction of ‘Natur’ and ‘Geist’
Alexander von Humboldt is often considered a decisive figure in establishing clear methodological standards in modern natural sciences. But many people forget that he was a true Romanticist in his descriptions of nature as given in his *Ansichten der Natur* (1849) and Cosmos (1866). He contributed to the distinction between *Geist* (spirit) and *Natur* (nature) in contemporary academia as it is understood today in the distinction between *Humanities* and *(Natural) Sciences*. Nevertheless, he called for a close and equal collaboration of these two domains to explain the world– something many experts miss when they think about contemporary academia.

Romanticism is known to be indefinable (Toreinx 1829) as it shares some characteristics with other historical movements. And following Arthur O. Lovejoy’s ideas (1948), there has not been a single movement called ‘Romanticism’ but several ‘Romanticisms’, not only among the different European countries, but also within those countries. However, there are some features that can be considered as genuinely romantic. Many of them are related to the observation of landscape and, simultaneously, the reference to transcendental entities. Scientific observation follows the strict path of objective observation, and this seems to be the way Humboldt chose for analyzing nature. *Ansichten der Natur* is well considered as a scientific work that provides the reader with detailed natural descriptions as well as an attempt to explain nature's phenomena as a whole.

However, there are some easily recognizable features in the text that can remind the reader of the way romantic poets used to describe their feelings when observing some kind of landscapes or natural phenomena. Humboldt could be, therefore, considered as the bridge that links Romanticism and Naturalism as he, on the one hand, analyzes nature with a scientific eye but on the other hand describes the effect that these elements of nature or landscape cause in him. This kind of gentle gesture to the importance of metaphysics is what makes it possible to consider Humboldt as a 'hybrid' author. The romantic defense of human feelings and the pursuit of measuring the world from a scientific point of view show the existence of one great historical dichotomy between *science* and *literature*.

This dichotomy remains in the distinction of (natural) sciences and humanities in contemporary academia. Today, there seems to be an outright conflict between natural scientists and those academics who are engaged in history, art, literature or even philosophy– with a high degree of willful ignorance on both sides. In many cases, a dialogue becomes almost impossible. Even though Humboldt's ideas were partly responsible for this dichotomy, the clash was not at all what Humboldt intended 200 years ago. On the contrary, his idea was to describe nature as a whole.

Humboldt has not only established many scientific standards, he also should be seen as an embodiment of the dialogue between the humanistic and the scientific way of measuring the world – a true universal genius from whom we can still benefit as we will point out in our presentation.

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■ **MennickeZaal, C5, Friday, 18:30 – 19:15 h.**

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A Kantian A Priori in Experimental Science

Over the second half of the preceding century, logical positivism has been largely replaced by the model- theoretic view on scientific theories. A scientific experiment is, on this view, a generator of data which form themselves a structure that can be embedded in a larger algebra (the theory itself), a model within the model, so to say, in contrast to the older, syntactic view where a theory is a closed axiomatic system based on the difference between conceptual and observational predicates connected by a principle of correspondence, which guarantees that the theory is speaking about something in the world 'out there'. We want to show that both approaches — apart from their eventual shortcomings on other grounds — have problems in dealing with the relation between 'theory' and 'world', because they do not take into account the very special kind of observational practice which scientific experimentation actually is.

We believe both approaches run into problems because they overlook an essential feature of experimentation per se: it is not just *observation*, it is a *procedure*, an *intervention*, which remodels — literally — the part of the world it is designed to investigate. The aim of an experiment is to generate a finite and discrete number of quantities, interpretable as predicable characteristics, the "properties" of the "system". The data structure that results from this intervention does not just describe the behaviour of the system; it rather describes the logic of actions open to the experimenter through the experimental procedure on that specific part of the world. The crucial point is that the experimental set up itself is always classical, whatever the nature of the underlying system, and whatever the logical structure of the model theory. This is completely comprehensible when we look at what an experiment is from the historical point of view.

Seventeenth century natural philosophy prided itself for having cast arbitrary *ontologies* out of rigorously consistent scientific discourse. By developing an observational practice which imposes the same rigorous consistency upon our experience of the world, the old metaphysical correspondence between the world and what we can say about it could be saved at least in principle in the new conceptual setting. The a priori logical frame shoring it up also warrants the universal validity of its results. As long as our theories deal with relatively simple and fairly nearby parts of the world, this approach works very well, but at extremely large or small scales, or in situations where the behaviour of the world 'out there' does not fit into our classical framework, it becomes clear that the mental a priori materialised in the design of our experiments distorts and transforms our observations substantially. By taking the 'logicalising' effect of the experimental procedure explicitly into account, it might not only be possible to deal with certain shortcomings of our present accounts of scientific experimentation, but also to clarify the relation between 'science' and the 'real world' from a point of view different from the received ones.

● **ReimanZaal, C5, Friday, 18:30 – 19:15 h.**

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Varieties of Misrepresentation
The activity of modelling primarily aims at adequately representing certain real-world targets. Unfortunately, not every scientific model fulfils this task successfully. To some extent models are misrepresentations. Thus, the phenomenon of misrepresentation is regarded as one crucial point in any theory of scientific representation. In general, one expects from every successful account of representation to say something illuminating

about this topic. In the sciences, there seem to be different forms of misrepresentation. Firstly, I want to ask what are these types of misrepresentation? And secondly, do all these types really form representations in a strong sense?

Models of the ether were definitely scientific models. Yet, it is debateable whether they really represent. If one wants to regard them as misrepresentations, they exemplify one extreme form, namely they are lacking any referent at all. And because of that one could argue that they do not represent. There are other misrepresentations, which apparently do represent while they do so only in a defective way. Toy models that are used to represent in a very sketchy way are examples at hand. Models incorporating idealizations and abstractions are at the other end of the spectrum. Are such models misrepresentations after all? Some scholars argue that most models incorporate quantitative inaccuracies. Should one evaluate models as misrepresentations only because they contain these inaccuracies? The Newtonian model of the movement of the earth around the sun is such a representation, which is not perfectly accurate. This model lacks the corrections from general relativity; still one can argue that it is accurate enough in order to adequately represent the earth's motion. So, not all misrepresentations seem to be on a par. Some models are lacking any real-world target; others get most of the story right while still other models completely disagree with the data gained by experiments.

At least there are three kinds of misrepresentation. The less harmful ones are the slightly distorted models. Their deviations are within an acceptable margin of error. So even they are not precise according to a high standard of accuracy, they are accurate enough according to a lower standard. Models being similar to their targets but only to a lesser degree constitute the second type of misrepresentation. They are defective in a way that they refer to their corresponding targets while the content of the representation is not in order. In this sense they are representations because the representational force of the model points to the respective target. Because they misrepresent their targets, the information they convey is inferior compared to an adequate representation. Models without a real-world target constitute the third type. They are clearly misrepresentations. Whether they are still representations at all is questionable. In the fine arts there is a related phenomenon. Suppose a person resembles a portrait painting shown in an exhibition. Because of this resemblance a visitor of the exhibition thinks wrongly that the painting represents this person. This is a misrepresentation, which is clearly not a representation at all. Maybe this "mistargeting" is a fourth type of misrepresentation. A further task is to show whether and how this type is exemplified in the sciences.

Parallel Session D (Saturday Morning 25 May)

■ **MennickeZaal, D1, Saturday, 09:00 – 09:45 h.**

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Systems in Technology: Instrumental System as a sui generis Kind of System
The term 'system' is routinely used in science and engineering – e.g. 'solar system', 'ecosystem', 'global positioning system' (GPS) – to indicate any complex whole which consists of smaller components related by physical interactions some of which are weak in the sense that if you were to 'pick up' one such component, the other components would not automatically be lifted up as well. Whether this notion of system extends to the social sciences is questionable, since interactions between subsystems are invariably of a physically weak form, and accordingly talk of systems in social science – *e.g.* 'economic system', 'modern world-system' (as used by Wallerstein) – is inevitably vaguer and less accepted. To what extent we would even be dealing with equivalent notions of system in these two realms is similarly unclear, since it is controversial, to say the least, whether the interactions between the candidate components of social systems can be described as causal in the first place.

I argue that, however one specifies notions of system that are useful for the natural and the social sciences, they fail to capture a type of complex entity that is of tremendous importance for human life. Such complexes are constituted by the intentional human involvement with material objects. They span a huge realm, from the purposeful use and modification-for-use of physical objects by single individuals to the participation of many individuals in various roles – user, operator, manager, owner, regulator – in large-scale, complex infrastructures. The human use, at some level of complexity, of material objects has both an intentional dimension – the use is a free act undertaken for a purpose – and a causal dimension – the purpose is achieved, if all goes well, by a causal chain set in motion by the user and continued in the material object. The material object used is often of the kind that natural scientists or engineers would refer to it as a system, as is the case, for instance, with the global positioning system, or with any infrastructure. All the more reason to emphasize that two quite different types of systems are at work here.
I give a philosophical argument why these large-scale material systems – systems in the scientific sense – cannot exhaust the notion of system in technology. This argument derives from the central role played in technology by the notion of function. Although there is no single account of function that is accepted as being able to deal with all uses of the notion of function, the theory that engineers accept as coming closest to how the term is used in engineering and technology is the causal theory introduced by Cummins in 1975. The notion of function explicated by this theory is also referred to as *system function*: the function of any object is explicated as the causal contribution it delivers to the behaviour of a larger system of which it is a component. In order to account for the function of artefacts used *hands-on*, such as hammers, toasters, and so forth, a notion of system is required of which such artefacts are components. The notion of system as used in natural or social science is not up to this because a clear specification of the involvement of both intentional, purposeful action and material objects to be acted upon is required, and neither the notion of a physical (material, causal) system nor that of a social (intentional, mental) system nor an aggregate of these two types of systems can achieve this.

To provide for the required type of system, I introduce the notion of an *instrumental system*. I will briefly sketch how I propose to analyse instrumental systems and how that analysis can be put to work. Any instrumental system is a structured complex consisting of three different constituents, performing the roles of user, instrument and object, are intentionally arranged in such a way as to transform a particular kind of input into a particular kind of output in a particular way. By introducing the possibility that any of these components can itself be analysed as a complex involving either a subset or the totality of the basic roles performed in an instrumental system, a conceptually sparse framework becomes available for analysing a large part of, if not the full complexity of instrumental systems encountered in modern technological society.

● **ReimanZaal, D1, Saturday, 09:00 – 09:45 h.**

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Fifty Years of Semantic View: An Assessment of its Success
In this paper I argue in favor of the following claims as reasons for the success of the semantic view of scientific theories: the semantic view
(i) provides a realistic answer to the right question: what is a scientific theory?
(ii) provides such an answer in the correct manner, i.e., remaining epistemologically neutral;
(iii) acknowledges scientific practice as crucial for dealing with (i) and (ii).

Notwithstanding the wide literature on the different formulations of the semantic view and on its potential consistency with either realist or anti-realist stances, a systematic analysis both of its significance and of the reasons for its orthodoxy status is yet to be provided. The aim of this paper is to provide such an analysis and, in order to do that, I mean to deploy Van Fraassen's and Shapiro's insights concerning philosophy of science. Van Fraassen claims that as the task of any "philosophy of *X*" is to make sense of *X*, so philosophy of science is an attempt to make sense of science and, elliptically, of scientific theories (1980, p. 663). This task is carried out by tackling two questions.

The *first* question concerns what a theory is *per se*. This is the question par excellence for the philosophy of science insofar as answering it is preliminary to, and independent of, tackling issues concerning the epistemic attitude to be endorsed towards the content of a theory. These issues fall under the *second* question, which concerns theories as objects for epistemic attitudes. Van Fraassen's remarks can be consistently integrated with Shapiro's view on how a philosophical analysis should be carried out. Shapiro advocates the necessity for any "philosophy of *X*" not to be "isolated from the practice of *X*" (1983, p. 525). Reducing explanation to a mere description of a target system does not suffice to justify in virtue of what the abstract description relates to the object described. Without such a justification it is in fact impossible to account for the explanatory success of theory. Only referring to the practice of theory construction allows to account for how science contributes to knowledge.

The semantic view evidently deals with the foundational question of philosophy of science. As the syntactic view did, the semantic view aims at providing a picture of scientific theories. However, unlike the syntactic view, the semantic view succeeds in providing a realistic picture of theories. The syntactic view has been driven in its formulation by the (anti-realist) Positivistic Credo, according to which a programmatic goal for the analysis of theories is to provide only a *rational reconstruction* of the latter, i.e., a reconstruction which omits the scientists' actions and focuses only on their result (i.e., theories. See Carnap, 1955, p. 42). The semantic view, on the other hand, preserving its neutrality with respect to any preexistent school of thought, whether realist or anti-realist, succeeds in providing a realistic image of scientific theories, which is obtained by focusing on "how science really works" (Suppe, 2000, p. 114).

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■ MennickeZaal, D2, Saturday, 09:45 – 10:30 h.

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Representation and Science Learning. An Ontic-Structural Realist Approach

The aim of my talk is to provide an ontic structural interpretation of science learning. Ontic structural realism (OntSR) is intended in my talk as the variety of structural realism that argues that the ontology of successful scientific theories is an *ontology of relations*.

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

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

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

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

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

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

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● **ReimanZaal, D2, Saturday, 09:45 – 10:30 h.**

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Dimensions of Internal Representations. The Instructive Case of the Neural Representation of Numbers
While it is consensually acknowledged that the features of external representations do play a role in isomorphic problem-solving situations (Holyoak and Bower 1985; Holyoak and Thagard 1989), arithmetical calculus has been the theatre of a quarrel between an externalist problem solving tradition (Becker and Varelas 1993; Gonzalez and Kolers 1982; Zhang and Norman 1995) which maintained:

> [...] that during numerical processing people do not transform different external number representations into a common abstract internal representation [but] operate upon different internal representations that reflect the physical characteristics of different

external representations" (Zhang and Wang 2005:832)

and an internalist neuro-based approach (Dehaene 1992; Dehaene et al. 1999; Piazza *et al.* 2002), which stated:

> [...] that the human brain converts numbers internally from the symbolic format to a continuous, quantity-based analogical format" (Dehaene, Dehaene-Lambertz, and Cohen 1998:358).

Our intervention will be dually goaled. *First*, from an historical point of view, we will suggest that the incommensurable opposition between these paradigms does not have much to do with the antagonism of their respective fields but lies on a tacit assumption; namely, the idea that the dominant sub-symbolic abstract model of the *neural activities* related with mathematics involve a merely sub-symbolic *neural representations* of numbers. In a nutshell, by rejecting the classical symbolic approach in modelling, authors as Verguts and Fias 2004 also rejected every theory based on mental manipulation of symbols as Gonzalez and Kolers 1982'. Such a conflation of the *product* with its underling *process* appears one the reason why the classical neuro-based approach could not account for complex influences of surfaces on numerical tasks such as, for instance, discontinuity effect on response times in two digits numbers comparison – i,e, sharp changes in response times across the boundaries (e,g,39, 41). Supported by both current research in neuro-science (notation dependent neural representations in Cohen Kadosh et al. 2007) and problem-solving (¨Ollinger, Jones, and Knoblich 2008), we will then advocate for this distinction in considering the algorithms responsible for daily calculus in different number systems such as the Roman (*CCCLXII*), the Greek ($\tau\ \xi\ \beta$) or the Mayan (*Mayan symbol*).

Then, in an epistemological perspective, we will suggest that such a conflation is not the only one in cognitive sciences for similar confusions between the *representatum* – i,e, what is represented – and its neural *representans* also occurred in the debate opposing symbolists with connectionists or in many philosophical quarterlies. We will thus use the paradigmatic case of neural representations of numbers to suggest that the kind of properties the *representans* must share with the *representatum* allow both externalized representations and memories but also some direct use of the physical and spatial external features by internal representations.

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■ **MennickeZaal, D3, Saturday, 10:30 – 11:15 h.**

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Reduction and Emergence in Physics: A Dynamical Systems Approach

In this paper, I seek to advertise and develop an approach to inter-theory relations in physics that has received relatively little attention in the philosophical literature on reduction and emergence, but that I believe goes a long way toward clarifying the nature of a wide range of inter-theory relations both within and beyond physics. This approach, which I call *Dynamical Systems* (DS) *reduction*, is grounded in the general framework of dynamical systems theory and is intended to apply to reductions between theories whose models can be formulated within this framework, as is the case with most existing theories in physics (since most of these theories can be formulated in terms of the evolution of a point in some mathematical state space). The DS approach is built around a natural condition for the reduction of dynamical systems that has been suggested independently in a number of places (for instance, by Giunti, and both in discussion and in lectures by Wallace). In a separate paper, I demonstrate how this approach to reduction should be applied in the case of certain quantum-classical reductions.

I begin by considering the two existing approaches to reduction in physics that have attracted the most attention in the literature on this subject - namely, Nagelian reduction and limit-based approaches. I provide a preliminary critique of the limit-based approach in this paper, deferring a more comprehensive critique to a separate paper. I also consider some of the more common critiques of Nagelian approaches, and of a particular refinement of Nagel's account dubbed the Generalised Nagel Schaffner model in a recent paper by Dizadji-Bahmani *et al*. (GNS).

After setting out the central elements of dynamical systems (DS) reduction, I argue that while Nagel's approach was formulated against the background of logical empiricism and the syntactic view of theories, it nevertheless offers a number of crucial insights that extend beyond this context, and in particular that can be carried over naturally to a realist, semantic understanding of the theories involved in the reduction. I emphasise a number of deep structural parallels between the DS approach to reduction the GNS approach, arguing that dynamical systems reduction successfully applies many of the core insights of Nagelian reduction to the context of reductions involving dynamical systems. Chief among the parallels that I emphasise between DS and Nagelian reduction is that both approaches incorporate bridging assumptions - what Nagelians often refer to as `bridge laws' - to connect the high- and low- level descriptions involved in a given reduction.

I then argue that, while DS reduction incorporates many basic insights of Nagelian reduction, in the cases where DS reduction applies, it successfully confronts or avoids a number of well-known concerns relating to the use of bridge laws in Nagelian reduction, including concerns relating to the logical and metaphysical status of bridge laws and to multiple realisation. I also argue that DS reduction succeeds in addressing a number of concerns relating not to Nagelian reduction generally, but to the GNS account specifically.

● **ReimanZaal, D3, Saturday, 10:30 – 11:15 h.**

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Could Popper have been a Bayesian? On the Falsification of Statistical Hypotheses
Karl R. Popper was a fervent opponent of Carnap's logical probability approach, and more generally speaking, any inductivist logic of inference. On the other hand, Popper clearly recognized the need to come up with a "degree of corroboration": a quantitative measure of how well a theory has stood up to test in practice.

In the context of statistical reasoning, this implies the need of having a methodological rule when a hypothesis can be regarded as falsified, and when (and to which degree) it is corroborated. However, concrete proposals for such a rule (e.g., Gillies 1971) were met with devastating criticism (e.g., Spielman 1974). So the problem of applying falsificationism to statistical hypothesis testing persists.

In this contribution, I explore whether an instrumental Bayesian approach can help to solve this problem. For instance, for Gelman and Shalizi (2013), the testing of complex statistical models combines a hypothetico-deductive methodology (that Popper acknowledged as a *qualitative* characterization of corroboration) with the technical tools of Bayesian statistics.

There are also other Bayesian proposals, like Bernardo's (1999) decision- theoretic approach to hypothesis testing, that display a surprising similarity to Popper's principal ideas. This suggests that Popper's views on corroboration can be fruitfully combined with modern techniques for hypothesis testing in Bayesian statistics.

The paper contends that such hybrid views on statistical inference may resolve the tension between a Popperian approach to the testing of scientific hypotheses, and the use of subjective Bayesian probability. This refutes the widespread prejudice that Popper's philosophy of science must be aligned with a frequentist (e.g., error-statistical) account of statistical inference. Moreover, I will show that the compatibility thesis can be supported by some central sections of Popper's 1934/59 monograph.

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